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A Theory of Structure: Duality, Agency, and Transformation——Sewell
Middle-Class Mobility in England, 1548–1689——Bearman and Deane
Payment Systems and the Structure of Inequality——Petersen
Growth Effects of Foreign and Domestic Investment——Firebaugh
Gender, Parenthood, and Job-Family Compatibility——Glass and Camarigg
The Sociological Concept of "Group" and Two Models——Freeman

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IN THIS ISSUE

William H. Sewell, Jr., is professor of political science and history at the University of Chicago. He is the author of *Work and Revolution in France: The Language of Labor from the Old Regime to 1848* and *Structure and Mobility: The Men and Women of Marseille, 1820–1870.* In addition to working on theories of social transformation, he is writing a book on the Abbé Sieyes and the political rhetoric of the French Revolution.

Peter S. Bearman is assistant professor of sociology at the University of North Carolina—Chapel Hill. He is currently working with Glenn Deane on mobility models for younger sons. He is the author of *Relations into Rhetorics* (Rose Monograph Series, Rutgers University Press) and is completing a book on localism and mobilization in the U.S. Civil War.

Glenn Deane is a lecturer in the Department of Sociology at the State University of New York at Albany. He is currently completing an investigation of the influence of inheritance on wealth inequality and demographic behavior in the colonial South. His current research also includes the study of racial differences among the determinants of adolescent cigarette smokers (with Sandra Headen and Karl Bauman) and birth-order effects on status persistence in early modern England (with Peter Bearman and Michael McKenna).

Trond Petersen is an associate professor in the Walter A. Haas School of Business, an affiliated faculty member of the Department of Sociology, and a research associate at the Institute of Industrial Relations, all at the University of California, Berkeley. His research is on reward systems in formal organizations and on quantitative methods of sociological research. He recently published, with Seymour Spilerman, "Job Quits from an Internal Labor Market" (in *Event History Analysis in Life Course Research* [University of Wisconsin Press, 1990]) and edited a special issue of *Sociological Methods and Research* (vol. 19, no. 3 [February 1991]).

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Editor's Note

This inaugural issue of volume 98 signals my formal tenure as editor of the *American Journal of Sociology.* Actually, I have been filling that role since August 1991, but I felt that the last three issues of volume 97 should appear under Bill Parish's name, since it was through his hard work with the editorial board that the articles in those numbers were selected. Now I am very excited to introduce the first issue constructed of works selected under my editorship.

It is both a privilege and a supreme challenge to adjudicate among the substantive range of high quality articles submitted annually to the *AJS.* I expect to maintain the high standards of my predecessors while representing the theoretical, substantive, and methodological breadth and depth of our science. In assuming this responsibility, I have set several goals for myself. My major goals are, first, to increase the presence of the *AJS* in the international community of sociologists and, second, to widen the appeal of the *AJS* to the social sciences generally. In addition, I hope to avoid long delays in the time required to adjudicate and publish manuscripts.

My first goal can be achieved by encouraging the submission of more articles from scholars residing outside the United States. I also hope to engage the efforts of international scholars in reviewing manuscripts for the *Journal.* Fortunately, technology is on our side and has greatly aided in this endeavor by allowing for the efficient electronic transmission of reviews. The second goal will be advanced by encouraging scholars from other social science disciplines to read the *AJS* and to think critically about the intersection of disciplinary perspectives on substantive and theoretical issues. That is, my goal is to encourage authors to use the theoretical prisms of other disciplines to generate alternative insights into sociological phenomena. The first three issues of volume 98 make an initial contribution toward these goals, and we currently have under consideration several manuscripts from foreign scholars and from social science disciplines other than sociology.

My third goal—that of reaching decisions within three months of receiving a manuscript—depends greatly on reviewer responsiveness. We are now sending all submissions to three reviewers. While this has expedited the review process in many cases, we occasionally experience difficulty in obtaining reviews in a timely manner. Whether this is a growing problem I cannot say, but with the help of the staff, I am monitoring the situation. In addition to our "readers at large," we rely heavily on our consulting editors for evaluations of manuscripts. These consulting editors are not paid; they are invited to serve for a period of two years during which they agree to evaluate an average of two manuscripts a month (we are not usually so cruel as to send more). Their input is essential in maintaining the quality of offerings published in the *AJS*. I would be very pleased to have readers make recommendations for consulting editors.

The past months have made me aware of two issues that I would like to share with both readers and prospective authors. First, the majority of the articles that appear in the *AJS* have been revised at least once; some of them have even been revised two or three times. The invitation to pursue a revision is no guarantee that the paper will eventually be published. Our policy is to send revised papers to one or more of the previous reviewers and to at least one new reviewer as well. As a matter of policy, the board does not request a second revision unless we are reasonably confident that the paper is very likely to succeed. But again, a second round of revisions is not a guarantee that we will publish the paper.

Another, more disturbing issue concerns the matter of submitting material that either has already been published or is being considered for publication elsewhere. Over the past few months several reviewers have brought to our attention manuscripts that either had appeared or were about to appear in different venues. While I cannot yet judge how pervasive this problem is, I suspect that pressure to publish may contribute to the phenomenon of producing narrowly focused papers that represent incremental variations on a theme. The temptation to produce this kind of work may be rather aggravated by the ease with which electronic means allow us to rearrange and revise our materials. Whatever the causes of this difficult problem, I wish to stress that the *American Journal of Sociology* publishes original work that has not appeared, in whole or in part, in any other publication. The editorial board will be glad to work with individual authors who feel uncertain about the implementation of this policy; if you are not confident that your piece meets this standard, ask for our judgment at the outset of the review process.

Finally, I am pleased to point out that the *American Journal of Sociology* will celebrate its centennial in 1995. Although I do not plan, in general, to organize "thematic" issues, the occasion of the centennial certainly justifies an exception. I welcome ideas for organizing a centennial volume, particularly if they include not just our first century, but our second as well.

MARTA TIENDA

A Theory of Structure: Duality, Agency, and Transformation[1]

William H. Sewell, Jr.
University of Chicago

"Structure" is one of the most important, elusive, and undertheorized concepts in the social sciences. Setting out from a critique and reformulation of Anthony Giddens's notion of the duality of structure and Pierre Bourdieu's notion of habitus, this article attempts to develop a theory of structure that restores human agency to social actors, builds the possibility of change into the concept of structure, and overcomes the divide between semiotic and materialist visions of structure.

"Structure" is one of the most important and most elusive terms in the vocabulary of current social science. The concept is central not only in such eponymous schools as structural functionalism, structuralism, and poststructuralism, but in virtually all tendencies of social scientific thought. But if social scientists find it impossible to do without the term "structure," we also find it nearly impossible to define it adequately. Many of us have surely had the experience of being asked by a "naive" student what we mean by structure, and then finding it embarrassingly difficult to define the term without using the word "structure" or one of its variants in its own definition. Sometimes we find what seems to be an acceptable synonym—for example, "pattern"—but all such synonyms lack the original's rhetorical force. When it comes to indicating

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[1] This article has benefited, during its many revisions, from the careful reading and constructive criticism of a large number of friends and colleagues. Although I have sometimes failed to heed their good advice, I am deeply grateful to Elizabeth Anderson, Jeffrey Alexander, Ronald Aminzade, Renee Anspach, Terry Boswell, Peggy Evans, Neil Fligstein, Steven Gudeman, Ronald Herring, Ronald Inden, David Laitin, Barbara Laslett, Michael Kennedy, Sherry Ortner, Sylvia Pedraza, Joan Scott, Ellen Sewell, Theda Skocpol, Ann Swidler, John Urry, Loïc Wacquant, several reviewers, and the audiences of seminars and colloquia at the University of Minnesota, the University of Michigan, Harvard University, Northwestern University, the University of Chicago, and the Center for Advanced Study in the Behavioral Sciences (CASBS). This article was revised extensively while I was a fellow at the CASBS. I am grateful for support provided by the National Science Foundation, grant BNS-870064, and by a fellowship from the John Simon Guggenheim Memorial Foundation.

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that a relation is powerful or important it is certainly more convincing to designate it as "structural" than as "patterning."

The term structure empowers what it designates. Structure, in its nominative sense, always implies structure in its transitive verbal sense. Whatever aspect of social life we designate as structure is posited as "structuring" some other aspect of social existence—whether it is class that structures politics, gender that structures employment opportunities, rhetorical conventions that structure texts or utterances, or modes of production that structure social formations. Structure operates in social scientific discourse as a powerful metonymic device, identifying some part of a complex social reality as explaining the whole. It is a word to conjure with in the social sciences. In fact, structure is less a precise concept than a kind of founding or epistemic metaphor of social scientific—and scientific—discourse.[2] For this reason, no formal definition can succeed in fixing the term's meaning: the metaphor of structure continues its essential if somewhat mysterious work in the constitution of social scientific knowledge despite theorists' definitional efforts.

There are, nevertheless, three problems in the current use of the term that make self-conscious theorizing about the meanings of structure seem worthwhile. The most fundamental problem is that structural or structuralist arguments tend to assume a far too rigid causal determinism in social life. Those features of social existence denominated as structures tend to be reified and treated as primary, hard, and immutable, like the girders of a building, while the events or social processes they structure tend to be seen as secondary and superficial, like the outer "skin" of a skyscraper, or as mutable within "hard" structural constraints, like the layout of offices on floors defined by a skeleton of girders. What tends to get lost in the language of structure is the efficacy of human action—or "agency," to use the currently favored term. Structures tend to appear in social scientific discourse as impervious to human agency, to exist apart from, but nevertheless to determine the essential shape of, the strivings and motivated transactions that constitute the experienced surface of social life. A social science trapped in an unexamined metaphor of structure tends to reduce actors to cleverly programmed automatons. A second and closely related problem with the notion of structure is that it makes dealing with change awkward. The metaphor of structure implies stability. For this reason, structural language lends itself readily to explanations of how social life is shaped into consistent patterns, but not to explanations of how these patterns change over time. In structural discourse, change is commonly located outside of structures, either in a telos of history, in notions of breakdown, or in influences exogenous to the system in question. Consequently, moving from questions of stability to questions of change tends to involve awkward epistemological shifts.

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[2] The term "structure" seems to play an essentially identical role in the natural sciences. Such usages originated, as far as I am aware, in 17th- and 18th-century botany, from which they spread to other natural and social sciences (see Foucault 1973, pp. 132–38).

The third problem is of a rather different order: the term structure is used in apparently contradictory senses in different social scientific discourses, particularly in sociology and anthropology. Sociologists typically contrast "structure" to "culture." Structure, in normal sociological usage, is thought of as "hard" or "material" and therefore as primary and determining, whereas culture is regarded as "soft" or "mental" and therefore as secondary or derived. By contrast, semiotically inclined social scientists, most particularly anthropologists, regard culture as the preeminent site of structure. In typical anthropological usage, the term structure is assumed to refer to the realm of culture, except when it is modified by the adjective "social." As a consequence, social scientists as different in outlook as Theda Skocpol and Marshall Sahlins can be designated as "structuralists" by their respective disciplines. Sociologists and anthropologists, in short, tend to visualize the nature and location of structure in sharply discrepant, indeed mutually incompatible, ways.[3]

In view of all these problems with the notion of structure, it is tempting to conclude that the term should simply be discarded. But this, I think, is impossible: structure is so rhetorically powerful and pervasive a term that any attempt to legislate its abolition would be futile. Moreover, the notion of structure does denominate, however problematically, something very important about social relations: the tendency of patterns of relations to be reproduced, even when actors engaging in the relations are unaware of the patterns or do not desire their reproduction. In my opinion, the notion of structure neither could nor should be banished from the discourse of social science. But it does need extensive rethinking. This article will attempt to develop a theory of structure that overcomes the three cardinal weaknesses of the concept as it is normally employed in social science. The theory will attempt (1) to recognize the agency of social actors, (2) to build the possibility of change into the concept of structure, and (3) to overcome the divide between semiotic and materialist visions of structure. My strategy will be to begin from what I regard as the most promising existing formulations—Anthony Giddens's notion of "the duality of structure" and, at a later point in the argument, Pierre Bourdieu's concept of habitus—and to develop a more adequate theory by means of critique, reformulation, and elaboration.[4]

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[3] This bifurcation of the meaning of structure especially inhibits communication between two groups of social scientists whose current projects seem convergent but who have thus far paid little attention to one another: the growing band of sociologists who are examining the cultural dimensions of social life and the anthropologists who are insisting on the importance of power and practice in understanding culture. For an assessment of the mushrooming field of cultural sociology, see Lamont and Wuthnow (1990). For trends in current anthropology, see the remarks of Ortner (1984, pp. 144–60).

THE DUALITY OF STRUCTURE: A CRITIQUE AND REFORMULATION OF GIDDENS'S THEORY

The most sustained effort at reconceptualizing structure in recent social theory has been made by Anthony Giddens, who has been insisting since the mid-1970s that structures must be regarded as "dual" (Giddens 1976, 1979, 1981, 1984). By this he means that they are "both the medium and the outcome of the practices which constitute social systems" (Giddens 1981, p. 27). Structures shape people's practices, but it is also people's practices that constitute (and reproduce) structures. In this view of things, human agency and structure, far from being *opposed,* in fact *presuppose* each other. Structures are enacted by what Giddens calls "knowledgeable" human agents (i.e., people who know what they are doing and how to do it), and agents act by putting into practice their necessarily structured knowledge. Hence, "structures must not be conceptualized as simply placing constraints on human agency, but as enabling" (Giddens 1976, p. 161). This conception of human agents as "knowledgeable" and "enabled" implies that those agents are capable of putting their structurally formed capacities to work in creative or innovative ways. And, if enough people or even a few people who are powerful enough act in innovative ways, their action may have the consequence of transforming the very structures that gave them the capacity to act. Dual structures therefore are potentially mutable. It is no accident that Giddens calls his theory "the theory of structuration," indicating by this neologism that "structure" must be regarded as a process, not as a steady state.

As a theoretically self-conscious social historian, I find Giddens's notion of the duality of structure particularly congenial. Much of the best social history of the past quarter-century has adopted an implicit theoretical strategy quite consistent with Giddens's theory. Social historians have significantly altered in practice the sociological and anthropological concepts of structure that they began to borrow so avidly in the 1960s and 1970s. Although they were probably writing more from professional instinct than from considered theoretical scruples, social historians have demonstrated how, in a great variety of times and places, structures are in fact dual: how historical agents' thoughts, motives, and intentions are constituted by the cultures and social institutions into which they are born, how these cultures and institutions are reproduced by the structurally shaped and constrained actions of those agents, but also how, in certain circumstances, the agents can (or are forced to) improvise or innovate in structurally shaped ways that significantly reconfigure the very structures that constituted them. Giddens has arrived at his position by way of a theoretical critique intended to reconcile phenomenology, interactionism, and ethnomethodology with Marx, Durkheim, and Weber; he has shown little interest in the work of social historians. Yet I believe that Giddens's notion of the duality of structure underwrites theoretically what social historians (and in recent years many historical sociologists and historical anthropologists as well) do in practice.

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[4] It is not my purpose to develop a full critique or appreciation of Giddens or Bourdieu. The critical literature on both is growing rapidly. Held and Thompson (1989) and Bryant and Jary (1991) include not only a wide range of critiques of Giddens's work by prominent scholars but also useful bibliographical listings of previous critiques. On Bourdieu, see DiMaggio (1979), Brubaker (1985), Lamont and Lareau (1988), and Wacquant (1989). The last of these contains extensive references to critical works on Bourdieu.

What Is Structure?

But in spite of its promise, Giddens's theory suffers from serious gaps and logical deficiencies that have persisted through the theory's all-too-frequent restatements (for the major statements see Giddens [1976, 1979, 1984]). Most strikingly, "structure"—the central term of Giddens's theory—remains frustratingly underspecified. Unlike most social scientists, he does not leave the term completely undefined and simply allow it to do its accustomed magical work in his readers' minds. Especially in *Central Problems in Social Theory* (1979), he discusses "structure" at some length. But I do not think that the concept of structure he elaborates there or elsewhere is sufficiently clear or robust to serve as the foundation of a theoretical system.

Giddens defines structure formally in several places, including in the glossary to *The Constitution of Society:*

> *Structure.* Rules and resources, recursively implicated in the reproduction of social systems. Structure exists only as memory traces, the organic basis of human knowledgeability, and as instantiated in action. [1984, p. 377]

This far-from-crystalline definition requires some exegesis. The terms "rules and resources," in spite of their deceptive simplicity, are quite obscure and will have to be discussed at length. Let us therefore begin with the rest of the definition, which is arcanely worded but relatively straightforward in meaning. By "social systems" Giddens means empirically observable, intertwining, and relatively bounded social practices that link persons across time and space. Social systems would encompass what most social scientists mean by "societies" but would also include social units greater (e.g., the capitalist world system) or more limited (e.g., the neighborhood community) in scope than the nation-state. Social systems, according to Giddens, have no existence apart from the practices that constitute them, and these practices are reproduced by the "recursive" (i.e., repeated) enactments of structures. Structures are not the patterned social practices that make up social systems, but the *principles* that pattern these practices. Structures, therefore, have only what he elsewhere terms a "virtual" existence (e.g., 1984, p. 17). Structures do not exist concretely in time and space except as "memory traces, the organic basis of knowledgeability" (i.e., only as ideas or schemas lodged in human brains) and as they are "instantiated in action" (i.e., put into practice).

Structures as Rules

Structures, then, are "virtual" and are put into practice in the production and reproduction of social life. But of what do these structures consist? According to Giddens's definition, they consist of "rules and resources." Giddens's notion of rules is largely derived from French structuralism. This is especially clear in *New Rules of Sociological Method* and *Central Problems of Social Theory*. In both of these he relies heavily on a typically structuralist analogy with Saussurian linguistics. Giddens likens his own distinction between structure and practice to the Saussurian distinction between *langue* and *parole*. According to this analogy, structure is to practice as *langue* (the abstract rules that make possible the production of grammatical sentences) is to *parole* (speech, or the production of actual sentences; 1976, pp. 118–22). Hence structure, like *langue,* is a complex of rules with a "virtual" existence, while practice, like speech, is an enactment of these rules in space and time. For a French structuralist, structure is the complex of such rules. For Lévi-Strauss, for example, structure refers to the set of rules that enables binary oppositions to be ordered into myths (Lévi-Strauss 1963). In *Central Problems in Social Theory* (1979, pp. 62–64), Giddens affirms the similarity of his concept of structure to that of Lévi-Strauss. But he also attempts to distinguish himself from the French structuralists, in part by insisting that, because structures "bind" time and space, they must be conceptualized as including not only rules but *resources* as well (1979, pp. 63–64). However, Giddens leaves his discussion of rules dangling, and he fails to give examples of rules that underlie any actual social practices. All we know from *Central Problems in Social Theory* is that rules are virtual and that they somehow generate social practices and social systems.

In *The Constitution of Society,* the most recent statement of his theory, Giddens retreats even farther from a Lévi-Straussian conception of rules. Now taking his cue from Wittgenstein, Giddens there defines rules simply but, in my opinion, with great promise: "Let us regard the rules of social life . . . as generalizable procedures applied in the enactment/reproduction of social life" (1984, p. 21). This definition of rules as generalizable procedures could of course include Lévi-Straussian transformation rules, but it also implies the possibility of rules of a wide range of types. Giddens, however, does not give examples or develop typologies of the sorts of generalizable procedures he has in mind. Consequently, his conception of rules is, if anything, more impoverished in *The Constitution of Society* than it was in *Central Problems in Social Theory,* which at least implied an analogy with Lévi-Strauss. However, I think his Wittgensteinian definition of rules as generalizable procedures can be used as a foundation for a more robust conception.

Throughout his theory, Giddens places a great deal of weight on the notion that actors are *knowledgeable*. It is, presumably, the knowledge of rules that makes people capable of action. But Giddens develops no vocabulary for specifying the *content* of what people know. I would argue that such a vocabulary is, in fact, readily available, but is best developed in a field Giddens has to date almost entirely ignored: cultural anthropology. After all, the usual social scientific term for "what people know" is "culture," and those who have most fruitfully theorized and studied culture are the anthropologists. Claude Lévi-Strauss, the one anthropologist Giddens has taken seriously, is virtually unique in his fixation on very deep or general structures. His attempt, ultimately, is to reach by successive abstractions the structure of the human brain itself. Even some of the structuralist anthropologists who have been most profoundly influenced by Lévi-Strauss (see, e.g., Sahlins 1976, 1981, 1985) have been far more interested in applying Lévi-Strauss's method of seeking out recurrent patterns of binary oppositions in order to specify the assumptions, practices, and beliefs of particular peoples than in tracing such oppositions back to the structure of "the savage mind" or the human brain.

Rather than staying at the deep structural level preferred by Lévi-Strauss, I think we should, like most anthropologists, think of rules as existing at various levels. Rules nearer the surface may by definition be more "superficial," but they are not necessarily less important in their implications for social life. "The rules of social life" should be thought of as including all the varieties of cultural schemas that anthropologists have uncovered in their research: not only the array of binary oppositions that make up a given society's fundamental tools of thought, but also the various conventions, recipes, scenarios, principles of action, and habits of speech and gesture built up with these fundamental tools.[5] Indeed, the term "rules" is probably not quite the right word, since it tends to imply something like formally stated prescriptions—the sorts of things spelled out in statutes, proverbs, liturgies, constitutions, or contracts.[6] What I mean to get at is not formally stated prescriptions but the informal and not always conscious schemas, metaphors, or assumptions presupposed by such formal statements. I would in fact argue that publicly fixed codifications of rules are actual rather than virtual and should be regarded as *resources* rather than as rules in Giddens's sense. Because of this ambiguity about the meaning of the word "rules," I believe it is useful to introduce a change in terminology. Henceforth I shall use the term "schemas" rather than "rules"—even though this destroys the pleasing alliteration of Giddens's "rules and resources" formula.

The various schemas that make up structures are, to quote Giddens, "generalizable procedures applied in the enactment/reproduction of social life." They are "generalizable" in the sense that they can be applied in or extended to a variety of contexts of interaction. Such schemas or procedures—whether rules of etiquette, or aesthetic norms, or such recipes for group action as the royal progress, grain riot, or democratic vote, or a set of equivalences between wet and dry, female and male, nature and culture, private and public, or the body as a metaphor for hierarchy, or the notion that the human being is composed of a body and a soul—can be used not only in the situation in which they are first learned or most conventionally applied. They can be generalized—that is, transposed or extended—to new situations when the opportunity arises. This *generalizability* or *transposability* of schemas is the reason they must be understood as virtual. To say that schemas are virtual is to say that they cannot be reduced to their existence in any particular practice or any particular location in space and time: they can be actualized in a potentially broad and unpredetermined range of situations.

I agree with Giddens, then, that the rules or schemas making up structures may usefully be conceptualized as having a "virtual" existence, that structures consist of intersubjectively available procedures or schemas capable of being actualized or put into practice in a range of different circumstances. Such schemas should be thought of as operating at widely varying levels of depth, from Lévi-Straussian deep structures to relatively superficial rules of etiquette.

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[5] It is not possible here to list a representative example of anthropological works that elaborate various "rules of social life." The most influential formulation of the anthropological concept of culture is probably Geertz (1973). For a superb review of recent developments in cultural anthropology, see Ortner (1984).

[6] For a particularly convincing critique of the notion of "rule," see Bourdieu (1977, pp. 1–29).

Structures as Resources

Surely part of Giddens's nervousness about embracing Lévi-Strauss's conception of structure is that he wishes to distance himself from Lévi-Strauss's sublime indifference to questions of power, domination, and social change—indeed, to questions of social practice more generally. Presumably it is largely for this reason that Giddens insists that structures are not merely rules, but rules and resources, or "rule-resource sets" (1984, p. 377). But Giddens's concept of resources is even less adequately theorized than his concept of rules.[7] I agree with Giddens that any notion of structure that ignores asymmetries of power is radically incomplete. But tacking an undertheorized notion of resources onto an essentially rule-based notion of structure succeeds merely in confusing things.

In *Central Problems in Social Theory,* Giddens (1979, p. 92) defines resources as "the media whereby transformative capacity is employed as power in the routine course of social interaction." Unless I am missing some subtlety, this obscurely worded definition could be rendered in ordinary English as "resources are anything that can serve as a source of power in social interactions." This seems to me an unexceptional and theoretically uninformative statement of what we usually mean by social resources. Besides this anodyne definition, almost all he tells us about resources is that they can be classified into two types, authoritative and allocative. In *Central Problems in Social Theory,* he defines "authorization" as those "capabilities which generate command over persons" and "allocation" as those "capabilities which generate command over *objects* or other material phenomena" (1979, p. 100). By extension, authoritative resources should be human resources and allocative resources nonhuman resources—which once again seems unexceptional.

I believe that Giddens's classification of resources is potentially useful, but that it needs to be reformulated and put into ordinary English. Resources are of two types, human and nonhuman. Nonhuman resources are objects, animate or inanimate, naturally occurring or manufactured, that can be used to enhance or maintain power; human resources are physical strength, dexterity, knowledge, and emotional commitments that can be used to enhance or maintain power, including knowledge of the means of gaining, retaining, controlling, and propagating either human or nonhuman resources. Both types of resources are media of power and are unevenly distributed. But however unequally resources may be distributed, some measure of both human and nonhuman resources are controlled by all members of society, no matter how destitute and oppressed. Indeed, part of what it means to conceive of human beings as *agents* is to conceive of them as *empowered* by access to resources of one kind or another.

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[7] Giddens's concept of rules has occasionally been criticized, most recently by Thompson (1989), but to my knowledge no one has systematically criticized his paired concept of resources.

Structures as Schemas and Resources

Reformulating Giddens's concept of resources does not make it clear how resources and schemas combine to form structures. Here the most glaring problem is Giddens's definition of structures as "virtual." As we have seen, this makes perfect sense for structures conceptualized as rules or schemas. But are *resources* also virtual? It is surprising that Giddens does not seem to have considered the point. The notion of a virtual resource seems particularly doubtful in the case of nonhuman (or in Giddens's terms "allocative") resources. Nonhuman resources would surely include such things as factories owned by capitalists, stocks of weapons controlled by kings or generals, land rented by peasants, or stacks of Hudson Bay blankets accumulated by Kwakiutl chiefs. It is clear that factories, armaments, land, and Hudson Bay blankets have had a crucial weight in shaping and constraining social life in particular times and places, and it therefore seems sensible to include them in some way in a concept of structure. But it is also hard to see how such material resources can be considered as "virtual," since material things by definition exist in space and time. It is, moreover, only in particular times, places, and quantities that such material objects can serve as resources.

The case of human resources is only a little less clear. By definition, human bodies, like any other material objects, cannot be virtual. But what about knowledge and emotional commitments, the mental aspects of human resources? Examples might be the Roman Catholic priest's power to consecrate the host and hear confession, children's sense of obligation toward their mothers, or the fear and reverence that subjects feel for their king. Unlike factories or Hudson Bay blankets, such resources are not material, or at least not in the same sense. Nevertheless they seem to me actual as opposed to virtual. They exist in what Giddens calls "time-space"; they are observable characteristics of real people who live in particular times and congregate in particular places. And it is their actualization in people's minds and bodies that make them resources. It is not the disembodied concept of the majesty of the king that gives him power, but the fear and reverence felt for him by his actual subjects.

If I am right that all resources are actual rather than virtual, Giddens's notion of structure turns out to be self-contradictory. If structures are virtual, they cannot include both schemas and resources. And if they include both schemas and resources, they cannot be virtual. He, and we, cannot have it both ways. But which way should we have it? The simplest way of conceptualizing structure would be to return to Giddens's starting point in structuralism and to assert that structure refers only to rules or schemas, not to resources, and that resources should be thought of as an *effect* of structures. In this way, structures would retain their virtual quality, and concrete distributions of resources would be seen not as structures but as media animated and shaped by structures, that is, by cultural schemas.

It is not unreasonable to claim that human resources are the products of schemas. A given number of soldiers will generate different amounts and kinds of military power depending on the contemporary conventions of warfare (such as chivalric codes), the notions of strategy and tactics available to the generals, and the regimes of training to which the troops have been subjected. The priest's power to consecrate the host derives from schemas operating at two rather different levels. First, a priest's training has given him mastery of a wide range of explicit and implicit techniques of knowledge and self-control that enable him to perform satisfactorily as a priest. And second, he has been raised to the dignity of the priesthood by an ordination ceremony that, through the laying on of hands by a bishop, has mobilized the power of apostolic succession and thereby made him capable of an apparently miraculous feat—transforming bread and wine into the body and blood of Christ. Fear and reverence for kings are manifestations of fundamental notions about the cosmic function of kingship, notions that are woven into a multitude of discourses and ceremonies at all levels of society; similarly, obligations felt by children toward their mothers are based in notions of the bonds of nature, of nurturance, and of obedience that are encoded in multiple routines of family life and in sermons, adages, novels, and works of political theory. Human resources, these examples suggest, may be thought of as manifestations and consequences of the enactment of cultural schemas.

But while we might reasonably speak of human resources as generated by rules or schemas, it is harder to see how nonhuman resources could be conceived of as so generated. Factories, land, and Hudson Bay blankets have material qualities that are certainly not generated by schemas. But it is also true that their condition as resources capable of producing and reproducing disparities in social power is not wholly intrinsic in their material existence. What they amount to as resources is largely a consequence of the schemas that inform their use. To take perhaps the most obvious case, an immense stack of Hudson Bay blankets would be nothing more than a means of keeping a large number of people warm were it not for the cultural schemas that constituted the Kwakiutl potlatch; but given these schemas, the blankets, given away in a potlatch, became a means of demonstrating the power of the chief and, consequently, of acquiring prestige, marriage alliances, military power, and labor services (Boas 1966; Sahlins 1989). In this case, the schemas constituting the potlatch determined the specific value, extent, and effects of Hudson Bay blankets as a resource. But I would argue that this is true of nonhuman resources in general. For example, the extent and kinds of resources generated by a factory will depend on whether it is owned by an individual capitalist or by a workers' cooperative—in other words, on rules defining the nature of property rights and of workplace authority. The resources gained by peasants from the land they use will be determined by the conventions of land tenure, the exigencies of customary law, the sets of obligations owed to kinsmen, and the agricultural techniques employed. Examples could be multiplied at will. Nonhuman resources have a material existence that is not reducible to rules or schemas, but the activation of material things as resources, the determination of their value and social power, is dependent on the cultural schemas that inform their social use.

It is clear, then, that resources can plausibly be thought of as effects of cultural schemas. It therefore would certainly be possible to clean up Giddens's concept of structure by defining structure as schemas with a purely virtual existence, and resources not as coequal elements in structure but as media and outcomes of the operation of structure. But notice that if we adopt this definition, the rhetorical power of the term structure insinuates a single direction of causality. That which is termed structure is, by this act of denomination, granted power over that which is not termed structure. Stocks of material goods and people's knowledge and emotional commitments become inert, mere media for and outcomes of the determinative operations of cultural schemas. If we insist that structures are virtual, we risk lapsing into the de facto idealism that continually haunts structuralism however much its exponents—for example, Lévi-Strauss (1966, p. 130)—protest their materialist credentials and intentions. Schemas—mental structures—become the only form-giving entity, and agents become agents of these mental structures, actors who can only recite preexisting scripts. To define structures in this way threatens, in short, to deny their duality and, consequently, to annihilate the central premise of Giddens's theory.

The Duality of Schemas and Resources

If the duality of structure is to be saved—and as far as I am concerned the notion of duality of structure is the main attraction of Giddens's theory—we must take the other alternative and conceive of structures as having (appropriately) a *dual* character. Structure, then, should be defined as composed simultaneously of schemas, which are virtual, and of resources, which are actual.

If structures are dual in this sense, then it must be true that schemas are the effects of resources, just as resources are the effects of schemas. This seems to me a reasonable claim, one whose plausibility can be demonstrated by a few examples. A factory is not an inert pile of bricks, wood, and metal. It incorporates or actualizes schemas, and this means that the schemas can be inferred from the material form of the factory. The factory gate, the punching-in station, the design of the assembly line: all of these features of the factory teach and validate the rules of the capitalist labor contract. Or take the priest's performance of the Mass. When the priest transforms the host and wine into the body and blood of Christ and administers the host to communicants, the communicants are suffused by a sense of spiritual well-being. Communion therefore demonstrates to the communicants the reality and power of the rule of apostolic succession that made the priest a priest. In short, if resources are instantiations or embodiments of schemas, they therefore inculcate and justify the schemas as well. Resources, we might say, are *read* like texts, to recover the cultural schemas they instantiate. Indeed, texts—whether novels, or statute books, or folktales, or contracts—are resources from the point of view of this theory. They, too, are instantiations of schemas in time-space that can be used by actors to generate power.

If resources are effects of schemas, it is also true that schemas are effects of resources. If schemas are to be sustained or reproduced over time—and without sustained reproduction they could hardly be counted as structural—they must be validated by the accumulation of resources that their enactment engenders. Schemas not empowered or regenerated by resources would eventually be abandoned and forgotten, just as resources without cultural schemas to direct their use would eventually dissipate and decay. Sets of schemas and resources may properly be said to constitute *structures* only when they mutually imply and sustain each other over time.

THE TRANSFORMATION OF DUAL STRUCTURES: OUT OF BOURDIEU'S HABITUS

A definition of structure as made up of both schemas and resources avoids both the material determinism of traditional Marxism and the ideal determinism of traditional French structuralism. But how it can enhance our ability to understand transformations of structures is not immediately apparent. Indeed, one could argue that if the enactment of schemas always creates resources that inculcate the schemas, schemas and resources should simply reproduce each other without change indefinitely. The claim that dual structures engender stasis is far from fanciful; such an argument has in fact been made with great panache in Pierre Bourdieu's (1977) widely influential discussion of what he calls "habitus" in *Outline of a Theory of Practice*. Any attempt to argue that duality of structure improves our ability to understand social transformations must confront this argument.[8]

Duality and Stasis

Although he uses a different terminology, Bourdieu has powerfully illustrated the mutually sustaining relationship between schemas and resources (what he calls "mental structures" and "the world of objects"). For example, his well-known discussion (Bourdieu 1977) of the Kabyle house shows how the design of the house and the placement of objects in it reproduces fundamental Kabyle cultural oppositions, such as those between high and low, male and female, fire and water, and light and dark, and thereby patterns all activities conducted in the house in terms of such oppositions. Bourdieu remarks that "all the actions performed in a space constructed in this way are immediately qualified symbolically and function as so many structural exercises through which is built up practical mastery of the fundamental schemes" (Bourdieu 1977, p. 91).

The house is given its shape by the application of schemas ("mental structures" in Bourdieu's vocabulary), and the house in turn inculcates these schemas by assigning tasks, objects, persons, and emotional dispositions to differently coded spaces. As Bourdieu puts it, in his characteristically ornate and paradoxical style,

> The mental structures which construct the world of objects are constructed in the practice of a world of objects constructed according to the same structures. The mind born of the world of objects does not rise as a subjectivity confronting an objectivity: the objective universe is made up of ob-

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[8] Some of Bourdieu's more recent work, esp. *Homo Academicus* (1988), which is a study of the French professoriat in the events of 1968, deals more directly with change. I do not think, however, that Bourdieu has considered the question of how habitus itself might *generate* change. In *Homo Academicus,* e.g., change arises from sources external to the habitus he is analyzing—fundamentally from the immense rise in the population of students in French universities in the 1960s. The concept of habitus is used to argue that the professors' responses to the crisis was wholly determined by their location in the "academic field." *Homo Academicus* seems to indicate that Bourdieu has not overcome the lack of agency inherent in the concept of habitus elaborated in *Outline of a Theory of Practice.*

> jects which are the product of objectifying operations structured according to the very structures which the mind applies to it. The mind is a metaphor of the world of objects which is itself but an endless circle of mutually reflecting metaphors. [Bourdieu 1977, p. 91]

In many respects, Bourdieu's "theory of practice" is fully compatible with the conception of the duality of structure for which I am arguing in this paper. Bourdieu recognizes the mutual reproduction of schemas and resources that constitutes temporally durable structures—which he calls "habitus." His discussion of habitus powerfully elaborates the means by which mutually reinforcing rule-resource sets constitute human subjects with particular sorts of knowledge and dispositions. Moreover, Bourdieu's Kabyle subjects are not cultural dopes. They are endowed with the capacity to engage in highly autonomous, discerning, and strategic actions. (See, e.g., Bourdieu's discussion of gift exchange and matrimonial strategies [1977, pp. 4–10 and 32–53, respectively].) Bourdieu's Kabyles would seem to be exactly the sort of knowledgeable actors called for by Giddens's theory.

Yet Bourdieu's habitus retains precisely the agent-proof quality that the concept of the duality of structure is supposed to overcome. In Bourdieu's habitus, schemas and resources so powerfully reproduce one another that even the most cunning or improvisational actions undertaken by agents necessarily reproduce the structure. "As an acquired system of generative schemes objectively adjusted to the particular conditions in which it is constituted, the habitus engenders all the thoughts, all the perceptions, and all the actions consistent with those conditions and no others" (Bourdieu 1977, p. 95). Although Bourdieu avoids either a traditional French structuralist ideal determinism or a traditional Marxist material determinism, he does so only by erecting a combined determinism that makes significant social transformations seem impossible.

But is this powerful implication of stasis really warranted? After all, the Kabyle society in which Bourdieu carried out his fieldwork produced a momentous anticolonial revolution shortly after Bourdieu returned to France to analyze his data. It seems to me that, in spite of his devastating attacks on Cartesian and Lévi-Straussian "objectivism" (Bourdieu 1977, esp. pp. 1–30), Bourdieu's own theory has fallen victim to an impossibly objectified and overtotalized conception of society. Only in the idealized world constructed by the social scientific observer could habitus engender "all the thoughts, all the perceptions, and all the actions" consistent with existing social conditions "and no others." In the world of human struggles and strategems, plenty of thoughts, perceptions, and actions consistent with the reproduction of existing social patterns fail to occur, and inconsistent ones occur all the time.

Why Structural Change Is Possible

It is, of course, entirely proper for Bourdieu to insist on the strong reproductive bias built into structures—that is the whole point of the structure concept and part of what makes the concept so essential for theorizing social change. After all, as Renato Rosaldo (1980) and Marshall Sahlins (1981, 1985) have brilliantly demonstrated, the same reproductive biases of structures that explain the powerful continuities of social relations also make it possible to explain the paths followed in episodes of social change. What gets Bourdieu off the track is his unrealistically unified and totalized concept of habitus, which he conceptualizes as a vast series of strictly homologous structures encompassing all of social experience. Such a conceptualization, which Bourdieu in fact shares roughly with many structurally inclined theorists, cannot explain change as arising from within the operation of structures. It is characteristic that many structural accounts of social transformation tend to introduce change from outside the system and then trace out the ensuing structurally shaped changes, rather than showing how change is generated by the operation of structures internal to a society. In this respect, Marshall Sahlins's (1981) analysis of how Captain Cook's voyages affected the Hawaiians is emblematic. It is my conviction that a theory of change cannot be built into a theory of structure unless we adopt a far more multiple, contingent, and fractured conception of society—and of structure. What is needed is a conceptual vocabulary that makes it possible to show how the ordinary operations of structures can generate transformations. To this end, I propose five key axioms: the multiplicity of structures, the transposability of schemas, the unpredictability of resource accumulation, the polysemy of resources, and the intersection of structures.

The multiplicity of structures.—Societies are based on practices that derive from many distinct structures, which exist at different levels, operate in different modalities, and are themselves based on widely varying types and quantities of resources. While it is common for a certain range of these structures to be homologous, like those described by Bourdieu in *Outline of a Theory of Practice,* it is never true that all of them are homologous. Structures tend to vary significantly between different institutional spheres, so that kinship structures will have different logics and dynamics than those possessed by religious structures, productive structures, aesthetic structures, educational structures, and so on. There is, moreover, important variation even within a given sphere. For example, the structures that shape and constrain religion in Christian societies include authoritarian, prophetic, ritual, and theoretical modes. These may sometimes operate in harmony, but they can also lead to sharply conflicting claims and empowerments. The multiplicity of structures means that the knowledgeble social actors whose practices constitute a society are far more versatile than Bourdieu's account of a universally homologous habitus would imply: social actors are capable of applying a wide range of different and even incompatible schemas and have access to heterogeneous arrays of resources.

The transposability of schemas.—Moroever, the schemas to which actors have access can be applied across a wide range of circumstances. This is actually recognized by Bourdieu, but he has not, in my opinion, drawn the correct conclusions from his insight. Schemas were defined above as generalizable or transposable procedures applied in the enactment of social life. The term "generalizable" is taken from Giddens; the term "transposable," which I prefer, is taken from Bourdieu.[9] At one point Bourdieu defines habitus as "a system of lasting transposable dispositions which, integrating past experiences, functions at every moment as a *matrix of perceptions, appreciations, and actions* and makes possible the achievement of infinitely diversified tasks, thanks to analogical transfers of schemes permitting the solution of similarly shaped problems" (1977, p. 83; emphasis in original).

The slippage in this passage occurs in the final phrase, "permitting the solution of similarly shaped problems." Whether a given problem is similarly shaped enough to be solved by analogical transfers of schemes cannot be decided in advance by social scientific analysts, but must be determined case by case by the actors, which means that there is no fixed limit to the possible transpositions. This is in fact implied by the earlier phrase, "makes possible the achievement of infinitely diversified tasks." To say that schemas are transposable, in other words, is to say that they can be applied to a wide and not fully predictable range of cases outside the context in which they are initially learned. This fits with what we usually mean by knowledge of a rule or of some other learned procedure. In ordinary speech one cannot be said to really *know* a rule simply because one can apply it mechanically to repeated instances of the same case. Whether we are speaking of rules of grammar, mathematics, law, etiquette, or carpentry, the real test of knowing a rule is to be able to apply it successfully in *unfamiliar* cases. Knowledge of a rule or a schema by definition means the ability to transpose or extend it—that is, to apply it creatively. If this is so, then *agency,* which I would define as entailing the capacity to transpose and extend schemas to new contexts, is inherent in the knowledge of cultural schemas that characterizes all minimally competent members of society.[10]

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[9] To generalize a rule implies stating it in more abstract form so that it will apply to a larger number of cases. The verb "transpose" implies a concrete application of a rule to a new case, but in such a way that the rule will have subtly different forms in each of its applications. This is implied by three of the *Oxford English Dictionary's* (1971, s.v. "transpose") definitions: "To remove from one place or time to another; to transfer, shift," "to alter the order of or the position of in a series . . . to interchange," and, in music, "to put into a different key." *Transposer,* in French (which was of course the language in which Bourdieu wrote), also has an even more appropriate meaning: "faire changer de forme ou de contenu en faisant passer dans un autre domaine," (to cause something to change in form or content by causing it to pass into another domain, *Le Petit Robert* [1984, s.v. "transposer"]). I would like my use of *transpose* to be understood as retaining something of this French meaning.

The unpredictability of resource accumulation.—But the very fact that schemas are by definition capable of being transposed or extended means that the resource consequences of the enactment of cultural schemas is never entirely predictable. A joke told to a new audience, an investment made in a new market, an offer of marriage made to a new patriline, a cavalry attack made on a new terrain, a crop planted in a newly cleared field or in a familiar field in a new spring—the effect of these actions on the resources of the actors is never quite certain. Investment in a new market may make the entrepreneur a pauper or a millionaire, negotiation of a marriage with a new patriline may result in a family's elevation in status or its extinction in a feud, planting a crop in the familiar field may result in subsistence, starvation, or plenty. Moreover, if the enactment of schemas creates unpredictable quantities and qualities of resources, and if the reproduction of schemas depends on their continuing validation by resources, this implies that schemas will in fact be differentially validated when they are put into action and therefore will potentially be subject to modification. A brilliantly successful cavalry attack on a new terrain may change the battle plans of subsequent campaigns or even theories of military tactics; a joke that draws rotten tomatoes rather than laughter may result in the suppression of a category of jokes from the comedian's repertoire; a succession of crop failures may modify routines of planting or plowing.[11]

The polysemy of resources.—The term polysemy (or multiplicity of meaning) is normally applied to symbols, language, or texts. Its application to resources sounds like a contradiction in terms. But, given the concept of resources I am advocating here, it is not. Resources, I have insisted, embody cultural schemas. Like texts or ritual performances, however, their meaning is never entirely unambiguous. The form of the factory embodies and therefore teaches capitalist notions of property relations. But, as Marx points out, it can also teach the necessarily social and collective character of production and thereby undermine the capitalist notion of private property. The new prestige, wealth, and territory gained from the brilliant success of a cavalry charge may be attributed to the superior discipline and élan of the cavalry officers and thereby enhance the power of an aristocratic officer corps, or it may be attributed to the commanding general and thereby result in the increasing subordination of officers to a charismatic leader. Any array of resources is capable of being interpreted in varying ways and, therefore, of empowering different actors and teaching different schemas. Again, this seems to me inherent in a definition of agency as the capacity to transpose and extend schemas to new contexts. Agency, to put it differently, is the actor's capacity to reinterpret and mobilize an array of resources in terms of cultural schemas other than those that initially constituted the array.

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[10] Here my thinking has been influenced by Goran Therborn (1980, esp. pp. 15–22).

[11] Although Marshall Sahlins (1981, 1985) does not explicitly include resources in his definition of structure, my argument here runs closely parallel to his. Sahlins argues that "in action in the world—technically, in acts of reference—the cultural categories acquire new functional values" because the categories are "burdened with the world" (1985, p. 138). This burdening of categories with the world is a matter of schemas being changed by the unanticipated effects of action on the resources that sustain the schemas.

The intersection of structures.—One reason arrays of resources can be interpreted in more than one way is that structures or structural complexes intersect and overlap. The structures of capitalist society include both a mode of production based on private property and profit and a mode of labor organization based on workplace solidarity. The factory figures as a crucial resource in both of these structures, and its meaning and consequences for both workers and managers is therefore open and contested. The intersection of structures, in fact, takes place in both the schema and the resource dimensions. Not only can a given array of resources be claimed by different actors embedded in different structural complexes (or differentially claimed by the same actor embedded in different structural complexes), but schemas can be borrowed or appropriated from one structural complex and applied to another. Not only do workers and factory owners struggle for control of the factory, but Marx appropriates political economy for the advancement of socialism.

Structures, then, are sets of mutually sustaining schemas and resources that empower and constrain social action and that tend to be reproduced by that social action. But their reproduction is never automatic. Structures are at risk, at least to some extent, in all of the social encounters they shape—because structures are multiple and intersecting, because schemas are transposable, and because resources are polysemic and accumulate unpredictably. Placing the relationship between resources and cultural schemas at the center of a concept of structure makes it possible to show how social change, no less than social stasis, can be generated by the enactment of structures in social life.

Agency

Such enactments of structures imply a particular concept of agency—one that sees agency not as opposed to, but as constituent of, structure. To be an agent means to be capable of exerting some degree of control over the social relations in which one is enmeshed, which in turn implies the ability to transform those social relations to some degree. As I see it, agents are empowered to act with and against others by structures: they have knowledge of the schemas that inform social life and have access to some measure of human and nonhuman resources. Agency arises from the actor's knowledge of schemas, which means the ability to apply them to new contexts. Or, to put the same thing the other way around, agency arises from the actor's control of resources, which means the capacity to reinterpret or mobilize an array of resources in terms of schemas other than those that constituted the array. Agency is implied by the existence of structures.

I would argue that a capacity for agency—for desiring, for forming intentions, and for acting creatively—is inherent in all humans. But I would also argue that humans are born with only a highly generalized capacity for agency, analogous to their capacity to use language. Just as linguistic capacity takes the form of becoming a competent speaker of some particular language—French, or Arabic, or Swahili, or Urdu—agency is formed by a specific range of cultural schemas and resources available in a person's particular social milieu. The specific forms that agency will take consequently vary enormously and are culturally and historically determined. But a capacity for agency is as much a given for humans as the capacity for respiration.

That all humans actually exercise agency in practice is demonstrated to my satisfaction by the work of Erving Goffman (1959, 1967). Goffman shows that all members of society employ complex repertoires of interaction skills to control and sustain ongoing social relations. He also shows that small transformative actions—for example, intervening to save the face of an interactant who has misread the situation—turn out to be necessary to sustain even the most ordinary intercourse of daily life (Goffman 1967, pp. 5–46). Once again, knowledge of cultural schemas (in this case of interaction rituals) implies the ability to act creatively. Actors, of course, vary in the extent of their control of social relations and in the scope of their transformative powers, but all members of society exercise some measure of agency in the conduct of their daily lives.

It is equally important, however, to insist that the agency exercised by different persons is far from uniform, that agency differs enormously in both kind and extent. What kinds of desires people can have, what intentions they can form, and what sorts of creative transpositions they can carry out vary dramatically from one social world to another depending on the nature of the particular structures that inform those social worlds. Without a notion of heaven and hell a person cannot strive for admission into paradise; only in a modern capitalist economy can one attempt to make a killing on the futures market; if they are denied access to the public sphere, women's ambitions will be focused on private life. Agency also differs in extent, both between and within societies. Occupancy of different social positions—as defined, for example, by gender, wealth, social prestige, class, ethnicity, occupation, generation, sexual preference, or education—gives people knowledge of different schemas and access to different kinds and amounts of resources and hence different possibilities for transformative action. And the scope or extent of agency also varies enormously between different social systems, even for occupants of analogous positions. The owner of the biggest art gallery in St. Louis has far less influence on American artistic taste than the owner of the biggest gallery in Los Angeles; the president of Chad has far less power over global environmental policy than the president of Russia. Structures, in short, empower agents differentially, which also implies that they embody the desires, intentions, and knowledge of agents differentially as well. Structures, and the human agencies they endow, are laden with differences in power.

Finally, I would insist that agency is collective as well as individual. I do not agree with Barry Hindess (1986) that the term "agent" must be applied in the same sense to collectivities that act as corporate units in social life—political parties, firms, families, states, clubs, or trade unions—as it is applied to individuals. But I do see agency as profoundly social or collective. The transpositions of schemas and remobilizations of resources that constitute agency are always acts of communication with others. Agency entails an ability to coordinate one's actions with others and against others, to form collective projects, to persuade, to coerce, and to monitor the simultaneous effects of one's own and others' activities. Moreover, the extent of the agency exercised by individual persons depends profoundly on their positions in collective organizations. To take the extreme case, a monarch's personal whims or quarrels may affect the lives of thousands (see, e.g., Sahlins 1991). But it is also true that the agency of fathers, executives, or professors is greatly expanded by the places they occupy in patriarchal families, corporations, or universities and by their consequent authority to bind the collectivity by their actions. Agency, then, characterizes all persons. But the agency exercised by persons is collective in both its sources and its mode of exercise. Personal agency is, therefore, laden with collectively produced differences of power and implicated in collective struggles and resistances.

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VARIETIES OF STRUCTURES

The concept of structure I elaborate in this article is very general and therefore could be applied to structures of widely differing character—ranging in import from structures that shape and constrain the development of world military power to those that shape and constrain the joking practices of a group of Sunday fishing buddies or the erotic practices of a single couple. This immense range in the scope and character of the structures to which this article's concepts can be applied is appropriate, given the premise that all social action is shaped by structures. But it suggests a need for some means of distinguishing the character and dynamics of different sorts of structures. I will offer no detailed typology—both because space is short and because I feel that typologies should arise out of concrete analyses of social change and reproduction. Instead, I shall simply indicate two important dimensions along which structures vary: depth, which refers to the schema dimension of structure, and power, which refers to the resource dimension. I shall try to demonstrate that thinking in terms of depth and power can help to illuminate the very different dynamics and durabilities of three important types of structures: those of language, states, and capitalism.

Depth has long been a key metaphor of linguistic and structuralist discourse. To designate a structure as "deep" implies that it lies beneath and generates a certain range of "surface" structures, just as structures underlie and generate practices. In structuralist discourse, deep structures are those schemas that can be shown to underlie ordinary or "surface" structures, in the sense that the surface structures are a set of transformations of the deep structures. Thus the structural schemas for the performance of a fertility ritual may be shown to be particular transformations of a deeper set of oppositions between wet and dry or male and female that also underlie structures informing other institutionally distinct practices—from housebuilding, to personal adornment, to oratory. Consequently, deep structural schemas are also pervasive, in the sense that they are present in a relatively wide range of institutional spheres, practices, and discourses. They also tend to be relatively unconscious, in the sense that they are taken-for-granted mental assumptions or modes of procedure that actors normally apply without being aware that they are applying them.

Different structures also vary enormously in the resources, and hence the power, that they mobilize. Military structures or structures shaping state finance create massive concentrations of power, whereas the grammatical structures of a language or the structures shaping schoolchildren's play create much more modest power concentrations. Structures also differ in the kinds of power they mobilize. For example, the power created by apostolic succession is based primarily (although far from exclusively) on persuasion, while that created by the military government of a conquering army is based primarily on coercion.

Language.—I believe that thinking about structures in terms of their depth and power can lead to insights about the structures' durability and dynamics. Consider, for example, linguistic structures, which scholars in many disciplines have used as the prime example of structure in general. Linguistic structures, which of course tend to be remarkably durable, actually fall at extremes on the dimensions of both power and depth. Linguistic structures are unusually deep. Intricate phonological, morphological, syntactical, and semantic structures underlie every sentence. Sentences, in turn, are aggregated into meaningful utterances or texts in accord with the discursive structures of rhetoric, narrative, metaphor, and logic. And all of these layered linguistic structures underlie the multitude of structures that rely at least in part on speech and writing—which is to say the immense preponderance of all structures.

Yet the *power* of linguistic structures is unusually slight. The enactment of phonological, morphological, syntactical, and semantic structures in speech or writing in itself has relatively modest resource effects. It confirms the speaker's membership in a linguistic community and reinforces the schemas that make the generation of grammatical sentences possible. Assuming that an utterance is made to other competent speakers of the language, the speaking of a grammatical sentence in itself creates no significant power disparities but rather establishes an equality among the conversants. Language, of course, serves as a medium for all kinds of enactments of power relations, but at the level of phonology, morphology, syntax, and semantics, it is as close as we are likely to get to a neutral medium of exchange. This relative neutrality with respect to power helps to account for the other peculiarity of linguistic structures: their extraordinary durability. If the enactment of linguistic schemas serves only to sustain the linguistic empowerment of speakers without sharply shifting resources toward some speakers and away from others, then no one has much incentive to engage in innovations that would transform linguistic structures.

If it is true that linguistic structures are much less implicated in power relations and much deeper and more durable than most structures, it follows that we should be wary of the widespread tendency to use linguistic structures as a paradigm for structures in general. Although the elegance of the linguistic model may set an enviable standard, structures that operate nearer the surface of social life and that are more directly implicated in power relations may have very different principles and dynamics. One danger that arises from accepting the linguistic model uncritically is a tendency to think of structures as composed purely of schemas, while ignoring the resource dimension. In studying the syntactic structures of languages, where the enactment of schemas has minor power consequences, it does not matter much if the resource aspect of structure is neglected. But when we try to make sense of the arenas of life more permeated by power relations, it may be downright crippling to apply the linguistic analogy and conceptualize structures purely as schemas.

States.—Particularly poor candidates for the linguistic analogy would be state or political structures, which commonly generate and utilize large concentrations of power and which are usually relatively near the surface of social life. State and political structures are consciously established, maintained, fought over, and argued about rather than taken for granted as if they were unchangeable features of the world. Although one might initially imagine that large power concentrations would tend to assure a structure's durability, this may not actually be true. Although centralized states with immense coercive power impose high costs on those who would challenge them, it is far from clear that centralized and coercive states have generally proved more durable than relatively decentralized or uncoercive states. Compare, for example, Britain and France between 1750 and 1850, the United States and Germany from 1870 to 1950, Costa Rica and Nicaragua, El Salvador, or Guatemala since World War II, or India and China over the same time span. Even the relatively stable states are subject to periodic structural transformations. Although the United States has had a single constitution since 1789, it has experienced a succession of fundamental political crises that produced at least five sharply distinct party systems over the past two centuries (Burnham 1967). One might argue that state structures are relatively mutable precisely because the massiveness (power) and obviousness (lack of depth) of their resource effects make them natural targets for open struggles.

But if most political structures are characterized by both high power and low depth, an inverse relationship between power and depth is by no means necessary. There are some political structures with immense power implications that are nevertheless relatively deep, that have become "second nature" and are accepted by all (or nearly all) political actors as essentially power-neutral, taken-for-granted means to political ends. Such structures also appear to be unusually durable. This would appear to be true of political structures as diverse as the American constitutional system, the French public bureaucracy, or the English community legal structures whose persistence Margaret Somers (1986) has traced from the 14th to the mid–19th century. Durability, then, would appear to be determined more by a structure's depth than by its power.

Capitalism.—How do structures with huge power effects become or remain deep? One would normally expect the massiveness of the effects to make social actors aware of and willing to contest the schemas and resource accumulations of those structures. I will approach this question by examining the case of capitalism, a spectacular case of a power-laden yet long-enduring structure. Capitalism is, of course, highly dynamic. Yet it is commonly maintained that the past 250–300 years (if not the entire period since the 16th century, according to Wallerstein [1974]) constitutes a unified capitalist era with a continuous dynamic of capital accumulation guided by an enduring core structure, or what in Marxian parlance is called the capitalist mode of production.

Marx himself noted the extraordinarily dynamic and changeable character of capitalist development, but he saw the change converging on a single form: the large-scale, mechanized factory staffed by an increasingly homogeneous proletariat. Recent developments have tended to make the changeability of capitalism seem more radical and permanent. Far from registering the onrush of the classic factory, the current era of world economic growth has been characterized by an increasing use of subcontracting, sweatshops, outsourcing, and "cottage industry," and by the burgeoning of services at the expense of manufacturing. At the same time, scholars are increasingly pointing out the unevenness, contingency, and openness of development patterns under capitalism, whether in the past (Samuel 1977; Sabel and Zeitlin 1985; Sewell 1988) or in the present and future (Piore and Sabel 1984). Sabel (1988) has even suggested that forms of economic change in the so-called capitalist era are so indeterminate that the very concept of capitalism, with its implication of underlying regularity, is misleading and should be abandoned. I think Sabel is right as far as he goes: a wide variety of institutional arrangements and property relations are compatible with "capitalism," and never in its history has capitalism obeyed uniform "laws of motion." Capitalist development has always been a messy and uneven affair. But I think that the messiness has been at the level of secondary or surface structures and that beneath the surface mutability lies a far more stable deep structure of schemas that are continually reinforced by flows of resources—even on occasions when the surface structures are revolutionized.

Unlike most Marxians, I see the core schemas not as those defining the wage-labor relationship but as those governing the conversion of use value into exchange value.[12] The core procedure of capitalism—the conversion of use value into exchange value or the commodification of things—is exceptionally transposable. It knows no natural limits; it can be applied not only to cloth, tobacco, or cooking pans, but to land, housework, bread, sex, advertising, emotions, or knowledge, each of which can be converted into any other by means of money. The surface instability of capitalism arises precisely from this interconvertibility, which encourages holders of resources to trade them for other resources as relative values change and which always makes it possible for resources not previously treated as commodities to enter the circuit of monetized exchanges. To put it otherwise, the commodity form, by making almost all resources readable as exchangeable commodities, organizes a virtually universal intersection of structures, which means that changes in any one structure—an increased or decreased accumulation of resources or a new procedure—can affect an indefinitely vast number of other structures that intersect through the medium of money. Changes at any point in the circuit of exchange will give rise to resource effects and innovations elsewhere. And these changes are not necessarily constrained to follow any particular institutional form, so long as they are profitable. Thus the rise of the automobile industry stimulated the simultaneous development of rubber plantations based on indentured or forced labor, automobile assembly operations based on immense factories manned by wage-earning proletarians, and a proliferation of repair shops run by self-employed petty capitalists.

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[12] John Roemer (1982) has proved to my satisfaction that capitalist exploitation can occur in the absence of wage labor.

But this chronic instability or unpredictability of capitalism's surface structures actually reinforces its deeper structures. An alteration anywhere along the vast chain of commodity exchanges is a new incitement to invest; the logic inherent in the commodity form makes any new array of resources or new procedure a potential opportunity for profit. And of course any new investment results in further changes. Even investments that fail create new opportunities that can be seized by following the normal procedures of capitalist investment and exchange—when a firm goes under there is plant and equipment to be brought up at bargain prices, a residual market for the firm's former competitors to exploit, and so on. Consequently, the procedures themselves are remarkably impervious to—indeed, paradoxically, are reinforced by—the failures of particular capitalist enterprises or industries. The displacement of handweavers by the power loom or of coal by petroleum may have destroyed skills, wrecked businesses, or blighted the economies of certain localities. But it simultaneously proved that following the logic of the commodity form creates wealth for those who do so, and even—over the long run and in spite of important local exceptions—for the capitalist economy as a whole. In some cases, structures can combine depth with great power and, consequently, can shape the experiences of entire societies over many generations.

CONCLUSION

Beginning from the premise that structure is an unavoidable epistemic metaphor in the social sciences, I have tried to specify how that metaphor should be understood. Structures, I have argued, are constituted by mutually sustaining cultural schemas and sets of resources that empower and constrain social action and tend to be reproduced by that action. Agents are empowered by structures, both by the knowledge of cultural schemas that enables them to mobilize resources and by the access to resources that enables them to enact schemas. This differs from ordinary sociological usage of the term because it insists that structure is a profoundly cultural phenomenon and from ordinary anthropological usage because it insists that structure always derives from the character and distribution of resources in the everyday world. Structure is dynamic, not static; it is the continually evolving outcome and matrix of a process of social interaction. Even the more or less perfect reproduction of structures is a profoundly temporal process that requires resourceful and innovative human conduct. But the same resourceful agency that sustains the reproduction of structures also makes possible their transformation—by means of transpositions of schemas and remobilizations of resources that make the new structures recognizable as transformations of the old. Structures, I suggest, are not reified categories we can invoke to explain the inevitable shape of social life. To invoke structures as I have defined them here is to call for a critical analysis of the dialectical interactions through which humans shape their history.

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The Structure of Opportunity: Middle-Class Mobility in England, 1548–1689[1]

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Models that allow the decomposition of mobility into its structural and exchange components are used to identify the structure, and consequences, of middle-class intergenerational mobility in preindustrial (1548–1689) Norwich, England. Dramatic shifts in the mobility opportunities of sons over time are seen to yield distinct political outcomes. Political stability is associated with almost universal upward mobility in the period from 1548 to 1589, while from 1590 to 1639 structural processes leading to massive downward mobility are associated with increased radicalization and participation in the English civil war. From 1640 to 1689, strata persistence in an unstable political context prevailed.

INTRODUCTION

In this article we analyze the occupational mobility experiences of the urban middle class in Norwich, England, from 1548 to 1689. Our first goal, to identify the determinants of mobility across a substantively important historical context, is largely descriptive. Our second goal is interpretive and aims to link the collective mobility experiences of men to the demographic, economic, social, and political processes which occurred in England during the 16th and 17th centuries.

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Our approach makes use of an existing family of log-linear models that we use to analyze cross-classificatory tables. This allows for a unique partition of mobility dynamics as resulting either from structural or exchange mobility (Sobel, Hout, and Duncan 1985; Sobel 1988). The ability to distinguish between structural and exchange mobility is a critical component of most models of intergenerational occupational mobility in contemporary contexts (Featherman and Hauser 1978; Sobel et al. 1985). In this article, we show that distinguishing between structural and exchange components of a mobility regime is a basic prerequisite for understanding the social and political consequences of mobility in its historical context.

There are others who have seen the importance of mobility regimes as shapers of historical context (cf. Goldstone 1991; Stone 1965; Hexter 1963). Few have tried, however, to distingiush the unique political outcomes of patterns of mobility across regimes. For sons of landed elites, Goldstone (1991) identifies two mobility patterns—"expansion" and "turnover and displacement"—which are largely driven by population growth and contraction. He argues that expansion regimes co-opted newcomers and resulted in political stability, while displacement regimes generated elite competition and factionalism, and ultimately radicalized elements of the landed gentry. This is a provocative argument and one to which we are generally sympathetic. While the mobility patterns Goldstone identifies neglect patterned downward mobility and fail to distinguish between the structural and exchange components of a mobility regime—thereby conflating exogenous demographic effects with endogenous mobility effects—the period-specific structure of middle-class mobility that we identify corresponds roughly to his discussion of the patterns of elite mobility. We show that structurally driven downward mobility, in the period 1590–1639, led to the radicalization of sectors of the urban middle class, whereas the more "expansionary" period of 1548–89 was associated with political stability. Our models, supported by Goldstone's less quantifiable evidence, point toward the critical political role that intergenerational mobility plays in historical context. A central focus of this article is thus concerned with exploring the complicated interaction between mobility regimes and political action.

England from 1548 to 1689 provides a rich setting for our study. Consider just a few of the economic, social, and political developments that occurred: the price and population revolution (1548–1640), expansion of centralized markets for food and commodities (1548–1689), a rapid emergence of a permanent wage-labor force (Bowden 1967; Lachmann 1987), and spatial expansion of protoindustrial activity in the countryside (Kriedte, Medich, and Schlumbohm 1981). If the pace of economic change was rapid, so was the pace of political and cultural change. At the start of our period, in 1548, county elites thought of themselves as members of a county community. In 1640, local gentry conceived of themselves as members of a national political community and participated in a prolonged civil war that led to the execution of a reigning monarch (Bearman 1992). Likewise, in 1548, the politics of urban elites outside of London consisted of struggles for place and prominence within the local urban setting—and yet by 1640, urban provincial elites actively participated in the civil war and revolutionary government that followed (Allen 1951; Evans 1979). In 1548, English political society was localist, by 1640, and certainly by 1689, most of the politically active class worked within a framework of national politics. Cultural change was also dramatic. Radical religious beliefs—the various forms of Puritanism—emerged during this period and motivated both new forms of political agency and new models for economic action (Walzer 1965; Weber 1958; Bearman 1992).

If we consider the crucial cultural, political, economic, and structural changes that occurred in English society from 1548 to 1640—the industrialization of the countryside, the emergence of national food and commodity markets, the rise of large-scale urban manufacturing, the rise of Puritanism, and a radical civil war that permanently altered the structure of politics—we find an interesting constant. Common to all of these developments, two groups played a central role: the landed elite and the urban commercial middle class.

As a rule, historians have stressed the localist and narrow role played by the provincial urban middle class in these macro-level transformations (Everitt 1969; Evans 1979; Howell 1967; Clarke 1976). Likewise, historians have tended to see local urban social structure as largely insulated from macro-level, exogenous shifts in the economy (Evans 1979; Allen 1951). Both views fit poorly with a number of our findings. Rather than observing independence and isolation of the middle class, we show that the fortunes of the middle class were inextricably tied to larger exogenous processes and that the experience of downward and blocked mobility from 1590 to 1640 was associated with participation in the civil war of 1640. Below we focus on both the causes and consequences of the mobility regimes that we identify in our analyses.

The mobility experiences we analyze are limited to father-son pairs, of which the sons were enrolled as freemen in the city of Norwich. Throughout, our focus is on the men who controlled influence and assets beyond their own labor power and who, consequently, drove the provincial economy. These men were "work givers," purchasing the labor power of others, rather than "work takers," men who exchanged labor power for a wage. This focus on middle-class mobility sheds light on one level of social stratification within the urban social structure. Missing is the mass of men and women who worked in agricultural occupations, those who labored in the large urban manufactories, those who worked at home in protoindustrial rural cottage industries as weavers, spinners, and combers, and those on the top of the English preindustrial stratification system, the landed elite.

The models that we use are designed to partition structural and exchange mobility. This partition is of interest to us so that we can describe and compare structures of opportunity over time. Therefore, our analysis focuses on the structural dimensions of mobility regimes and ignores individual correlates of occupational mobility. While the models we employ have provided a stepping-stone for structural equation models of the individual determinants of mobility, net of structural constraint resulting from marginal heterogeneity (DiPrete 1990; Hout 1988; Landale and Guest 1990), our goal is necessarily different. Studies of 20th-century mobility typically include covariates of the status attainment process in addition to origin; the most important among these are education, ethnicity, and race (Featherman and Hauser 1978; Hout 1988). These covariates are not salient for preindustrial mobility in Norwich. We seek to interpret strata mobility within the political and economic context of that era and place.

Over the 140 years from 1548 to 1689, mobility was driven by structural shifts in the distribution of vacancies, forcing sons through occupational strata characterized by strong, diffuse associations. By itself this is interesting, but this summary statement masks critical differences across each of the three periods analyzed: 1548–89, 1590–1639, and 1640–89. Much of our descriptive focus is thus on delineating period-specific mobility patterns and comparing these patterns to the earlier mobility regimes. To anticipate our central results, we show that in period 1, from 1548 to 1589, Norwich urban middle-class men experienced sustained, structurally driven upward mobility. Across all but one stratum, sons occupied (on average) higher status jobs than their fathers. An expanding export economy driven largely by gains in the textile/woolen sector lay behind this long period of upward mobility. We argue that political stability is associated with the structurally driven upward mobility.

In period 2, from 1590 to 1639, sons of fathers in the elite strata experienced massive downward mobility driven by structural processes, while sons of lower-status fathers experienced blocked aspirations as the result of there being too few elite vacancies. We argue that the radicalization of sons within the Norwich middle class, and their support for Parliament during the English civil war, follows two generations of structurally driven downward mobility. We show that those strata most likely to yield radical leaders in Norwich were those that evidenced the highest levels of blocked mobility during this period.

From 1640 to 1689, the mobility regime is best fit by a model of quasi independence, indicating strata inheritance and the independence of mobility across occupational strata. In the first period, structurally driven upward mobility was associated with political stability. In the second period, the antecedent effects of downward mobility were associated with the radicalization of sectors of the urban elite during the English civil war and interregnum. Here the absence of patterned mobility—almost 70% of all sons inherited their fathers' strata—suggests that local political events were relatively autonomous of the Norwich mobility regime. It follows that one must search for causes beyond the mobility experiences of men to account for the political changes that occurred at the end of our third period.

Over time, men's mobility was driven by structural change in the distribution of vacancies across the upper part of the classification strata. A different stratum in each period played the leading role and thus largely defined the opportunities available for sons in other strata. The fortunes of the merchant elite, distributive, skilled crafts, weaver, and allied cloth strata shifted radically over time, largely in response to exogenous macro-level developments that transformed the local setting. Opportunity was defined by structural shifts in the distribution of vacancies, which serve to drive men into occupational strata. Structural mobility results from macro-level impacts that cause declines in one stratum and increases in others. To understand structural mobility, we have to understand the operators—in the economy and in politics—that determine marginal heterogeneity. By far the most important operators driving the preindustrial mobility regime of England were national, namely the rise of the new draperies, the decline of local luxury markets stimulated by central London expansion, and demographic stagnation.

DATA AND CONTEXT

A major constraint governing research into the structure of preindustrial intergenerational mobility is the absence of data of sufficient quality. With the exception of the recent work by Guest, Landale, and McCann (1989) and Landale and Guest (1990) on the structure of occupational mobility at the turn of the 20th century in the United States, Katz's (1975) work on urban Canadian mobility, Thernstrom's (1964) models of late 19th-century mobility in America, and Sharlin's (1979) research on mobility of middle-class artisans in Frankfurt, much of the earlier research has suffered from marginal data and weak sample frames.[2] These weaknesses have led to conflicting assessments of the structure of mobility before the Industrial Revolution, making even simple statements about the comparative openness of mobility regimes hazardous. Because intergenerational mobility data require that a sample of fathers with known occupations be linked systematically to sons with known occupations, representative intergenerational mobility data for strata below that of the elite are exceedingly rare, and selectivity effects are typically severe. In this section, we describe our data, our classification of occupations into strata, the periodization, and we discuss the potential sources of selectivity bias that might be seen to threaten the reliability of the models reported. Given the limitation that our data report only on the mobility of freemen—the top one-third of the urban social structure—we conclude that our sample is representative, that selectivity bias is minimal, and that our mobility models are reliable.

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[2] In addition to selectivity bias in the samples has been the unfortunate tendency in historical research to misclassify occupations and categories. Occupational misclassi-

Norwich Freemen

All of the mobility data used in this article are derived from a register reporting the occupations of the 7,654 men who, from 1548 to 1689, obtained the freedom of the city of Norwich (Evans 1979; Pound 1966, 1981; Millican 1934). Norwich, England, provides an excellent setting for a study of preindustrial social mobility for a number of reasons. Norwich was the largest and most affluent provincial city in England from 1548 to 1689; it was second only to London in social, political, and economic importance. It was a center of the textile industry, the most important sector of the English economy throughout the 16th and 17th centuries. And, most important, the occupational and political structure of 16th- and 17th-century Norwich has been extensively researched, which provides a framework for inducing the occupational strata that are the focus of this research.

Our analysis is restricted to freemen of Norwich. Men obtained the freedom of the city either through inheritance (patrimony), service (apprenticeship), or purchase.[3] Freedom of the city meant that an individual

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fication that results in heterogeneous categories generates unreliable estimates of mobility. Finally, the tendency to import anachronistic categories in our models of preindustrial mobility process, e.g., using a modern classification scheme—professionals, managers, clerical, craftsmen, operative, farmer—where these strata have no tangible salience in historical context, has meant that our understanding of preindustrial mobility is lessened (Hauser 1982).

[3] The distribution of persons who obtained the freedom of the city through inheritance, service, or purchase changed over time. Evans (1979, n. 10) reports that "from 1590 to 1640 the percentage of freedoms gained by patrimony, service, and purchase were 22.7, 46.4, and 30.4 respectively, whereas during the period 1640 to 1690 the percent-

was a citizen able to engage in commerce, control the labor power of others, and participate in the political life of the community. Men who were not free—that is, men who were not admitted to the freedom of the city—could not engage in a craft specialty, market goods, or take on apprentices or journeymen. Guild and city regulations ruthlessly protected the economic interests of the free citizens by driving out unregistered craftsmen, producers, and commercial agents or forcing these latter to buy freedom to practice their craft.

Consequently, the largest occupational groups in the local economy, the underemployed, agricultural laborers, men with few skills, and journeymen, are not represented. Also not included in the data are the hundreds of women and children who worked in the textile and allied industries as spinners, combers, or carders of wool. Behind the mobility experiences of the stable economic actors lay the majority of Norwich residents who were poor and without prospects. These men and women experienced little mobility and led brutal lives of quiet desperation.

It follows that the mobility models discussed here are restricted to the fortunes of the elite group of economically secure men who were able to obtain the freedom of Norwich. These men compose the strata of "work givers"—the economic actors whose activity drove the local economy throughout the 16th and 17th centuries.

Since freedom of the city enabled men to ply their trades, recorders were careful to note the exact occupation for which freedom was obtained. The occupations that were enumerated are extremely detailed. For example, the register notes six different types of weavers, three different grades of masons, and over a dozen types of "textile makers," partitioned by either the type of cloth they worked with or the end product. In all, over 160 distinct occupations were recorded by the listing clerk. One of the benefits of using these lists derives from the detailed occupational data provided. It is clear, though, that detailed cross-classifications

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age of freedoms gained by patrimony, service, and purchase were 36.1, 50.5, and 11.7 respectively." Likewise, Pound (1981) reports that, in 1551, 17.5% of new admittances were based on patrimony, rising to "just over 28 percent in the second quarter of the seventeenth century." Despite these shifts, it is important to note that the proportion of sons in our sample who obtained their freedom of the city through patrimony or apprenticeship did not change over time. The real losers in these shifts were strangers who sought to purchase this freedom. Men who purchased freedom were "new men" who were prevented from obtaining their freedom through inheritance or service, and the price of freedom was exceedingly high. In our models these men appear as fathers but not as sons. The increase in the proportion of apprenticeships in period 3 suggests that, despite significant social and political dislocation as a result of the civil war, middle-class men continued to work within the traditional parameters of economic life. Underneath the instability of the political world lay economic stability.

would yield too many empty cells and that therefore some aggregation of occupations into internally consistent, ranked strata is necessary.

All of the men are middle class, and thus occupy a similar position with respect to the gentry and the mass of the laboring poor. Within the Norwich middle class, occupational groups showed distinctive status and wealth differences that were stable over time (Allen 1951; Pound 1966; Corfield 1976; Evans 1979; Blomefield 1806; Earle 1989). We derive ordered strata from a number of different sources that evidence wealth and status differences across occupations.[4] We identify seven occupational strata: merchant elite, distributors, skilled craftsmen, weavers, allied cloth workers, leather workers, and semiskilled craftsmen. Below we briefly describe each stratum.

Grocers, mercers, drapers, clothiers, hosiers, and merchants composed the merchant elite of Norwich throughout the 16th and 17th centuries. The merchant elite held substantial property in the city and county and were involved in large-scale translocal commercial markets (Pound 1966; Evans 1979; Fisher 1935; Grassby 1970).

The second stratum, distributors, was composed of a class of purveyors of restricted goods—haberdashers, mongers, brewers, and bakers—whose involvement in the economy was strictly local. The distributive stratum engaged in retail trade (Earle 1989; Crossley 1972).

The third stratum are the skilled craftsmen—goldsmiths, silversmiths, clockmakers, coopers, and wrights—who produced goods for the local luxury market (Earle 1989). Across all strata, these occupational groups were the most successful in ensuring occupational closure through apprenticeship (Millican 1934).

Weavers of bay, silk, russel, dornix, and worsted compose the fourth and intermediate stratum. The typical weaver worked in a small shop with two or three others, controlling very little more than a few looms and a small city lot (Kerridge 1985; Cooper 1969).

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[4] Some indication of status, net of wealth, for Norwich freemen is the order in which occupations marched in celebratory or political events (Blomefield 1806; Allen 1951). Likewise, the proportion of men from an occupation who are elected as mayor or alderman of the city yields a measure of occupational status (Blomefield 1806; Owens 1970; Allen 1951; Evans 1979). Finally, data on average start-up costs for both merchants and craftsmen—in London—are available and allow us to estimate occupational closure, which we treat as a proxy for occupational status. Together with our understanding of the historical context, these sources provide the framework from which we induce a rank ordering of occupational status. We prefer this use of historical sources to derive the order of strata, rather than an a priori statistical ordering from association models or network models (Breiger 1980), because of the large number of distinct occupations listed in the Norwich register. It is important to note, however, the Sobel et al. (1985) parameterization of quasi symmetry does not depend on ordered categories of the mobility table.

Occupations involved in the production of cloth preparatory to its being woven compose the allied cloth sector. This stratum, as with the weavers, was dependent on textile production for jobs. Composed of dyers, carders, combers, hot pressers, calanders, and sheermen, allied cloth workers engaged in tedious manual labor. These crafts were among the first to be deskilled during the Industrial Revolution (Earle 1989).

Below the allied cloth workers were leather workers, men who manipulated animal skins—tanners, glovers, and hatters.[5] While constituting 10% of the free population in Norwich, leatherworkers were considered to be of low status (Evans 1979).

Below all others in the middle class, were the unskilled and semiskilled laborers—carters, porters, carriers, paviors, and fishermen. Despite their relative poverty, compared to the other strata, it is important to recall that they were among the top one-third of the Norwich population whose fortunes, if small, were secured by restricted occupational access and a corresponding diminution of competition from those below.

Sample Selectivity

A recurrent problem in historical mobility analyses is that the available data are not representative of a population. Our data are derived from freemen's lists and our models are necessarily restricted to the urban social structure and cannot be generalized to mobility processes throughout England. While generally considered to be reliable sources (Pound 1981; Sharlin 1979), freemen's lists have been criticized for misrepresenting occupational distributions following visitations of plague, which yield strata-unique death rates, for idiosyncratic recording of fathers' occupations, and for censoring bias resulting from the differential migration of sons (Dobson 1973; Patten 1977; Woodward 1970). There is no evidence that these concerns affect our results.

While mortality from plague in the textile and weaving sectors was high in 1578–79, 1588, 1593, 1603, 1634–35, and 1666–67, plague deaths were not associated with unusually high admissions to the freedom of Norwich (Evans 1979). Even though in 1578–79 plague resulted in the death of one-third of the male population, only 2% of the new admissions in textiles during the 1580s appear to have been related to the plague (Pound 1981).

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[5] In the London context, glovers and hatters were high-status occupations, but in Norwich they were considered to be of low status. Of the 589 richest men in Norwich in 1645, none were leather workers. Over the 140 years under study, no men in the leather trades were elected to elite political positions in the city. Leather work is often associated with impurity and consequently is, even in Western society, considered to be a low-status profession. The Norwich data suggest this was true in urban areas where leather work was not an important industrial activity (Clarkson 1960).

For 18.1% of the 7,654 men who obtained the freedom of the city from 1548 to 1689, the recorder for the city listed the occupations of their fathers. These 1,382 father-son pairs make up our study sample. There appears to be no identifiable selection bias from idiosyncratic recording of fathers' occupations. Each occupation within each period had roughly the same proportion of men whose fathers' occupations were listed, and persistence (or the rate of occupational inheritance) rates in these data are similar to those in other urban preindustrial contexts (Sharlin 1979). Neither the characteristics of sons, nor the number of new entrants in a year, played a role in the decision to record a father's occupation; the distribution of occupations for which we have linked father-son pairs is similar to the underlying population.

From the freemen's lists, we have identified 243 pairs of brothers and found that their rates of occupational strata inheritance are almost identical: approximately 66% of elder sons inherit their fathers' strata while over 61% of their younger brothers do the same (Bearman, Deane, and McKenna 1991).[6] While many men migrated from Norwich during this period there is no evidence that the younger sons of Norwich freemen were more likely to migrate than their older brothers. As Thirsk argues (1984, p. 337), "The careers of younger sons and the fortunes of their families call for further investigation at all class levels. . . . Primogeniture did not occupy a tyranny over classes below that of the gentry." These data suggest that there is no serious censoring bias stemming from the migration of younger sons.

Considering all of the objections to the use of registers of freemen for studies of occupational structure, Pound (1981, pp. 58–59) concludes that, "While recognizing the importance of wills and inventories as occupational sources where freemen's lists do not exist, and their importance in illustrating those occupations which cannot be obtained from such lists, it is suggested that they are a distinctly inferior, and indeed misleading source. . . . As an indicator of primary occupation, freemen's lists are indispensable." Our consideration of selectivity biases supports this general conclusion, and we extend it to include the use of freemen's lists for mobility studies.

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[6] The 2 × 2 cross-classification of elder and younger brothers by strata inheritance and strata mobility yields a $\chi^2 = 0.8$, 1 *df*. Therefore, we accept the null hypothesis of no difference between brothers.

The Study Sample

We compare mobility regimes over time, utilizing data on the mobility experiences of 1,382 men over 140 years. The majority of sons (58.9%) held positions in their fathers' strata. Given these data constraints, our models focus on comparative mobility across periods, which summarizes the moves of two cohorts of freemen—roughly 50 years. Generation-specific models and comparison of mobility regimes across six generations would have been preferable, but the relatively small number of movers meant that, at best, if we assumed a random distribution of destinations from origin, we would have an average of only three sons for each off-diagonal cell of the 7 × 7 table. Given this, our strategy has been to partition time in order to yield periods that embrace the most important social and political changes that occurred in England during the period 1548–1689.

We identify three periods, which appear from the national historical evidence, to present relatively crisp contrasts. The first period (1548–89) was marked by social and political stability; the second period (1590–1639) witnessed faction formation and increasing political discontent; the third period (1640–89) embraced remarkable political upheaval in civil war, the execution of a reigning monarch, radical parliamentary rule, and a counterrevolution that resulted in the restoration of the Stuart monarchy, which in 1688 broke down in crisis. Internally, all three periods were demographically and economically homogeneous; thus our temporal divisions isolate the association between economic regimes and political action. In the first period, national population growth and available positions grew rapidly. From 1590 to 1639, population growth continued but the expansion of elite positions constricted—leading to conflict in the English civil war. The third period was defined by marked demographic stagnation (Wrigley 1987, pp. 218–23; Goldstone 1986).

Table 1 summarizes the characteristics of our study sample described above. The top half of table 1 reports the total number of newly admitted Norwich freemen, the number whose fathers' occupations were listed (our study sample), the ratio of admitted freemen to our estimated free populations, and the proportion of admitted freemen in our study sample for each time period. Although our sample proportion declines over the three periods, we find no reason to suspect that our sample is biased.[7]

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[7] Although we find no evidence of systematic bias in the recording of fathers' occupations across each period, table 1 does show a decline in the proportion of recorded fathers' occupations, from 30% in period 1 to 23% in period 2 to 10% in period 3. Clerks wrote a short inscription when a freeman was first registered as having freedom of the city. Although they were required only to list the occupation in which freedom was obtained, clerks often embellished the entry with additional personal detail about the freeman. To some, fathers' occupation was of interest, to others, different detailed

TABLE 1

Total and Sampled Freemen and Odds Ratios for the Association between Sampled and Listed Freemen

	Period 1 (1548–89)	Period 2 (1590–1639)	Period 3 (1640–89)
No. of admissions to freedom	1,223	2,821	3,610
Admissions to freedom/free population*	.52	.49	.50
No. in sample	369	643	370
Sample/freeman	.30	.23	.10
Strata			
Merchant elite	.96	.88	2.00
Distributor	.96	.96	.88
Skilled craftsman	.99	1.14	1.28
Weaver	1.46	1.23	.79
Allied cloth worker	.73	.74	.97
Leather worker	.93	.88	.78
Semiskilled craftsman	1.03	1.06	1.07

Source.—Millican (1934).

Note.—The ratio of admissions to freedom to estimated free population for period 1 is based on counts from the 1580–89 decade only.

* Free population is estimated in the Appendix, table A1.

Table 1 also shows that the proportion of the free population represented by new admissions was constant across the three periods (.52 in period 1, .49 in period 2, and .50 in period 3) and the odds ratios for the association between sampled freemen and listed freemen, shown in the bottom half of table 1, are generally close to "1." There are several exceptions. Allied cloth workers in the first and second periods are underrepresented, while weavers in the first period and the merchant elite in the third period are overrepresented.

The influx, starting in 1565, of Protestant refugees, who brought the new draperies from the Low Countries to Norwich, accounts for the overrepresentation of the weaver stratum in the first period.[8] Recorders were sensitive to the wave of refugees and distinguished them from na-

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information about the freeman was more noteworthy. A careful reading of entries reveals that busy clerks recorded less detail, thus as the number of applications for freedom increased, recorded fathers' occupations decreased, but this proportional decrease over time is not systematically related to fathers' occupational distribution.

[8] In 1565, letters patent from Queen Elizabeth granted Norwich the power to admit Protestant refugees to the freedom of the city so they could exercise "the faculties of making bays, arras, sayes, tapestry, mockadoes, staments, carsay and such other outlandish commodities as hath not bene used to be made within this our Realme of England" (Millican 1934, p. xiii).

tives by noting patrimony, even if the new freeman was admitted through apprenticeship.

Overrepresentation of the merchant elite in the third period is likely a by-product of political instability. In a context where the political careers of those in the elite strata were made, unmade, and made again within decades (e.g., there were purges of officeholders in 1648–49, 1660–64, and 1676–88), the lineages of men who controlled significant economic resources would be salient to recorders and others controlling new admissions.

Our sample also underrepresents allied cloth workers in the first and second periods. Other than the possibility that recorders were unable to clearly distinguish allied cloth workers from weavers and thus classified some of them incorrectly, we are unable to account for the underrepresentation of this stratum.

It is interesting that the first period is characterized by pronounced structurally driven upward mobility, despite the fact that it may contain too many lower-status weavers. Likewise, the odds of sons becoming merchant elite in the third period are exceedingly small, even though our sample may overrepresent this category. In both cases the potential bias cuts across the general pattern of mobility. Thus there are no theoretical reasons to expect that it is associated with the dependent variable: the structure of mobility over time. In summary, all the available evidence suggests that our data, our classification of occupations into strata, and our periodization, despite political heterogeneity in the third period, are quite robust given a basic constraint of matching fathers to sons over 140 years three centuries ago.

METHOD AND MODELS

Father-son occupational strata mobility is measured by using selected log-linear models. We begin by fitting the distributions of each of the three periods, 1548–89, 1590–1639, and 1640–89, independently; we than assess differences in intergenerational strata mobility across time. If sons showed no pattern of intergenerational mobility other than inheriting their fathers' occupational strata, this situation is described by a model of quasi independence. Quasi independence (QI) fits the main diagonal of the mobility table perfectly while positing that stratum of origin and stratum of destination are independent elsewhere in the table and, as such, represents an appropriate baseline for our study. The basis for our analysis, however, is the Sobel et al. (1985) parameterization of the quasi-symmetry model (QS) and its extension for multiway mobility tables (Sobel 1988). Sobel et al. have shown that an acceptable fit of QS allows an unambiguous decomposition of mobility into its structural and exchange components, and, as such, QS represents a dramatic advance in our ability to interpret the mobility process.

The concepts of structural and exchange mobility have been important features of studies of intergenerational mobility, but their unambiguous measurement has always been troublesome, and disagreement over the most appropriate analytic approach has been extensive (Cobalti 1988; DiPrete 1990; Hauser 1986; Hauser and Grusky 1988*a*, 1988*b*; Hout 1988; Jones 1985*a*, 1985*b*, 1986; Krauze and Slomczynski 1986*a*, 1986*b*; Slomczynski and Krauze 1988; Sobel 1983; Sobel, Hout, and Duncan 1986). The debate has revolved around the use of log-linear modeling with cell-specific parameter estimates versus more elegant single parameter measures and the decomposition of mobility into structural and exchange components. While we certainly are sympathetic toward simple statements of mobility, we are more concerned with isolating and quantifying the mobility experiences of particular strata in preindustrial Norwich and relating these experiences across time. Cross-classification tables with a one-to-one correspondence among categories, and particularly the QS model, allow us to do this. Following the work of Sobel et al. (1985) we match the concept of structural mobility (mobility caused by transformations in occupational opportunities), with a column effect that accounts for all marginal heterogeneity, and we match exchange mobility with symmetric exchanges between pairs of occupational strata.

When marginal distributions in a square table differ, as is the case with our data, some movement from origin to destination is forced by the unequal marginals. This shift is due to differences in proportional distributions of origins and destinations. Table 2 reports the observed frequencies for our three periods. Data for period 1 in table 2 make it clear that some sons destined for the merchant elite had to come from lower strata, since there were 70 merchant elite fathers and 81 merchant elite sons. In this period, 11 sons would be pulled into the first column. If the movement to a particular destination is allowed to affect all origins proportionately, the movement from origin to destination due to marginal differences represents the concept of structural mobility (Featherman and Hauser 1978; Sobel et al. 1985).

Sobel (1983) and Sobel et al. (1985) have shown that efforts to model structural mobility as the residual of total mobility after the exchange component has been estimated, or conversely, efforts to model exchange as the residual of structural mobility, lack substantive meaning; numerous attempts merely to equate structural mobility with trends in the distributions of occupations over time should also be rejected because many exogenous factors are known to affect origin and destination distributions (Cobalti 1988; Hauser and Grusky 1988*a*). Under QS, structural mobility may be interpreted as a factor that raises or lowers the odds on

TABLE 2

DISTRIBUTION OF SONS BY FATHERS

FATHERS' OCCUPATIONAL STRATA	OBSERVED COUNTS (f_{ij}) FOR SONS' OCCUPATIONAL STRATA							TOTAL PER STRATUM
	Merchant Elite	Distributor	Skilled Craftsman	Weaver	Allied Cloth Worker	Leather Worker	Semiskilled Craftsman	
Period 1:								
Merchant elite	46	7	2	8	4	2	1	70
Distributor	10	31	6	3	1	5	3	59
Skilled craftsman	1	5	19	2	2	1	1	31
Weaver	8	5	1	48	3	7	7	79
Allied cloth worker	7	5	0	5	24	3	2	46
Leather worker	7	6	1	4	2	22	0	42
Semiskilled craftsman	2	2	2	7	5	8	16	42
Total for period	81	61	31	77	41	48	30	369
Period 2:								
Merchant elite	65	15	4	35	5	2	3	129
Distributor	7	51	1	37	6	8	1	111
Skilled craftsman	2	3	39	4	7	3	4	62
Weaver	3	2	2	76	4	1	6	94
Allied cloth worker	2	16	6	22	39	7	2	94
Leather worker	2	3	1	12	2	42	5	67
Semiskilled craftsman	3	6	10	20	8	5	34	86
Total for period	84	96	63	206	71	68	55	643
Period 3:								
Merchant elite	39	3	1	10	9	1	0	63
Distributor	2	30	1	7	8	1	0	49
Skilled craftsman	2	0	29	4	2	1	2	40
Weaver	1	4	1	53	9	4	2	74
Allied cloth worker	0	2	0	8	48	2	0	60
Leather worker	3	0	1	4	2	28	1	39
Semiskilled craftsman	0	0	1	9	5	2	28	45
Total for period	47	39	34	95	83	29	33	370

SOURCE.—Millican (1934).

a son's occupational stratum relative to that stratum's proportion of the distribution of fathers' strata (Sobel et al. 1985, p. 361).

On the other hand, the concept of exchange, or circulation, mobility represents the flow between pairs of origins and destinations. Following the QS parameterization, exchange mobility is defined as the equal flow between cells (i,j) and (j,i). The QS model constrains the origin-destination interactions to be symmetrical, allowing the marginals to be heterogenous (see also Hout 1983; Knoke and Burke 1980). Exchange coefficients may be interpreted as the odds on sons changing occupational strata relative to staying in their fathers' strata. These parameters measure association inversely to the usual convention for cross-product ratios, that is, a small value indicates a strong association between pairs of strata and, consequently, relatively little exchange.

Model fits are estimated using GLIM (Baker and Nelder 1978). Note that the figures for period 3 in table 2 include a fairly large number of sampling zeroes among those frequencies. Depending on the location of the zeroes in a contingency table, GLIM may produce erroneous parameter estimates with greatly inflated standard errors (SEs). Lindsey (1989) shows that the solution to this problem is to eliminate those zero cells responsible for this occurrence. We utilize this correction when it is appropriate.[9]

Quasi independence, reported in table 3, shows an acceptable fit only for the period from 1640–89, indicating independence of sons' occupational destinations from fathers' occupations for those sons who experienced occupational mobility during this third period. It is during period 3 that we also find a substantial increase in the proportion of observed frequencies along the main diagonal of the mobility table. From 1548 to 1639, that is, periods 1 and 2, approximately 55% of all sons inherited their father's occupational stratum. This figure jumps to nearly 70% during period 3.

Quasi symmetry fits father-son strata mobility in all three periods, particularly well in period 1 ($\chi^2 = 16.02$, 15 *df*) and period 3 ($\chi^2 = 13.17$, 13 *df*). Although QS fits the observed frequencies in period 2 under a conventional definition of statistical significance, we can substantially improve the model fit by including an apparent asymmetric exchange between the weaver and semiskilled craftsmen strata. The additional γ parameter reduces the χ^2 of QS by 5.79 with the loss of 1 *df*.[10] This

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[9] When sampling zeroes affect a fitted model, some residuals of observed minus fitted cell frequencies will be either very large positive or negative. In these cases, a zero weight is assigned to the offending cell, while all other cells are weighted by their frequencies (Lindsey 1989).

[10] Under QS, expected cell frequencies are the product of three types of parameters: alpha (α_j) coefficients, which capture marginal shifts between fathers' strata and sons'

TABLE 3

SELECTED MODELS FOR FATHER-SON OCCUPATIONAL MOBILITY

	χ^2	*df*	*P*
Period 1, 1548–89:			
QI model	41.53	29	.03
QS model	16.02	15	.20
Period 2, 1590–1639:			
QI model	64.14	29	.00
QS model	25.24	15	.05
QS + γ model*	19.45	14	.18
Period 3, 1640–89:			
QI model	30.51	29	.23
QS model	13.17	13	.28
Across all periods, 1548–1689:			
CQS model	54.45	43	.05
CQS + H_α model	112.40	55	.00
CQS + H_8 model	129.00	83	.00

* $\gamma_{ij} = \gamma$ when $i = 4$ and $j = 7$, $\gamma_{ij} = 1/\gamma$ when $i = 7$ and $j = 4$; $\gamma_{ij} = 1$ otherwise.

unreciprocated exchange between sons of semiskilled craftsmen who moved upward into weaving occupations and sons of weavers who eschewed semiskilled occupations warrants caution when interpreting the structural and exchange mobility ratios formed by these two strata (Sobel et al. 1985, 1986). Note also that the change in χ^2 from QI and QS also is statistically significant for periods 1 and 2, but not for period 3 ($\Delta\ \chi^2 = 25.51$, 14 *df*, $\Delta\ \chi^2 = 38.90$, 14 *df*, and $\Delta\ \chi^2 = 17.34$, 16 *df*, for periods 1, 2, and 3, respectively).[11] Although QI fits the period 3 cross-classification quite well, a further test of the difference between the model of symmetry and QS (not shown) is statistically significant, thus establishing the contribution of structural mobility. It follows that we have no compelling reason to examine its parameter estimates more closely.

Since our purpose is also to offer insight into the extent of the change in occupational strata mobility over 140 years, we now combine the

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strata; delta (δ_{ij}) coefficients, which measure the association between symmetric pairs of occupational strata; and beta (β_j) coefficients, which represent symmetric adjustments for fathers' and sons' marginal frequencies. In addition, where QS does not strictly hold, some degree of unreciprocated mobility between pairs of strata is operating. This asymmetrical association and/or uniform effect may be denoted as gamma (γ_{ij}) (Sobel et al. 1985; Sobel et al. 1986).

[11] Note that the difference in degrees of freedom for the QS model between period 3 and periods 1 and 2 is due to the correction for sampling zeroes. This reduction in degrees of freedom is due to the elimination of two parameter estimates with grossly inflated SEs. Thus the correction establishes a more stringent test of the QS model fit in the presence of sampling zeroes.

period-specific QS models into a three-way contingency table, a conditional quasi-symmetry model (CQS) of fathers' strata with sons' strata by period (see Sobel 1988). Under CQS, interactions with period are of substantial interest because they quantify the shifts in structural and exchange mobility across the study periods and allow explicit tests of the period heterogeneity assumption.

Given the acceptable fit of CQS (shown in "Across all periods," table 3), we next try imposing homogeneity restrictions for period on structural and exchange mobility parameters. The first alternative model posits that CQS holds but that there is no difference in structural mobility over the three periods (CQS + H_α); the second alternative tests whether exchange mobility across the three periods is homogeneous (CQS + H_δ). Since neither of these restrictions on the CQS model attain an acceptable fit, and the change in χ^2 from CQS to CQS + H_α and from CQS to CQS + H_δ are statistically significant ($\Delta\ \chi^2 = 57.95$, 12 *df*, and $\Delta\ \chi^2 = 74.55$, 40 *df*, respectively), we conclude that no general statements about structural and exchange mobility may be made about the three periods.

In summary, the strata mobility models reported in table 3 have shown that (1) each period is adequately described by QS, (2) the values of parameters that estimate structural and exchange components of strata mobility are unique within each period, and (3) period 3 is best fit by the model of QI. Although the conclusion that the occupational destinations of sons who do not inherit their fathers' stratum is independent of fathers is succinct and conceptually simple, the presence of structural mobility is not precluded by an acceptable fit of QI. Consequently, we make specific inferences about strata mobility from the QS (tables 4–6) and CQS (table 7) parameter estimates.

RESULTS

Period 1: 1548–89

In the period from 1548 to 1589, sons benefited from almost uniform upward structural mobility which drew them into the more elite destinations. Structural mobility ratios between categories are reported below the diagonal in rows 11–16 in table 4. These ratios index the extent of downward or upward social mobility as driven by structural changes exogenous to exchange mobility. Ratios less than one mean that structural mobility was downward, ratios greater than one imply upward mobility as the result of marginal shifts. In interpreting these ratios, we give particular attention to values above 1.3 and below 0.7, which we consider to be of substantive significance.

We are interested in sons' opportunities relative to fathers as they are conditioned by structural change. Consider the odds of a son becoming

TABLE 4

INTERGENERATIONAL OCCUPATIONAL MOBILITY IN NORWICH, ENGLAND: PERIOD 1, 1548–89

	DESTINATION						
ORIGIN	Merchant Elite	Distributor	Skilled Craftsman	Weaver	Allied Cloth Worker	Leather Worker	Semiskilled Craftsman
Fitted counts under QS (F_{ij}):							
1. Merchant elite	46.0	7.6	1.3	6.3	3.9	4.2	.8
2. Distributor	9.4	31.0	5.2	3.6	2.5	5.7	1.6
3. Skilled craftsman	1.8	5.8	19.0	1.4	.9	1.1	1.0
4. Weaver	9.7	4.4	1.6	48.0	3.7	6.3	5.2
5. Allied cloth worker	7.1	3.6	1.1	4.3	24.0	3.1	2.9
6. Leather worker	4.8	5.3	.9	4.7	1.9	22.0	2.4
7. Semiskilled craftsman	2.2	3.4	2.0	8.8	4.1	5.6	16.0
Marginal parameter estimates:							
8. α_j	1.47	1.18	1.05	.94	.81	1.28	.56
9. β_j	4.19	3.85	3.19	5.34	4.08	3.10	4.00
δ_{ij} (above diagonal) and α_j / α_i (below diagonal):*							
10. Merchant elite	. . .	.22 (.06)	.05 (.03)	.17 (.05)	.16 (.05)	.14 (.05)	.05 (.03)
11. Distributor	1.25		.23 (.08)	.10 (.04)	.11 (.05)	.21 (.07)	.10 (.05)
12. Skilled craftsman	1.40	1.12		.05 (.03)	.05 (.03)	.05 (.04)	.08 (.05)
13. Weaver	1.56	1.26	1.12		.12 (.04)	.17 (.06)	.24 (.08)
14. Allied cloth worker	1.81	1.46	1.30	1.16		.11 (.05)	.18 (.07)
15. Leather worker	1.15	.92	.82	.73	.63		.20 (.08)
16. Semiskilled craftsman	2.63	2.11	1.88	1.68	1.45	2.29	. . .

* Standard errors, which were obtained through the δ method, are in parentheses.

a merchant relative to the odds of a merchant's son losing status and becoming a retailer, craftsman, weaver, or semiskilled craftsman. These odds are reported, for each category, down the first column. Thus, for instance, the son of a semiskilled craftsman is 2.63 times more likely to become a merchant than a merchant elite's son is to become a semiskilled craftsman. Independent of origin, all sons have greater opportunity to move into the merchant elite than the merchant elite sons have of moving down. In period 1, from above, sons can expect to do better than fathers by virtue of pervasive upward structural mobility. Sons of the merchant elite remain in the elite, although the openness of the category yields dilution of the elite status.

These mobility ratios evidence, with the exception of the leather workers category, a monotonic ordering down the columns of the lower half of table 4. The expansion in higher-status strata during this period creates an excess of positions for sons of lower-status fathers; consequently, the ratio for sons of distributors becoming merchant elites is lower than that for merchant sons whose fathers were semiskilled craftsmen. Demonstration of the strength of upward structural mobility above the dyadic level, where structural effects for occupation pairs are revealed, is induced from the ordered pattern of the α_j coefficients (see row 8, table 4). Disrupting this monotonic pattern is the case of the leather trades. The leather industry expanded somewhat throughout the late 16th century (Clarkson 1960) and consequently almost all sons, independent of origin, were drawn into the industry. Note that α_j for leather workers is 1.28, which demonstrates an excess of sons of various origins entering the leather trade. Note that while ratios for the leather workers (across row 15, table 4) are almost all less than one, only the ratio for sons of allied cloth workers reaches substantive significance (.63).

As QS fits these data we are able to uniquely partition the mobility process into its constituent components: mobility as a result of marginal shifts (structural mobility) and mobility as a result of exchange (circulation or exchange mobility). Above the diagonal in rows 10–16 of table 4, we report the δ_{ij} coefficients for each pair of strata. Small coefficients report strong association and indicate little exchange that can be attributed to association, while large coefficients report weak association and signal substantial exchange; if an association coefficient is insignificant then the exchange between a particular pair of origin and destination is essentially open (Sobel et al. 1985).

Note that in period 1 every δ_{ij}, except for the stratum of skilled craftsmen, is small and statistically significant, suggesting strong association. Apparently, occupational strata were closed to strata encroachment, even from those strata closely arrayed in social space. In general, mobility in the period 1548–89 is described by a patterned movement of sons upward through the urban occupational strata, despite the globally strong association between origins and destinations.

Period 2: 1590–1640

In the 50 years preceding the English civil war of 1640, the fortunes of the Norwich middle class worsened. In this period, downward structural mobility was profound, and it forced sons of elites into lower-status positions.

Consider table 5, row 8, which reports α_j coefficients for period 2. Recall that values less than one index downward mobility. Structural change in this period is driven by the expansion of the weaver occupational stratum and the simultaneous severe contraction of all other occupations. Thus, less than half as many sons as expected (.47) became merchant elites, and while more than five times the expected proportion of sons became weavers, the upward mobility of weavers' sons was almost completely eliminated. This pattern can be more clearly demonstrated if we turn to the mobility ratios for estimating structural mobility as reported in rows 10–16 of table 5.

Reading down column 1, we see that the odds of a distributor becoming a merchant elite are half (.48) that of a merchant elite becoming a distributor. Considered from the point of view of an elite son, this means that sons of elites are twice as likely to move down to distributors than sons of distributors are to move up. Sons of elites experience downward mobility, and sons of retailers have blocked aspirations as the result of occupational closure. With one exception (row 12, col. 2), which does not appear substantively significant, each of the three top occupation groups experience downward mobility relative to the adjacent category.

While the elite occupations are closed to those below, much of the action is occurring with the weavers as reported in column 4 (to obtain ratios for downward mobility for the top three occupations into the weaver category, compute the inverse of row 13). The odds of any son becoming a weaver are 4–11 times greater than the odds of a weaver's son moving to another occupation. Weaving occupations drew men down and up the mobility ladder. This observation becomes even more striking when we consider the mobility ratios representing the odds on upward mobility for the sons of weaver fathers. Sons of weavers had practically no opportunity for structurally driven upward mobility, as their odds on moving up are all less than 0.2. Related to the expansion of opportunity in weaving, however, the sons of allied cloth workers and semiskilled workers experienced a degree of upward mobility, although, again, they were blocked from the merchant elite, and the bulk of their opportunity for advancement was to be found in the weaving occupations.

TABLE 5

INTERGENERATIONAL OCCUPATIONAL MOBILITY IN NORWICH, ENGLAND: PERIOD 2, 1590–1639

ORIGIN	DESTINATION						
	Merchant Elite	Distributor	Skilled Craftsman	Weaver	Allied Cloth Worker	Leather Worker	Semiskilled Craftsman
Fitted counts under QS (F_{ij}):							
1. Merchant elite	65.0	14.8	3.9	34.9	4.3	2.9	3.2
2. Distributor	7.2	51.0	1.9	32.9	9.5	6.0	2.5
3. Skilled craftsman	2.1	2.1	39.0	5.2	5.9	2.3	5.4
4. Weaver	3.1	6.1	.9	76.0	3.2	2.4	2.4
5. Allied cloth worker	2.7	12.5	7.1	22.8	39.0	5.6	4.3
6. Leather worker	1.1	5.0	1.7	10.6	3.4	42.0	3.2
7. Semiskilled craftsman	2.8	4.5	8.6	23.6	5.7	6.8	34.0
Marginal parameter estimates:							
8. α_j	.47	.97	.87	5.29	.73	1.19	.55
9. β_j	5.97	3.67	3.39	1.77	3.70	3.02	9.39
δ_{ij} (above diagonal) and α_j / α_i (below diagonal):*							
10. Merchant elite	. . .	.18 (.04)	.06 (.02)	.15 (.03)	.07 (.03)	.03 (.02)	.06 (.03)
11. Distributor	.48		.04 (.02)	.23 (.05)	.24 (.06)	.12 (.04)	.08 (.03)
12. Skilled craftsman	.54	1.11		.04 (.02)	.17 (.05)	.05 (.03)	.19 (.06)
13. Weaver	.09	.18	.16		.16 (.04)	.09 (.03)	.15 (.04)
14. Allied cloth worker	.64	1.33	1.19	7.25		.11 (.04)	.14 (.05)
15. Leather worker	.39	.82	.73	4.45	.61		.12 (.04)
16. Semiskilled craftsman	.85	1.76	1.58	9.62	1.33	2.16	. . .

* Standard errors, which were obtained through the δ method, are in parentheses.

Consider now the association between pairs of occupational strata (rows 10–16 above the diagonal in table 5). Almost all of the δ_{ij} coefficients are small, indicating strong association and statistical significance. As in period 1, there is no pattern to the associations based on occupational distance. Although exchange mobility continued to be an important component of occupational mobility, mobility—up or down—was defined by economic crisis and the structure of opportunity.

Period 3: 1640–89

The pattern of downward structural mobility evidenced in the years preceding the English civil war continued during the period 1640–89 for sons of the merchant elite, distributors, and textile workers (see table 6). On the other hand, sons of skilled craftsmen and leather workers were forced up into near and adjacent occupational categories, while sons of semiskilled workers continued to gain throughout the urban occupational structure.

While marginal shifts tended to drive sons' mobility in the third period, association between origins and destinations remained extremely strong across strata. Coincident with these observations, however, we note that, relative to the mobility experiences across other periods, the proportion of movers over stayers was very low. As mentioned in the previous section, almost 70% of sons during this period inherited the occupational strata of their fathers, and the distribution of sons relative to fathers fits the QI model quite well. The odds of a son inheriting his father's occupational stratum tell the compelling economic history of this period.

The stratum of weaving occupations, containing the largest number of fathers, absorbed its sons at a rate of almost four times that for mobile sons; this was by far the weakest persistence found in Norwich during period 3. Sons of skilled craftsmen were almost 65 times more likely to become skilled craftsmen than to take occupations outside this stratum. In between these extremes, sons of the merchant elite, of distributors, of allied cloth workers, of leather workers, and of semiskilled craftsmen were all at least 10 times more likely to work within their fathers' strata than to have become economically mobile.

Mobility Over Time: Period Effects

The mobility of sons relative to their fathers' occupational strata has been our focus so far. We have found substantial differences in structural mobility across periods. Here we consider structural and exchange shifts across periods. Table 7 reports period-specific comparisons of the components of structural mobility.

TABLE 6

INTERGENERATIONAL OCCUPATIONAL MOBILITY IN NORWICH, ENGLAND: PERIOD 3, 1640–89

	DESTINATION						
ORIGIN	Merchant Elite	Distributor	Skilled Craftsman	Weaver	Allied Cloth Worker	Leather Worker	Semiskilled Craftsman
Fitted counts under QS (F_{ij}):							
1. Merchant elite	39.0	3.0	1.4	8.9	7.8	2.8	0
2. Distributor	1.9	30.0	.4	8.0	8.0	.6	0
3. Skilled craftsman	1.6	.6	29.0	4.1	1.7	1.4	1.5
4. Weaver	2.0	2.9	.9	53.0	10.2	2.9	2.0
5. Allied cloth worker	1.2	2.0	.3	6.8	48.0	1.1	.6
6. Leather worker	1.2	.4	.6	5.1	2.9	28.0	.9
7. Semiskilled craftsman	0	0	1.5	9.0	4.4	2.1	28.0
Marginal parameter estimates:							
8. α_j	.53	.82	.49	2.24	3.35	1.26	.50
9. β_j	4.58	3.21	4.11	2.59	2.02	2.51	17.46
δ_{ij} (above diagonal) and α_j / α_i (below diagonal):*							
10. Merchant elite	. . .	.07 (.03)	.04 (.03)	.09 (.03)	.07 (.03)	.06 (.03)	.00 (.00)
11. Distributor	.64		.02 (.02)	.12 (.04)	.10 (.04)	.02 (.02)	.00 (.00)
12. Skilled craftsman	1.08	1.69		.05 (.02)	.02 (.01)	.03 (.02)	.05 (.03)
13. Weaver	.23	.37	.22		.17 (.04)	.10 (.04)	.11 (.04)
14. Allied cloth worker	.16	.25	.15	.67		.05 (.03)	.05 (.02)
15. Leather worker	.42	.05	.39	1.78	2.66		.05 (.03)
16. Semiskilled craftsman	1.06	1.66	.98	4.52	6.75	2.54	. . .

* Standard errors, which were obtained through the δ method, are in parentheses.

TABLE 7

OVER-TIME STRUCTURAL MOBILITY SHOWN AS THE ODDS OF A SON MOVING INTO A GIVEN OCCUPATIONAL STRATUM IN PERIOD $t + 1$ RELATIVE TO PERIOD t

	Merchant Elite	Distributor	Skilled Craftsman	Weaver	Allied Cloth Worker	Leather Worker	Semiskilled Craftsman
Upward mobility relative to period 1:							
Period 2	.32	.82	.83	5.63	.90	.93	.98
Period 3	.36	.70	.47	2.38	4.14	.98	.89
Upward mobility relative to period 2:							
Period 3	1.13	.85	.56	.42	4.59	1.06	.91

SOURCE.—See CQS model from table 3 above.

Column 1 reports the odds of sons becoming merchants in period $t + 1$ relative to period t, column 2 the odds of becoming a distributor in period $t + 1$ relative to period t, and so forth. As shown earlier, the alpha parameters yield odds ratios that represent an excess or deficit in the expected proportion of sons in particular occupational strata. In light of this, table 7 reveals the collapse of opportunity in the elite occupational strata (merchants, distributors, and skilled craftsmen), temporal stability of strata at the bottom of the Norwich occupational order, and the growth and contraction of structural opportunity in Norwich's core strata: the weavers and allied cloth workers. Mobility into weaving occupations explodes in the beginning of the 17th century then declines relative to this second-period peak, while expansion of opportunity in the allied cloth sector begins in the middle of the 17th century. Relative to the 16th century, opportunity in the elite strata was closed. Sons in periods 2 and 3 were only one-third as likely as first-period sons to join the merchant elite stratum (.32, .36), and the odds of sons in lower strata becoming distributors or skilled craftsmen were not much better. Finally, we see almost no temporal change in structural opportunity among the leather workers and semiskilled craftsmen for periods 2 and 3 relative to period 1, and for period 3 relative to period 2.

Contrary to the action shown by the temporal measures of structural mobility, measures of exchange (not shown) report a high degree of constancy over time. Although the model that postulates a homogeneity of exchange over time (the CQS + H_8 model in table 3 above) does not provide an adequate fit, comparisons of period 2 and period 3 with period 1, and period 3 with period 2 reveal only three statistically significant differences among exchange parameters in each of the comparisons with period 1 and only one difference between periods 2 and 3.[12] We can now say that the rigidity of strata barriers to exchange persisted across the 140 years preceding England's Industrial Revolution.

DISCUSSION

Recall the crucial cultural, political, economic and structural changes that occurred in English society from 1548 to 1689: the radical civil war, which permanently altered the structure of politics; the rise of Puritanism; the industrialization of the countryside; the emergence of national commodity, food, and labor markets; and the rise of large-scale urban manufacturing. Two classes played a critical role in driving these transitions: the landed elite and the urban middle class. We know less about the middle class than about the elite. This article initiates what we hope will be a new interest in the determinants of urban, commercial, middle-class political action. Our basic theme is that opportunity and the structure of the intergenerational mobility regimes that govern opportunity matter. Our central conclusions are that structural mobility governed opportunity for freemen, that its strength and direction changed over time, and that political action was tied to economic opportunity. Below we identify the structural forces driving mobility over time.

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[12] Model program code and full sets of parameter estimates are available on request from the authors.

Period 1: The Tudor Ascendancy and Upward Mobility

From 1548 to 1589 structural shifts served to drive sons into higher positions than their fathers. At the local level, the expansion of the elite strata was governed by two interrelated processes—the emergence of Norwich as a local center for elite consumption, and the development and subsequent expansion of the "new draperies" textile sector (Allen 1951; Evans 1979; Coleman 1969; Kerridge 1985).[13]

The localism of commodity markets, and especially the luxury markets, made possible an elite culture centered in Norwich which induced the expansion of the strata for elite merchants, distributors, and skilled craftsmen.[14] Elizabethan state policy encouraged thousands of Protestant emigrants seeking to flee religious persecution in the European low countries to establish themselves in England, and it was these men who brought with them the techniques eventually adopted by the Norwich weaving community for producing light woolens (Allen 1951; Allison 1960, 1961). At home, Elizabethan economic policy acted to bolster the fortunes of the sheep farmers and, consequently, to stabilize English wool production levels, despite the fact that with inflationary grain prices and declining wool quality, an investment in crops was often more profitable than an investment in sheep (Bowden 1956, 1967; Wilson 1960). Although both the home and export markets for cloth showed weaknesses throughout the latter half of the 16th century, the new worsted wools continued to be a growth sector for the English economy as a whole (Gould 1971). The Norwich middle class benefited from these national and local developments—the net result being upward structural mobility. During this period Norwich politics were remarkably consensual (Allen

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[13] These were light, fashionable worsted fabrics made mostly from inferior English long-staple wool and suited primarily for the export markets in the Mediterranean.

[14] During this period (1548–89) the English landed gentry largely exchanged "feudal" trappings—manors and retainers—for country estates and conspicuous consumption of luxury goods. The shift from the manor to the market as a determinant of elite status drove the expansion of the luxury goods market.

1951). Political stability is a likely by-product of sustained upward mobility.

Period 2: Instability and Downward Mobility

In the second period the national economy experienced periods of severe depression (1590–1600, 1620–40) and some strength (1600–20). Deep declines in the textile sector were tempered in Norwich, in part because the end of the war with Spain (1604) opened new markets for the new draperies and also because the worsted wool market was strong in the southern European countries, whose economies were more robust than those of northern Europe (Bowden 1967). If the economy of the second period was unstable, so was the court. By 1590, the Tudor political position had weakened considerably. The last decades of Elizabeth's reign were marked by fiscal crisis, price inflation, political intrigue, faction formation, and uncertainty—structural conditions that the Stuart monarchs inherited and subsequently deepened in the pre–civil war period.

The mobility experiences of the Norwich middle class in this period reflect national, rather than local, developments. In Norwich, while the textile sector showed strength, the three elite strata declined markedly. Motivating declines in the elite strata were exogenous changes, mainly the breakdown of local luxury markets resulting from the shift toward London as the center for elite status achievement (Stone 1965). Both processes involve the emergence of a national elite world centered on London, which was established during the late Elizabethan and early Stuart periods. As a result of these larger structural changes, sons of elites experienced downward mobility and sons of lower-stratum fathers experienced blocked ambitions.

Period 3: Status Persistence and Political Conflict

By July 1642, all hopes for peace were gone. For the next three years, England was at war with itself. By August, 2,000 men in Norwich were armed and prepared to defend the city against a potential Royalist attack. In December, the Eastern Association arrested the mayor (a Royalist), and by the end of March 1643 the entire Royalist party in Norwich was crushed (Evans 1979). Whereas other provincial cities became isolated, Norwich politics during the civil war and interregnum were defined by the politics of London and Parliament. In 1645, conflicts between Independents and Presbyterians spilled out from London and organized the bases for conflict in Norwich. Independents in Norwich, and elsewhere, achieved power over Presbyterians, but at a cost. In Norwich, the failure of a Presbyterian coup in 1648 left 200 men dead and led to the purge of all conservative leaders from local offices. By 1649, Independents controlled the city scene, but for the next decade, until the dismissal of the Protectorate, factional strife in Norwich was intense. By 1659, the political elite of the city ruled without, and often against, the popular will.

Even before General Monck arrived in London, 794 citizens of Norwich signed a petition presented to the Rump Parliament which demanded a restored bull Parliament. Popular and elite discontent were marked. A majority of the citizens and the magistracy supported the king's return in 1660, and while waiting for guidance from Monck before acting, were poised to reconstruct the local political scene. In the period from 1660 to 1663, the Commonwealth leaders were purged and a fragile association of restored Royalists and conservative Presbyterians controlled the city. In 1664 this alliance broke down and political conflicts again simmered to the surface. For the next 25 years, these conflicts, defined at the national level, structured local Norwich politics (Evans 1979).

The conflicts, and the parties which gave them meaning, Whig and Tory, Presbyterian and Independent, were over ideal ends and not tangible economic or social reform. Unlike London, where Levellers and other groups were powerful enough to mark political thought, Norwich political conflicts were not driven from below. Nor were they driven by economic processes occurring within the city—the marked decline of markets in textiles and luxury goods. When the political context appears autonomous, we do not expect to find an association between the mobility experiences of men and political outcomes.

But this was not the case in the English civil war of 1640. The leading actors in the interregnum were men who obtained their freedom in 1620–25. We seek to account for their actions on the basis of their mobility experiences as young men. Many of the leading figures were merchant elites, as is shown in table 8. Members of this and other strata experienced blocked mobility in their youth. But the new radicals disproportionately came from strata whose mobility fortunes were constrained by structural processes beyond their control. These men, living in parishes dominated by weavers and allied cloth workers, were collectively radicalized and took active roles in the revolutionary movements of the time (Evans 1979). While data on the political persuasions of most freemen are not available, we note that freemen from the strata most constrained by the mobility regime were more likely to become officeholders during the interregnum than at any other time.

Of the restoration and political battles of the late half of our period we can say less with certainty. The men who played the leading roles after 1664 obtained their freedom of Norwich in the early years of the civil war period. Most men were stayers, and those who moved were as likely

TABLE 8

Odds Ratios for the Association between Stratum and Holding Political Office in Norwich, 1620–89

Strata	Pre–Civil War Period, 1620–39	Revolutionary Period, 1640–59	Restoration Period, 1660–89
Merchant elite	2.7	2.1	2.5
Distributor	1.6	1.5	2.0
Skilled craftsman	.0	.3	.4
Weaver	.6	1.8	.9
Allied cloth worker	.2	1.4	.4
Leather worker	.0	.0	.0
Semiskilled craftsman	.0	.0	.2

Source.—Evans (1979).

to go up as they were to go down (see table 6). The political conflicts of this period were swift and intense. We believe that in the absence of clear and powerful mobility processes, which drive the fortunes of entire strata in a consistent manner, politics will be driven by springs independent of the underlying economic and social processes for which mobility models provide one trace. While it appears counterintuitive to link occupational persistence to intense political conflict, we note that politics are often a focus of human activity precisely when the ends that govern actions are decoupled from underlying economic and social processes.

Radicalization

Beyond analysis of the structure of middle-class opportunity in England from 1548 to 1689, we note two significant implications, the first of which is historical while the second is methodological. A major historical concern is understanding the political outcomes following periods of structurally driven downward occupational mobility. In this case, the consequences of downward mobility in the pre–civil war period (1590–1639) suggest a process of radicalization that led middle-class men in strata experiencing the greatest downward and blocked mobility to disproportionately support Parliament. In the civil war Norwich went for Parliament, but not uniformly and not without struggle between elite factions competing for control of the city's political and administrative offices. Despite evidence of political conflict, which at times led to the mobilization of the whole city, Evans (1979) argues that the revolutionary leaders of Norwich were drawn from the same strata as the traditional elite. Our analysis of his data does not support this conclusion. Rather, we show that radical urban leaders were disproportionately likely to occupy positions in occupational strata below the Norwich social elite.

Following Evans (1979), we define as the political elite men holding office in Norwich. By estimating the odds ratios for the association between stratum and political office we can usefully compare the distribution of officeholders by strata over time.[15] We define the revolutionary period as the years from 1640 to 1659, the pre–civil war period as the years from 1620 to 1639 and the restoration period as the years from 1660 to 1689. We report, in table 8, the odds on becoming an officeholder in each period.

Across all three periods, the merchant elite and distributors' strata were more likely to be represented among the political elite than would be expected from chance, but their contribution decreased during the interregnum. In the revolutionary period, weavers and allied cloth workers were as likely as men in the top two strata to hold office. At no other time were men from strata other than that of the distributors or merchant elite likely to hold political office in Norwich. Note also that the structural mobility ratios for the weaver and allied cloth sectors (reported in rows 13 and 14 of table 5) are generally less than one, indicating a net inflow of sons whose fathers were from higher occupational strata. This collective experience underlay the radicalization of these strata as evidenced by their disproportionate support for Parliament during the civil war. Our disaggregation of the middle class yields occupational strata that experienced differential opportunity; beginning in 1640, these strata differences appear to have played a critical role in structuring radical urban politics during the English civil war.

We identify the political radicalization of strata within an urban middle class as a consequence of sustained, structurally driven downward mobility in the 50 years preceding the English civil war, and we necessarily wonder whether this association holds across context. Radicalization and revolutionary activity, in a context of sharp economic decline following periods of general expansion, is not an uncommon historical experience (Davies 1962; Goldstone 1982, 1991). Our findings suggest a similar pattern, but one organized over a long period. Since mobility is perceived as individual attainment, only when structural processes completely govern a mobility regime for sustained periods of time will individuals interpret opportunity as socially organized. When this occurs, we expect an association between a mobility regime and politics. It follows that other instances of popular radicalization may be associated with structurally driven downward mobility. We note that the middle class has consistently played an important role in major historical revolutions (Davies 1963; Moore 1966; Skocpol 1979). Perhaps more important than the structure of interclass transfer are temporal shifts in the opportunity structure within the middle class that induce radical action.

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[15] The odds ratios are formed as the ratio of two ratios; the number of political officeholders from each stratum over the total number of officeholders, and the number of freemen per stratum (Evans 1979) over the total number of freemen in the given interval (Millican 1934). We approximate the number of freemen during the 1620–39 interval by counting sons in the Norwich register from 1548–89, the number of freemen in 1640–59 by counting fathers in the register from 1590–1639, and the number of freemen in the 1660–89 interval by counting sons in the register during the 1590–1639 period. The odds on being an officer, reported in table 8, are thus standardized by strata counts.

Implications

In this article we show that a partition of the structural and exchange components of English middle-class mobility regimes from 1548 to 1689 yields insight into political and economic processes not otherwise observed. From 1548 to 1589, middle-class men were propelled into higher-level occupations than their fathers as the result of structural processes that generated elite vacancies. It is not surprising to find that collective prosperity is associated with political stability. From 1589 to 1639, structural processes reversed themselves, and, for two generations, sons faced obstacles to upward mobility. Most sons fared worse than their fathers. The cohort of men facing blocked aspirations were those who, in 1640, participated in a violent civil war. Men from strata experiencing blocked mobility were more likely during the revolutionary period than at any time before or after to become leaders in Norwich. We associate downward mobility with radicalization of this cohort and argue that there is an elective affinity between mobility experiences of men and subsequent political action.

The equation of mobility processes and political outcomes is not uniform nor does it necessarily hold in all periods. Politics of elites pursuing ideal ends in a context defined by the absence of structural mobility processes—as in period 3—are not expected to be driven by a mobility regime. Only where one finds consistently strong structural processes organized over a lengthy period of time can one expect an association between a mobility model and political outcomes.

APPENDIX

Estimation of Free Population in Norwich, 1548–1689

The number of freemen in Norwich at any given time can be approximated from the number of new admissions before that time. Evans (1979) sets the average age of becoming a freeman in our period 2 at just over 27 years; he sets that figure at 25.5 years for our period 3. If we assume a life expectancy of 56 years for the first group and 52.5 years for the second group (Wrigley 1968; Laslett 1965), the free population can be estimated by surviving forward new admissions from appropriate model life tables (Coale and Demeny 1983).

Our method employed five-year survival probabilities from the West model life table levels 3 (before 1640) and 5 (after 1640) for males. These tables provided the closest match to additional expectation of life (29 years for those who obtained freedom of the city before 1640 and 27 years for those who became freemen after 1640) for newly admitted freemen. The method does require a stable population assumption, but we feel the procedure produces acceptable estimates. Essentially, we "survive" forward the cohorts of admitted freemen from six earlier decades (say, cohorts from times *t*-1 to *t*-6) using the survival probabilities from the model life tables to yield a free population at time *t*. Thus the estimated free population at 1680, for instance, is generated from the 886 entries from 1670–79 × .91 × .89 (the survival probabilities from ages 25–29 and 30–34 West model life table for males level 3) + 886 entries from 1660–69 × .91 × .89 × .87 × .85 (survival probabilities ages 25–29, 30–34, 35–39, 40–44) + 642 entries from 1650–59 × .91 × .89 × .87 × .85 × .82 × .79 + 620 entries from 1640–49 × .91 × .89 × .87 × .85 × .82 × .79 × .73 × .66 + 654 entries from 1630–39 × .92 × .91 × .89 × .87 × .85 × .81 × .77 × .69 × .61 × .49 (the survival probabilities from ages 25–29, . . . , 70–74 West model life table for

TABLE A1

ADMISSIONS TO FREEDOM AND ESTIMATED FREE POPULATIONS IN NORWICH

Years	Admissions to Freedom	Total Free Population	Ratio
1580–89	502	974	.52
1590–99	434	1,059	.41
1600–09	456	1,049	.43
1610–19	630	1,039	.61
1620–29	672	1,175	.57
1630–39	654	1,317	.50
1640–49	620	1,407	.44
1650–59	642	1,407	.46
1660–69	886	1,386	.64
1670–79	886	1,544	.57
1680–89	559	1,657	.34

SOURCES.—Evans (1979); Coale and Demeny (1983).

males level 5) + 672 entries from 1620–29 × .92 × .91 × .89 × .87 × .85 × .81 × .77 × .69 × .61 × .49 × .33 × .22.

Table A1 reports the number of admissions to freedom and the estimated number of freemen by decade from 1580 to 1689, using available counts of admissions from the Norwich register (Millican 1934; Evans 1979). Note that while new admissions report fluctuations between decades caused by economic and epidemiological conditions, the ratio of admissions to free population at any given time was near .50, with the notable exception being the decade 1680–89 during which the ratio falls to .34. The premature admission of over 300 men for political reasons following the Restoration—note the jump to 886 new admissions between 1660–69 and 1670–79—accounts for this unusual decline (Evans 1979).

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Payment Systems and the Structure of Inequality: Conceptual Issues and an Analysis of Salespersons in Department Stores[1]

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Output-related payment systems—such as commissions and piece rates—are common in many jobs. Their central feature is that the wage of an employee is tied directly to her current performance. Focusing on salespersons in department stores, the author considers wage differences under three payment systems: a straight salary, a straight commission, and a salary plus commission. Four conceptions of wage inequalities under the three systems are contrasted: a risk-sharing, a bargaining, a sorting, and a social status interpretation. The empirical analyses compare the pay schemes and wages of approximately 63,000 salespersons in 178 department stores in 1981.

I. INTRODUCTION

Payment systems based on piece rates and production bonuses are common in many jobs. For example, in the United States, about 20% of the blue-collar work force receive part of their pay from piece rates and

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production bonuses (see Cox 1971, table 1, p. 54). In Britain, about 40% of the workers in the manufacturing industries are remunerated by results (see U.K. Department of Labor 1977, tables 79, 81, pp. C39, C44). And, as is well known, in some Eastern European countries, as much as 80% of the work force is paid in this manner (see Kirsch 1972, chap. 2; International Labor Office 1984, pp. 144–45, esp. table 12).

Sociologists have examined the operation of such payment systems, which I will refer to as direct incentive structures, for more than half a century, resulting in a series of important studies (see, e.g., Mathewson [1931] 1969; Roethlisberger and Dickson 1939; Roy 1953; Whyte 1955; Lupton 1963; Burawoy 1979; and for reviews of the studies, see Form [1985, pp. 12–16] and Edwards [1986, chap. 6]). The foci of these investigations have been how direct incentive structures induce workers to expend more effort, but, sometimes, also to restrict output; how they allocate uncertainties at the point of production to the workers; and, in particular, how they give rise to social rewards and informal relationships that regulate the behavior and interaction of workers (see, e.g., Roy 1953; see also Frank 1985, chap. 5).

None of these studies addresses what, at least from the workers' viewpoint, perhaps is the most important feature of direct incentive structures: their effects on pay itself. The current study attempts to partly fill this gap.

In addressing the relationship between payment systems and pay, I will discuss and contrast four theoretical viewpoints. According to the first viewpoint, output-related payment systems serve two basic functions: they provide workers with incentives to expend more effort and they allocate to the workers some of the risks at the point of production (see, e.g., Stiglitz 1975).[2] Under this interpretation, part of the effect of direct incentive structures on pay may therefore be due to differences in performances and part of the effect to compensation for risk-bearing (see Pencavel 1977; Seiler 1984). This is the interpretation found in economic studies of uncertainty and asymmetrical information, sometimes called agency theory. I refer to this as the *risk-sharing* interpretation of incentive schemes.

According to the second viewpoint, wage inequalities under various payment schemes need not primarily reflect different risk-sharing arrangements, but may rather reflect differences in the division of surplus

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[2] Note that the two functions are conceptually distinct, but are here, as in most other treatments, discussed together and are referred to as one rather than two viewpoints.

from the relationship between workers and managers. This viewpoint does not deny the importance of performance and risk-sharing, but stresses that the additional social process of bargaining may be operating. The interpretation is drawn partly from historical and partly from sociological research on payment systems. I refer to this as the *bargaining* interpretation of the payment schemes.

The third viewpoint purports that direct incentive structures neither induce workers to expend more effort nor allocate risks at the point of production to the workers. Instead, they primarily serve to sort high- and low-productivity workers into incentive- and time-rated jobs (see Lazear 1986). Incentive-rated employees therefore earn more because they are better workers than time-rated employees. This I refer to as the *sorting* interpretation of direct incentive structures.

The fourth viewpoint claims that wage inequalities between different jobs and payment systems reflect differences in social status among the workers occupying the positions. Workers in high-paid incentive jobs are there due to privileged social ties, and their high pay reflects their social status (Talbert and Bose 1977). I refer to this as the *social status* interpretation of the payment systems.

With these viewpoints as the background, I attempt to accomplish two tasks in this article. The first is a straightforward empirical task: Do intraorganizational contractual arrangements—in this case payment systems—matter for the distribution of wages? This task is of interest in itself for the factual information it provides. It has not been addressed by the numerous empirical and conceptual studies of payment systems.

The second task is more ambitious: I attempt to discuss and contrast the four viewpoints. The sorting and status interpretations are considered only in the conceptual analysis. There, I conclude, partly on the basis of data and partly on the basis of conceptual arguments, that neither interpretation carries sufficient force in the social setting considered here to be in focus in the empirical analysis. The two other interpretations of incentive schemes—the risk-sharing and the bargaining interpretations—are contrasted in both the conceptual and the empirical analyses. Each implies different patterns of wage inequalities, and these can be confronted with the data at hand. I stress here that neither interpretation is tested directly. There are no direct measures of risks, of effort, or of bargaining power. Instead, what is tested is the pattern of wage differences implied by the two conceptions.

The empirical focus of the study is on the payment systems and wages of salespersons in department stores. In many respects the case of department stores is almost ideal for the study of the effects of incentive structures. First, there exists a simple measure of output for salespersons, the amount of sales. Second, there are few interdependencies among employees and little teamwork. These features, observable output, measurable output, and no externalities at the point of production make individual incentive schemes feasible and easily applicable. Finally, salespersons in department stores are remunerated primarily by one of three methods of pay: straight salary, straight commission, or salary plus commission. I shall evaluate how the three payment systems give rise to differences in rewards, using data from a specific industry on one of the largest occupational groups in the United States.[3]

A. Relationship to the Social Stratification Literature

Before proceeding to the tasks at hand it may be instructive to examine how the current approach relates to dominant themes in social stratification research. Most would agree that the main conception in recent sociological research of the mechanisms behind inequality is that rewards are attached primarily to jobs or positions as opposed to performances within positions (see Stinchcombe 1974, chaps. 5–6; Kalleberg and Sørensen 1979; Wright 1979, chap. 3; Granovetter 1981; Sørensen 1983; Baron 1984). The driving idea, in extreme form, was stated by Thurow (1975, pp. 85–88), who claimed that a person's productivity is determined by the job. A worker's main strategy for increasing his or her rewards, therefore, is to move from positions with lower rewards to positions with higher ones. The focus of research has correspondingly been on the positional determinants of rewards and on the movements of people between positions (see Sørensen and Kalleberg 1981; Rosenbaum 1984, chap. 1; Spilerman 1977, 1986; Granovetter 1986; Petersen, Spilerman, and Dahl 1989). The complementary conception suggested here is that positions sometimes allow for considerable variability in job performances and hence may provide employees with scope to influence rewards. In such cases we need to study the incentive structures used to generate performances and their effects on rewards (for conceptual steps in this direction, see Jacobs [1981] and Granovetter and Tilly [1989]).

The remainder of the article is organized as follows. The next section discusses the four viewpoints on the relationship between payment systems and pay. Sections III and IV present the data and the empirical results, based on the wages of approximately 63,000 nonsupervisory salespersons in 178 department stores. Concluding remarks follow.

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[3] According to the 1/100 Public Use Microdata Samples from the 1980 census (see U.S. Department of Commerce 1983), there were, counting both part- and full-time employees, roughly 7.5 million salesworkers in the United States in 1980, nearly 7% of the labor force, of whom about 12% were employed in department stores.

II. THE FOUR VIEWPOINTS

In this section I address, in each of four subsections, the four viewpoints on the relationship between payment systems and pay. But first I describe in more detail the three payment systems.

The payment systems considered are a straight salary, a straight commission, and a salary plus commission. Let z_i denote the sales of employee i and let w_i denote her wage. The three payment systems are then given by:

$$w_i = w_s \qquad \text{for all } z_i, \tag{1}$$

$$\begin{aligned} w_i &= \beta_c(z_i - z_c) && \text{if } z_i \geq z_c, \\ w_i &= 0 && \text{otherwise} \end{aligned} \tag{2}$$

$$\begin{aligned} w_i &= w_{s+c} + \beta_{s+c}(z_i - z_{s+c}) && \text{if } z_i \geq z_{s+c}, \\ w_i &= w_{s+c} && \text{otherwise,} \end{aligned} \tag{3}$$

where equations (1)–(3) correspond to straight salary, straight commission, and salary plus commission, respectively. Further, w_s and w_{s+c} are the salary and baseline salary under a straight salary and salary plus commission, respectively; β_c and β_{s+c} are the rates of commission under straight commission and salary plus commission, respectively; and z_c and z_{s+c} are the amount of sales that must be made before the worker starts earning the commission, under straight commission and salary plus commission, respectively.[4] The salary, either w_s or w_{s+c}, will be determined in part by competition between firms in the external labor market and in part by the bargaining power of the parties. In the case of an output-related scheme, the bargaining power of the parties may determine both whether a salary is received and its amount. The relationships in (1)–(3) are illustrated in figure 1. The equations and the figure show that under a straight salary there is no relationship between performance and pay, that under a straight commission the pay depends exclusively on output z_i, and that whereas under a salary plus commission one part of the pay is independent of output, the other part depends on output.[5]

Before comparing the four viewpoints, one preliminary issue needs mentioning. The approach taken here is partial. It asks, *Given* that a certain payment system has been agreed upon, *what* are its consequences for the level and distribution of wages? These consequences may, however, in turn lead to bargaining over and alterations of the payment systems themselves. This equally interesting issue of the endogeneity of

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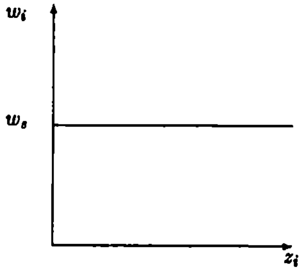
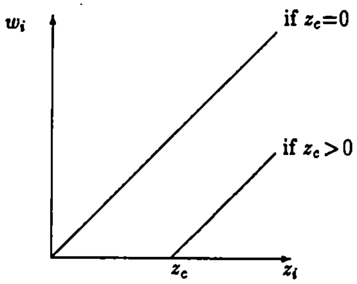
[4] The commissions, β_c and β_{s+c}, will typically vary with the line of sales and the product sold. But that is not at issue here.

[5] The system described in (2) is equivalent to the fermage (cash renting) system described by Stinchcombe (1983, p. 180).

(a) Straight salary



(b) Straight commission



(c) Salary plus commission

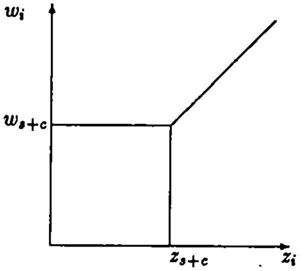


FIG. 1.—Graphic illustration of the material reward schemes in equations (1) to (3). The symbols in the figure denote the following: z_i is the amount of sales and w_i is the wage paid as function of amount of sales; w_s and w_{s+c} are the salary and baseline salary under respectively a straight salary and salary plus commission; and z_c and z_{s+c} are the amount of sales that must be made before the worker starts earning the commission, under, respectively, straight commission and salary plus commission.

payment systems is not addressed below. It is legitimate, nevertheless, from the viewpoint of cross-sectional analysis, to treat the payment system as exogenous relative to the wages it gives rise to in the cross section. Were one to expand the time frame for the analysis, the payment system itself must be considered endogenous. Then, in order to understand the determinants and evolution of payment systems, a temporal and historical approach is required.[6]

A. The Risk-Sharing Interpretation

The core argument in the risk-sharing interpretation is that output-related payment systems serve first to induce workers to expend more effort, and second, in situations where output, for a given amount of effort, is uncertain, to allocate to the workers some or all of the risks at the point of production. Workers paid on an output-related scheme will therefore earn more on the average than workers paid a straight salary, in part because they work harder and in part because they are compensated for the extra risks they carry.

Considerations about effort and uncertainty are clearly relevant in sales occupations. The two output-related payment schemes, straight commission and salary plus commission, are commonly seen as inducements to expending effort, which in turn may lead to higher sales and hence higher earnings (see Dalrymple 1985, p. 351; Churchill, Ford, and Walker 1985, p. 464). From the workers' viewpoint, the output-related schemes are obviously riskier than a straight salary; earnings are not as safe.

A quantitative study corroborates both claims, about effort and about risks. Barry and Henry (1981, p. 32, table 6.4) report that among managers of salesworkers, the main perceived disadvantage of a straight salary is that it leads to a lack of motivation, while its main perceived advantage is security of income. For salary plus commission and straight commission the main perceived advantages are their positive effects on efforts and earnings (see, respectively, p. 30, table 6.3, and pp. 33–34, tables 6.5–6.6).[7] The weak motivation properties of a straight salary might be alleviated by supervision, regular salary increases based on performance, and promotion ladders (see Noyelle 1987, chap. 3). However, these remedies, in particular steady salary increases and supervision, are widely believed to be inferior in terms of motivation to the two direct incentive schemes (see Dalrymple 1985, p. 351; Churchill et al. 1985, pp. 465, 480–81).

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[6] Some representative examples of historical research on payment systems are Stieber (1959), Aitken (1960), Stearns (1975, chap. 6 and esp. statistical appendices), Montgomery (1979, chaps. 2 and 5), Edwards (1979, chaps. 1 and 7), Lichtenstein (1982, chap. 8), Schatz (1983, pp. 139, 150–60), Elbaum (1984), Lewchuk (1987, esp. chap. 4), Haydu (1988, chap. 2), and Schneirov and Suhrbur (1988).

[7] Weaver (1969), in a study of salespersons in retail trade, showed that workers paid a straight salary sell less on the average than workers paid on some type of commission system.

One prediction from this line of reasoning is therefore that workers paid on one of the two output-related payment schemes, straight commission or salary plus commission, will, on the average, earn higher wages than workers paid a straight salary.

The next task is to compare the two output-related reward structures. The existence in department stores of two distinct output-related payment systems, straight commission and salary plus commission, can be given at least two separate interpretations, with different implications for the distribution of wages. In this subsection, I report the risk-sharing interpretation; in the next subsection I report the bargaining interpretation.

Under a salary plus commission, the workers are provided some security against bad times, namely the baseline salary, whereas straight-commission workers, in contrast, carry all the uncertainties of both bad and good times (see, e.g., Dalrymple 1985, p. 353; Churchill et al. 1985, pp. 466–67). If the two payment systems reflect differences in risk-sharing arrangements, one would surmise that salary-plus-commission workers trade increased security for lower expected wages. As Moynahan (1980, p. 77) asserts: "Sales jobs that have considerable compensation risk must also offer high reward opportunities." The implication is, therefore, that salary-plus-commission workers on the average will earn less than straight-commission workers, because the latter are compensated in expected wages for the extra risks they carry.[8]

In summary, the central prediction from the risk-sharing interpretation is this:

HYPOTHESIS R1.—Workers paid a straight commission will, on the average, earn more than workers paid a salary plus commission who in turn will on the average earn more than workers paid a straight salary.

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[8] Unfortunately, it is difficult to test whether one payment scheme is riskier than another. This is so because it is onerous to develop measures of the riskiness of the payment systems—i.e., measures that can be related to variables we observe. One reason for this is that the observed distributions of wages are influenced by the actions taken by the employees, and those actions may have been chosen to reduce the riskiness of wages in situations where the amount of sales itself is very risky. An often used measure of risks, the variance, need not be a good measure. It works only when the risky variable is normally distributed or when preferences over money are quadratic (Hey 1979, p. 52). In this article, wages need not be normally distributed, for the simple fact that they are truncated from below in the case of a salary plus commission. Quadratic preferences over money is not an appealing or even acceptable hypothesis, as discussed in Arrow (1965, pp. 35–36).

Under the risk-sharing interpretation, the three payment systems are equally desirable from the workers' viewpoint, provided workers do not vary much in their attitudes towards risks. The schemes only present different trade-offs in terms of wages, effort, and risk-sharing, but none of the trade-offs is superior. Therefore, there are no obstacles to using each of the three schemes side-by-side within the same store and occupation.[9] This will particularly be the case when there is opportunity for workers to move between schemes or choose the scheme by which to be remunerated. The second prediction from the risk-sharing interpretation is therefore:

HYPOTHESIS R2.—Within an occupation, the within-store variation in the use of the three payment systems—straight salary, straight commission, and salary plus commission—equals the between-store variation in the use of the three payment systems. That is, there are no obstacles to combining two or more payment systems within the same store and occupation.[10]

B. The Bargaining Interpretation

According to the bargaining interpretation, the wage differences between a straight salary, a straight commission, and a salary plus commission may in some measure reflect compensation for effort and risk-sharing. But, they may also reflect different divisions of the surplus from the relationship between workers and management.

The bargaining and the risk-sharing interpretations make the same prediction regarding wage differences between a straight salary and the two output-related payment systems: Straight salary workers earn the lowest average wages. However, in the comparison of salary-plus-commission and straight-commission workers, the two interpretations make different predictions.

Several studies report how incentive workers in many places and in many periods have fought to gain and to raise baseline salaries without lowering the incentive part.[11] Historically, the use of incentive schemes with no baseline salary, such as straight piece-rate schemes or straight commissions, was predominant in the heyday of scientific management. Since then, their spread has declined, and baseline salaries or guarantees have been added, in part as a result of pressures from workers.[12] These are struggles over the division of surplus between workers and employers, rather than over shifting the risks of the employment relationship or trading earnings for increased security. When workers succeed in procuring and eventually raising the baseline salary without lowering the incentive part, they achieve both increased security and higher expected wages. They have won a larger part of the surplus from the relationship; their bargaining power has enabled them to demand the baseline salary in addition to the commission. This is the bargaining interpretation of the two output-related schemes. The implication is that salary-plus-commission workers on the average earn more than straight-commission workers, even though they neither work harder nor carry more risks.[13]

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[9] A complication abstracted from here is that transactions costs (see Williamson 1985) might increase with the number of schemes used side by side, which might favor the use of only one scheme.

[10] Hypothesis R2 follows also when workers vary in their attitudes toward risks, because the three schemes could then serve as self-selection mechanisms where the more risk-averse workers choose to be paid by a salary or salary plus commission.

[11] For some commentaries on this issue, see Stieber (1959, pp. 56, 61), Burawoy (1979, chap. 4), Montgomery (1979, p. 130), Schatz (1983, pp. 139, 150–60), Jacoby (1985, p. 152), and on salespersons, see Barry and Henry (1981, chap. 4) and Peck (1982, p. 16). In some industries, state governments have even interfered through legislation to

Barry and Henry (1981, p. 26, table 6.1) report that managers using a straight commission are more satisfied with their pay scheme than managers using a salary plus commission, who in turn are more satisfied than those using a straight salary. From the employer's point of view, therefore, straight commission appears to be the scheme leading to the highest degree of satisfaction, as also reported in Dauner (1970, p. 155, table 62). I am aware of no comparable data covering the workers' points of view, but historical evidence shows that workers have fought to gain and raise baseline salaries. It is known, however, that salesworkers tend to favor one of the two incentive schemes over a straight salary (see Belcher 1973, pp. 505–7; see also Bluestone et al. 1981, pp. 109–10). When I conducted some informal interviews with salespersons in department stores in the Boston area in 1987, a salesperson stated, "I used to

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provide piece-rate workers with baseline salaries, as in the California canning industry in 1913–37 (Zavella 1987, p. 38).

[12] On this issue in general, see International Labor Office (1984, p. 86), Schatz (1983, pp. 139, 150–60), Jacoby (1985, p. 152), Stieber (1959, chap. 10), Edwards and Scullion (1982, p. 176); and for historical evidence on salespersons specifically, see Peck (1982, p. 16).

[13] It should be noted that there is a specific institutional arrangement for many salespersons in department stores and elsewhere that alleviates some of the risks of a straight commission. Often employees face a commission with a draw (see, e.g., Dessler 1984, p. 384; Dalrymple 1985, p. 352; Churchill et al. 1985, pp. 466–67; Hill 1987, p. 363). In bad weeks and months the employee is allowed to draw wages against future commissions. These wages are then repaid when the good weeks and months turn up. In this manner the income stream is smoothed over time. The mechanism is much like the tax rules for self-employed persons in many countries: they are allowed to average income over a period of 3–5 years so as to smooth the income stream and protect themselves from the vagaries of the market.

feel pity for the commission salespeople. How could they live with all that uncertainty? Now I know better and I envy them."

If the schemes reflect differences in risk-sharing arrangements, one would predict that the proportions of managers who are satisfied with their scheme would be the same for the two output-related schemes. If, instead, they reflect differences in the division of surplus from the relationship, caused, for example, by differences in bargaining power, one would expect that more managers would be satisfied under a straight commission than under a salary plus commission, as shown by Barry and Henry (1981).

The central prediction following from the bargaining interpretation is given below.

HYPOTHESIS B1.—Salary-plus-commission workers will, on the average, earn more than straight-commission workers who in turn on the average will earn more than straight-salary workers.

Turning then to the issue of the use of the various payment schemes in the same store and occupation, the following can be noted. Substantial differences in bargaining power between employees in the same store and occupation (or line of sale) are not likely, as bargaining power in these types of occupations probably rests as much in the organization and the position within the organization as with the individual employee. That is, bargaining power is apt to be a structural feature as opposed to a feature residing in individuals. We should expect to see differences in bargaining power between establishments and possibly between occupations or lines of sale within an establishment. But there should be few differences in bargaining power between individuals in an occupation within the same establishment. If this is the case and if hypothesis B1 holds, it would be infeasible to use simultaneously a straight commission and a salary plus commission in the same occupation and store, since the former workers would object to an arrangement where the latter on the average earn more and at the same time face lower risks. Under the bargaining interpretation, therefore, the two output-related payment schemes, straight commission and salary plus commission, are not likely to be used side-by-side in the same store and occupation. To the extent that they are, their joint use must have come about through a different process than bargaining, for example, through a need to have more than one risk-sharing arrangement. The second hypothesis following from the bargaining interpretation is:

HYPOTHESIS B2.—Within an occupation, there is less within- than between-store variation in the use of a straight commission and a salary plus commission.

It is difficult to make any prediction regarding whether a straight salary will be used side-by-side with one of the two output-related schemes.

C. The Sorting Interpretation

In the accounts just given, incentives induce workers to expend more effort and they allocate risks at the point of production to the workers. Lazear (1986) proposed an alternative account, that incentives primarily serve a sorting function, where low-productivity workers choose to be paid by the hour and high-productivity workers by the piece (see also Maskin and Riley 1984, pp. 173–74; Lawler 1981, p. 41). In this theory, workers differ in their productivities, but these cannot be observed by the employer. Output can be observed only by incurring a cost, which the worker must carry if he or she elects to be paid according to output. When the cost of observing output is sufficiently high, low-productivity workers will choose the time-rated jobs whereas high-productivity workers will choose the piece-rate jobs. In this scenario, a piece-rate scheme sorts workers, but has no effects on effort levels or individual productivities. The driving mechanism is that it is costly to observe output and that each worker, but not the employer, knows his or her own productivity.

In the case of salespersons in department stores, output, that is, the amount of sales, appears to be observable at low costs, owing, among other things, to electronic cash registers (see Noyelle 1987, chap. 3, esp. pp. 35, 39–40, 47–48; see also Bluestone et al. 1981, pp. 113–14). The issue raised by Lazear is therefore probably not pressing, though of possible importance in other contexts.[14]

Some insight on sorting of employees can be gained by juxtaposing how various groups of salesworkers in department stores differ in terms of observable characteristics such as education. If education is positively correlated with the unobservable productivities, one would expect to find considerable differences in educational attainment between the occupations, with the higher educated employees in occupations where incentives are more widespread and where earnings are high.

Table 1 gives the means and standard deviations (SDs) of education for seven groups of salesworkers in department stores and for seven

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[14] A more plausible line of reasoning in this context is that in jobs where output is costly to measure, salaries rather than output-related payment systems will prevail, whereas in jobs where output is less costly to observe, commission systems will prevail, as Eisenhardt (1985) shows in the case of retail salesjobs. According to this alternative line of reasoning, the employer chooses to reward jobs either by the hour or by results, but not by both. The costs of observing, measuring, and assessing the value of output will determine the choice. In Lazear's framework, in contrast, the worker in each job can choose to be paid by the hour or by results.

TABLE 1

AVERAGES AND STANDARD DEVIATIONS OF YEARS OF SCHOOLING BY OCCUPATION FOR SALESWORKERS

Occupation	Education (in years)	SD	*N*
Salesworkers in department stores:			
Apparel	12.0	2.0	277
Shoes	12.2	2.6	79
Furniture and home furnishings	12.7	2.6	61
Radio, TV, hi-fi, and appliances	12.9	2.1	60
Hardware and building supplies	12.9	1.5	26
Parts	11.8	2.0	41
Other commodities	12.0	2.0	4,262
Salesworkers in other industries:			
Insurance	13.8	2.3	3,962
Real estate	13.8	2.3	3,638
Securities and financial services	15.3	2.4	926
Advertising and related sales occupations	14.2	2.2	877
Sales occupations, other businesses	13.8	2.4	2,784
Sales engineers, except retail	15.2	1.9	365
Sales representatives, except retail	13.6	2.4	9,929

SOURCE.—Bureau of the Census' 1/100 Public Use Microdata Samples from the 1980 Census of the Population and Housing (see U.S. Department of Commerce 1983).

NOTE.—*N*'s pertain to employed persons (as opposed to self-employed) who, by the date of the interview, had completed their education.

groups of salesworkers in other industries. These means and SDs were computed from the 1/100 Public Use Microdata Samples from the 1980 Census of the Population and Housing (see U.S. Department of Commerce 1983). While the differences in the use of incentive schemes between the occupations are vast (see table 2, sec. IV, below), there are, in department stores, only negligible differences in mean education between the occupations (see table 1).[15] In terms of the observable characteristic education, very little sorting goes on. The salesworkers in the other industries are better educated and the educational differences between the occupations are slightly more marked, indicating that some sorting of salesworkers occurs between department stores and the other industries. In many respects, salesworkers in department stores, in contrast to industrial salesworkers, are quite "proletarian," with comparatively low and compressed educational attainment and they rank lowest among nine groups of salesworkers in median weekly earnings for full-time employees (see Rytina 1982, p. 27, table 1).

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[15] The overlap in occupations between tables 1 and 2 is only partial. The census data provide less detailed occupational information than the Industry Wage Survey data used in Sec. IV. Nevertheless, the results are suggestive with respect to the issues discussed.

For the reasons discussed above—observable output at low cost and little correlation between sales occupation and education—I conclude that Lazear's interpretation does not carry much force in the present context.

D. The Social Status Interpretation

The final interpretation considered claims that workers are allocated into the high-paying straight-commission and salary-plus-commission jobs according to their social status. The idea is that high social status leads to privileged social ties which in turn allow the workers to request the better-paid commission jobs (see Talbert and Bose 1977, p. 409). The high pay in these positions reflects the high status of their incumbents.

This interpretation appears to carry little force in the present context for four reasons (see also Granovetter 1981, p. 39). First, in department stores, output is easily observable, measurable, and assessable in terms of its value (see sec. C above). Therefore, performance-related rewards, rather than status-related rewards, should have a high probability of succeeding (see Pfeffer 1977, esp. p. 556).

Second, department stores are a highly competitive industry (Bluestone et al. 1981, chap. 3). Hence, there is probably little leeway for employers to reward workers for characteristics other than performance. As an executive of a department-store chain said, "The whole subject of productivity is a very, very major one" (cited from Bluestone et al. 1981, p. 119). Had the salesperson had little effect on the amount of sales, rewarding employees for status rather than performance would have made some sense. The employer's objective might then be to project a certain image of the occupation or line of sales.

Third, a correlation between education and occupation is almost nonexistent in department stores (see table 1). As is well known, the best predictor of social status is education (see, e.g., Blau and Duncan 1967). If social status were an important factor, one would expect to find a correlation between occupation and education. Furthermore, as shown in Petersen (1989, table 2), education has almost no effect on pay among salespersons in department stores. In fact, the effect is negative. Thus, pay appears to be caused by other factors than social status.

Fourth, there is no evidence that high-status persons are better at selling the big-ticket items in commission jobs than low-status persons (see Churchill et al. 1985, p. 362). Hence, contrary to Talbert and Bose's (1977, p. 409) suggestion, there is no rational reason for employers to allocate high-status persons to the high-paying commission jobs.

For these reasons, I conclude that the social status interpretation carries little force in the present context, though it may very well be important in other settings where performance measures are harder to obtain (see Pfeffer 1977).

III. DATA AND METHODS

The data used are taken from the Bureau of Labor Statistics' (henceforth BLS) *Industry Wage Survey* of department stores in June 1981 (see U.S. Department of Labor 1982). The population for the survey consisted of approximately 264 retail stores in 19 metropolitan areas in the United States. These retail stores were selling general lines of merchandise in separate sections or departments and belong to industry group 531 as defined in the *Standard Industrial Classification Manual* (U.S. Executive Office of the President 1987, p. 317). A store was defined as all outlets of a company in the metropolitan area sampled (see U.S. Department of Labor 1982, p. 85). Of the 264 stores in the population, 199 were sampled. Information is used here about the 178 stores and 62,874 salespersons for which complete data were available.

For each store, information about store-level characteristics and about individual employees within the store were collected. We know the number of employees, the regional location, and whether it is a discount or a regular store. For each nonsupervisory worker, we know her occupation, whether she is a full- or part-time worker, the method of pay (salary, straight commission, or salary plus commission), and the hourly earnings. Eleven sales occupations were surveyed (as described in U.S. Department of Labor [1982, pp. 89–91]). Unfortunately, no information is available on collective agreements or on sex, race, education, age, or experience. The omitted variable bias accruing from these factors is discussed in Section IV B below. Information on wages relates to straight hourly earnings during regular hours, excluding overtime and work on weekends, holidays, and late shifts. For salaried employees the wages refer to mid-June 1981 payroll. For workers paid a straight commission or salary plus commission, wages were averaged, when feasible, over a 12-month period ending as near as possible to May 1981 (see U.S. Department of Labor 1982, pp. 85–86). The average wages for these two groups, therefore, probably reflect well their expected wages since short-term fluctuations in wages due to large or small sales are smoothed out, when possible, by the averaging over a 12-month period. For salary-plus-commission workers, the wages then consist of the base pay plus the incentive earnings. For the straight-commission workers, the wages consist exclusively of the commissions earned. Excluded from the collection of wage data were apprentices, learners, beginners, trainees, handicapped, temporary, and probationary workers (see U.S. Department of Labor 1982, p. 89).

Although these data have some serious deficiencies, notably the few worker-level characteristics measured, they also have some unique features that are not likely to be available or replicated in other surveys. I will mention three such features.

First, in the industry studied, the data give information about 178 stores and all of the salesworkers within each of 11 sales occupations within these stores. This allows one to study intra- versus interestablishment processes. No other data set available or likely to be collected in the near future is so extensive in this regard. Usually, only a few workers are sampled within each establishment, making intra- versus interestablishment comparisons infeasible: one cannot separate the processes that occur within from those that occur between establishments. Or, if many workers are sampled within each establishment, only few establishments are sampled, possibly making the data set specific to features of a few establishments..

Second, the wage data are unusually good. Most survey data record only monthly or annual earnings. In those cases one needs to impute wages from weeks worked and usual hours worked per week in the period earnings cover (see Stolzenberg 1975, pp. 651–52). This is likely to lead to some error. The wage data in the BLS survey come from establishment records, are not subject to recall error, and ought to be extraordinarily reliable.

Third, few surveys obtain as detailed information on occupational characteristics as the one used here. In the current case, the information corresponds to nine digits in the *Dictionary of Occupational Titles* (*DOT*) (U.S. Department of Labor 1977). The intraoccupational variation in personal characteristics is therefore likely to be limited, as demonstrated in the case of education in table 1.

The main method used in the statistical analysis is linear regression analysis of the logarithm of the hourly wage on store and employee-level characteristics. The semilogarithmic form of the wage equation is chosen for its ease of interpretation: a coefficient is interpretable (roughly) as the percentage change in the dependent variable resulting from a unit increase in the independent variable. We need not consider the general wage level prevailing in 1981 or the average level of wages within an occupation in order to make social sense of the results (for further discussion, see Petersen [1989, sec. 3]). In some of the equations I estimated, I also allow for establishment- and occupation-specific effects to affect wages, using analysis of covariance techniques.

IV. RESULTS

First, I investigate so-called total differences in wages by method of pay, in the terminology of analysis of covariance, not distinguishing within- from between-establishment differences. These analyses address hypotheses R1 and B1 concerning the risk-sharing and the bargaining interpretations respectively. Two objections to the results are discussed in Section IV B. In Section IV C, I address the second objection by conducting a within-establishment analysis of differences in wages by method of pay. At the same time, I address hypotheses R2 and B2 about the within-establishment uses of payment schemes.

A. Total Differences within and between Establishments

In this discussion, I first present some descriptive statistics, then I proceed to the regression analysis. In table 2 I give the distributions on methods of pay for nonsupervisory salespersons in department stores. The percentages paid on incentives (straight commission or salary plus commission) are highest in the occupations where the big-ticket items are sold—electrical appliances, kitchen appliances, major appliances, furniture and bedding, and floor coverings—ranging from 84% to 98% of the employees. These are precisely the occupations where differences in sales performances are likely to have the largest impacts. The results hence illustrate the relationship between the degree to which the employee controls output and incentive coverage.[16]

Table 3 contains the averages and SDs of hourly wages measured in dollars for each occupation as a whole and according to method of pay within the occupation. In all 11 occupations, workers paid according to one of the incentive schemes on the average earn more than workers paid a straight salary. Workers paid a salary plus commission earn more on the average than workers paid a straight commission in 10 of the 11 occupations (except in major appliances). The differences in hourly pay received by salary and salary-plus-commission workers range from a low 30% in floor coverings to a high 100% in men's clothing and kitchen appliances.

Table 3 shows further that there are vast differences between the 11

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[16] Several studies discuss the relationship between the degree to which an employee controls output and the extent of incentive coverage. The basic idea is that unless there is leeway for the worker to influence output, there is no point to providing incentives. See, e.g., Stieber (1959, chap. 11), Lokiec (1966, p. 314), Edwards (1979, pp. 19–21), Schatz (1983, p. 238), Edwards and Scullion (1982, pp. 187, 189), Dessler (1984, p. 391), Elbaum (1984, p. 96), International Labor Office (1984, chaps. 2 and 5), and Gartman (1986, pp. 159, 281).

TABLE 2

DISTRIBUTION OF METHODS OF PAYMENT FOR NONSUPERVISORY SALESPERSONS

METHOD OF PAYMENT AS % AND *N* OF SAMPLE	OCCUPATIONS										
	Floor Coverings	Furniture and Bedding	Housewares, Except China	Kitchen Appliances	Men's Clothing	Notions	Shoes	Sporting Goods	Electrical Appliances	Major Appliances	General Sales
Salespersons paid by salary:											
%	15.9	12.5	94.3	1.1	49.3	96.9	31.9	84.6	13.4	13.4	97.7
N	214	384	3,490	20	2,023	771	1,441	2,127	200	99	37,823
Salespersons paid by straight commission:											
%	44.3	53.9	1.8	32.4	32.9	1.3	36.6	13.7	42.1	54.0	0.2
N	586	1,656	67	613	1,348	10	1,650	344	629	400	63
Salespersons paid by salary plus commission:											
%	40.7	33.6	3.9	66.5	17.8	1.9	31.5	1.7	44.5	32.7	2.1
N	550	1,032	144	1,258	732	15	1,421	43	664	242	815
Total:											
%	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0
N	1,350	3,072	3,701	1,891	4,103	796	4,512	2,514	1,493	741	38,701

SOURCE.—Bureau of Labor Statistics, *Industry Wage Survey: Department Stores, June 1981* (U.S. Department of Labor 1982).
NOTE.—Total *N* (salespersons) = 62,874; *N* (stores) = 178.

TABLE 3

AVERAGES AND STANDARD DEVIATIONS OF HOURLY WAGES (in Dollars) BY OCCUPATION AND PAYMENT SYSTEM

Occupation	Salary	Straight Commission	Salary Plus Commission	All Three Pay Schemes
Floor coverings	6.47	8.37	8.45	8.10
	(3.75)	(4.09)	(3.41)	(3.75)
Furniture and bedding	6.72	9.11	10.38	9.24
	(4.14)	(3.76)	(3.72)	(3.96)
Housewares, except china	4.22	4.29	6.34	4.31
	(.97)	(1.50)	(2.38)	(1.15)
Kitchen appliances	6.64	9.31	12.50	11.46
	(2.25)	(2.80)	(2.99)	(3.31)
Men's clothing	4.27	8.09	8.49	6.27
	(1.18)	(3.26)	(3.84)	(3.28)
Notions	3.89	6.34	7.04	3.98
	(.64)	(1.18)	(1.34)	(.84)
Shoes	4.91	5.98	6.84	5.91
	(1.80)	(2.26)	(2.88)	(2.47)
Sporting goods	4.29	6.59	6.69	4.65
	(1.30)	(2.05)	(3.06)	(1.69)
Electrical appliances	7.20	8.64	10.20	9.14
	(5.03)	(2.95)	(3.29)	(3.60)
Major appliances	4.13	7.89	7.18	7.15
	(1.31)	(3.14)	(3.48)	(3.32)
General sales	3.99	6.37	6.39	4.05
	(.71)	(2.48)	(1.87)	(.84)

SOURCE.—Bureau of Labor Statistics, *Industry Wage Survey: Department Stores, June 1981* (U.S. Department of Labor 1982).

NOTE.—Total N (salespersons) = 62,874; N (stores) = 178. Numbers in parentheses are SDs.

occupations in their average wage levels, also when we control for the method of pay. The big-ticket-item occupations—furniture, kitchen appliances, electrical appliances—earn the highest average wages, while the lowest are earned in notions, general sales, and housewares.

In summary, from table 3 we see that the method of pay matters for wages. Employees paid according to one of the two output-related schemes on the average earn more than employees paid a straight salary in all 11 occupations. In 10 of the 11 occupations, employees paid a salary plus commission earn more than employees paid a straight commission, supporting the bargaining interpretation underlying hypothesis B1 over the risk-sharing interpretation of hypothesis R1.

Turning to the regression analysis where a richer set of controls is introduced, the following equation is estimated:

$$\ln w_{ijt} = \boldsymbol{\theta}\mathbf{x}_{ijt} + \epsilon_{ijt}, \tag{4}$$

where $\ln w_{ijt}$ is the logarithm of the hourly wage (defined in Sec. III) of employee i in occupation j in store t, $\boldsymbol{\theta}$ is a $1 \times p$ vector of parameters, $\mathbf{x}_{ijt}$ is a $p \times 1$ vector of explanatory variables (including the constant 1.0), and ϵ_{ijt} is a random error term.

The vector of explanatory variables contains the store size (the number of employees in the store), regional location, whether it is a regular or a discount store, the full- or part-time status of the employee, the occupation of the employee, and the method of paying the employee. All variables are entered as dummies. Other than method of pay, none is of substantive interest in the present discussion. Estimates of all the effects are reported in order to secure completeness of the results.

Estimates of the parameters in equation (4) are given in table 4. In the first column all the regressors are entered simultaneously; in the second column the occupation dummies are excluded; and in the third column the method of pay is excluded, but occupation is retained.

The information in table 4 reveals three clear findings. First, the method of pay matters considerably for the average wage, even when the richer set of controls is introduced. Consider the estimates in the first column. Workers paid a salary plus commission earn on the average 11% more per hour than workers paid a straight commission and about 50% more per hour than those paid a straight salary, while straight-commission workers on the average earn 35% more than those paid a straight salary.[17] The main pay difference is between employees paid a salary and employees paid on one of the two incentive schemes. The differences between straight commission and salary-plus-commission workers come out as predicted by the bargaining interpretation. The 95% confidence intervals for the coefficients for salary-plus-commission and straight-commission workers do not overlap.

Second, it is not surprising to find that occupation matters for wages. The difference in hourly wages between the highest and the lowest paid occupation is as much as 80% (between notions and kitchen appliances).[18]

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[17] The effects of 50% and 35% were computed as follows. The point estimate for the effect of being paid a salary plus commission is .408. Hence, the relative difference in wages between salary-plus-commmission workers and straight-salary workers is $\exp(.408) - 1 = .50$. The effect of 35% was computed by the same formula, using the point estimate of .301.

[18] The effect of 80% was computed as follows. The point estimates for the occupational effects of kitchen appliances and notions are, respectively, .301 and $-.288$. Hence, the relative pay differential between the two occupations is $\exp(.301 + .288) - 1 = .80$.

TABLE 4

EFFECTS ON THE LOGARITHM OF THE HOURLY WAGE RATE

	1	2	3
Constant	1.468 (.064)	1.225 (.069)	1.716 (.069)
Area of store:[a]			
Boston	.036 (.004)	.038 (.004)	.042 (.004)
South	−.027 (.002)	−.017 (.002)	−.037 (.002)
West and north central	.034 (.002)	.044 (.002)	.022 (.002)
Discount store[b]	−.075 (.002)	−.089 (.003)	−.094 (.003)
Size of store:[c]			
20–49	.149 (.070)	.138 (.070)	.150 (.076)
50–99	.027*(.078)	.038*(.085)	.057 (.085)
100–249	.033*(.064)	.020*(.069)	.046*(.069)
250–499	.034*(.064)	.026*(.069)	.067*(.069)
500–999	.064*(.064)	.066*(.069)	.072*(.069)
1,000–2,499	.083*(.063)	.094*(.069)	.109*(.069)
2,500+	.150 (.063)	.139 (.069)	.190 (.069)
Full-time employee[d]	.121 (.002)	.150 (.002)	.151 (.002)
Employee's occupation:[e]			
Furniture and bedding	.127 (.007)		.128 (.007)
Housewares, except china	−.221 (.007)		−.482 (.007)
Kitchen appliances	.305 (.008)		.373 (.008)
Men's clothing	−.098 (.007)		−.211 (.007)
Notions	−.288 (.010)		−.557 (.011)
Shoes	−.188 (.007)		−.236 (.007)
Sporting goods	−.183 (.008)		−.413 (.008)
Electrical appliances	.131 (.008)		.143 (.008)
Major appliances	−.075 (.010)		−.066 (.010)
General sales	−.253 (.007)		−.524 (.006)
Method of pay:[f]			
Straight commission	.301 (.004)	.504 (.003)	
Salary plus commission	.408 (.004)	.631 (.003)	
R^2 (σ)	.637 (.230)	.572 (.249)	.577 (.248)

SOURCE.—Bureau of Labor Statistics, *Industry Wage Survey: Department Stores, June 1981* (U.S. Department of Labor 1982).

NOTE.—See eq. (4). Numbers in parentheses are estimated SEs *except* for numbers in parentheses for R^2. These are estimates of residual SEs. Total N (salespersons) = 62,874; N (stores) = 178.

[a] Reference category = Northeast (except Boston).

[b] Reference category = regular store.

[c] Reference category = 1–19 employees.

[d] Reference category = part-time employee.

[e] Reference category = employees selling floor coverings.

[f] Reference category = employees paid a straight salary.

* Not significantly different from zero at the 5% level (two-tailed test).

Hence the position occupied, when controlling for the method of pay, is important.

Finally, the second column of table 4 shows that the effects of method of pay are higher when occupation is not controlled for, and the third column shows that the effects of occupation are amplified in all but two cases when method of pay is not controlled for. This means that marginal differences in pay among different incentive schemes in some measure capture occupational differences, and conversely that marginal differences among occupations in some measure capture differences in incentive schemes. But none of the two effects is reducible to the other.

Next, I estimate the regression equation in (4) separately for each occupation, allowing for a full set of interaction terms between occupation and the other covariates. The equation is

$$\ln w_{ijt} = \boldsymbol{\theta}_j \mathbf{x}_{ijt} + \epsilon_{ijt}, \tag{5}$$

where $\boldsymbol{\theta}_j$ is an occupation-specific parameter vector and ϵ_{ijt} an error term with occupation-specific variance. In table 5 below, I report the estimates only of the constant term, the method of pay, and full- versus part-time employment.[19]

Two items are notable in table 5. First, in all 11 occupations, employees paid a salary earn less on the average than employees paid with one of the two incentive schemes. The differences range from 3% to 80% in hourly pay.[20] Second, in nine out of 11 occupations, employees paid a salary plus a commission earn more than employees paid a straight commission. The differences range from 5% to 44%. In the two deviant occupations—sporting goods and major appliances—employees paid a straight commission earn about 2% and 4% respectively more than employees paid a salary plus commission.[21]

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[19] Other than merely statistical arguments, such as the goal to minimize bias due to uncontrolled interaction effects, there is also a substantive argument for studying each occupation separately. In many stores, employees are promoted from one line of sales to another, in part due to seniority and in part due to merit (see Noyelle 1987, pp. 30–35). Considering each occupation separately allows one to some degree to study the effects of methods of pay on wages net of the stage on the career ladder the employees find themselves.

[20] These relative differences were computed by the same formulas as in nn. 17 and 18, as are the relative differences reported below.

[21] In five of the nine occupations where salary-plus-commission workers earn more than straight-commission workers, the 95% confidence intervals for the two coefficients overlap, indicating that the differences between the two schemes may not be significant. However, the pattern of the differences is here so striking that even if the two coefficients were not significantly different from each other in a single one of the 11 occupations, we would nevertheless have strong support for the hypothesis that salary-plus-commission workers earn more than straight-commission workers. Using the rather conservative sign test (DeGroot 1986, pp. 572–73), we can reject the null

TABLE 5

EFFECT OF METHOD OF PAY AND FULL- VERSUS PART-TIME STATUS ON THE LOGARITHM OF THE HOURLY WAGE RATE BY OCCUPATION

	INDEPENDENT VARIABLES				
OCCUPATION	Constant	Straight Commission[a]	Salary Plus Commission[a]	Full-Time[b]	R^2 (σ)[c]
Floor coverings	1.252	.106	.201	.390	.280
	(.223)	(.031)	(.031)	(.026)	(.370)
Furniture and bedding	1.362	.141	.288	.236	.283
	(.223)	(.023)	(.024)	(.020)	(.368)
Housewares, except china	1.329	.031*	.342	.133	.264
	(.166)	(.024)	(.016)	(.006)	(.191)
Kitchen appliances	1.464	.215	.578	.252	.364
	(.183)	(.056)	(.056)	(.021)	(.245)
Men's clothing	1.455	.467	.500	.197	.505
	(.133)	(.013)	(.015)	(.011)	(.320)
Notions	1.189	.454	.588	.083	.482
	(.043)	(.041)	(.034)	(.010)	(.129)
Shoes	1.223	.112	.228	.116	.191
	(.070)	(.013)	(.013)	(.010)	(.329)
Sporting goods	1.275	.379	.302	.197	.383
	(.054)	(.029)	(.029)	(.010)	(.231)
Electrical appliances	1.800	.132	.322	.374	.309
	(.140)	(.029)	(.029)	(.022)	(.330)
Major appliances	1.472	.370	.351	.315	.312
	(.098)	(.048)	(.051)	(.038)	(.366)
General sales	1.214	.378	.406	.078	.316
	(.029)	(.018)	(.005)	(.001)	(.148)

SOURCE.—Bureau of Labor Statistics, *Industry Wage Survey: Department Stores, June 1981* (U.S. Department of Labor 1982).

NOTE.—Total N (salespersons) = 62,874; N (stores) = 178. The dependent variable is the hourly wage (its logarithm). The estimated equations contain, other than the variables in the table, region, no. of employees in the store, and discount vs. regular store, represented by three, seven, and one dummy variables, respectively. See eq. (5). Numbers in parentheses are SEs of regression coefficients, except where noted.

[a] Reference category: employees paid a straight salary.

[b] Reference category: part-time employees (full-time = 1).

[c] Estimated residual SEs in parentheses.

* Not significantly different from zero at the 5% level (two-tailed tests).

In conclusion, the evidence from table 5 shows that: (1) incentives matter considerably within occupations, because employees paid a salary on the average earn less than employees paid on one of the two incentive schemes; (2) the prediction of the bargaining interpretation regarding

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hypothesis of no difference between the two schemes at a significance level of .032 in a two-tailed test. Using the same test, we can reject at a much higher level of significance the null hypothesis, corresponding to hypothesis B1, that salary-plus-commission workers earn less or no more than straight-commission workers.

differences between straight commission and salary plus commission—that the former workers on the average earn less than the latter—are confirmed in nine out of 11 occupations. Thus, when investigating each occupation separately, the data continue to support the bargaining over the risk-sharing interpretation.

B. Discussion of Results

Two potential problems with the analysis so far are likely to be raised, and both pertain to the interpretation of the effects of incentive schemes. First, the effects may not reflect the operation of incentive schemes per se. They may as well reflect differences in productive capacities between employees who work under the various schemes. Employees who are more productive may choose to be paid by results and the less productive to be paid a salary. That is, the two groups of workers self-select themselves into their most preferred scheme (see Lazear and Rosen 1981, pp. 258–61; Lazear 1986; Goldin 1986, pp. 18–19). I can do little to provide a rigorous empirical response to this objection, but see the discussion in Section II C above. The data contain no individual productivity characteristics beyond those introduced in tables 4 and 5, the full- or part-time status of the employee. I am not aware of other data sets that, in addition to the relevant employee-level characteristics, contain detailed information about pay schemes and occupation.

Some defense against the objection, nevertheless, can be made. The occupational classification of salespersons used here is exceptionally detailed. It is unlikely therefore that much variation occurs within an occupation in characteristics such as schooling and other formal credentials that might be correlated with the unobserved productivities. As was shown in table 2, the differences between the occupations in the use of incentive schemes are vast, while, as shown in table 1, the differences within and between occupations in educational attainment are few (for some further, albeit limited, quantitative support of this point see Talbert and Bose [1977, p. 412, n. 9]). The omitted variable bias from not controlling for education and other productivity characteristics is therefore probably less severe than it would have been had the detailed occupational classification not been available.[22]

Three studies of wages in department stores that do take account of several of the relevant human capital variables reveal the following. The

[22] Churchill et al. (1985, pp. 341–45) summarize research on the effects of personal characteristics on sales effectiveness. They report that the findings are ambiguous with respect to the effects of age, experience, and education, with no statistically significant effects found in a majority of the studies.

two historical studies—using data from, respectively, 1888, 1907, and 1913–14—found strong effects of experience on wages (Goldin 1980, p. 87; Carter and Carter 1985, pp. 592, 596). In both studies the increase in wages peaks after about 10 years of experience. The study using contemporary data (collected between June 1972 and January 1973) found almost no effects on wages of age, experience, and tenure with store (Talbert and Bose 1977, p. 412, table 1; see also French 1960, p. 129). If the latter is representative for current (i.e., 1981) conditions, the omitted variable bias accruing from age, experience, and tenure is likely to be small. Similarly, union status appears to have no effect on wages among department store employees (see Bluestone et al. 1981, pp. 110–11). There is still a bias from not controlling for sex and race. The direction of the bias is difficult to assess, since we do not know how or whether gender and race covary with the three methods of pay within each of the occupations.

A second objection is that the effects of incentive schemes may be confounded with the effects of working in particular stores. If stores paying higher wages also tend to pay their employees with incentive schemes as opposed to salaries, the differences revealed here may reflect differences between stores as opposed to the effects of incentive schemes. This objection can be addressed using the present data set. We know the wages and pay schemes of all workers within each of the 178 stores. Hence, within-establishment differentials in wages according to the method of pay can be computed.

C. Within-Establishment Differences

I first address the extent to which different methods of pay are used within the same occupation and store. In doing this I also address hypotheses R2 and B2. Table 6 gives the distribution of stores on their practices with respect to combining different methods of pay, for each of the 11 occupations. Most striking in the table is that, in all 11 occupations, 65% or more of the stores use only one method of pay and, in seven of the occupations, more than 80% of the stores use only one method of pay. Thus, mixing different methods of pay in the same occupation within a store is the exception rather than the rule. Differences in methods of pay in an occupation occur across more than within stores. The table shows further that the two output-related methods of pay are used simultaneously in the same store and occupation in only 27 of the 969 occupation-store matches, which is less than in 3% of the matches. In four of the 11 occupations the two methods of pay are never used side-by-side in the same store and occupation. In the remaining seven occupa-

TABLE 6

DISTRIBUTION (%) OF STORES ON COMBINATIONS OF METHODS OF PAY USED WITHIN EACH OCCUPATION

OCCUPATION	STORE-LEVEL COMBINATION OF METHODS OF PAY WITHIN A GIVEN OCCUPATION								
	S Only	*C* Only	*S* + *C* Only	*S* and *C*	*S* and *S* + *C*	*C* and *S* + *C*	*S*, *C* and *S* + *C*	Sum	*N* Stores (*N* workers)[a]
Floor coverings	9.5	43.8	16.7	7.3	19.8	1.0	2.1	100	96
	(5.7)	(33.6)	(10.7)	(10.7)	(36.1)	(1.2)	(1.9)	(100)	(1,350)
Furniture and bedding	15.2	50.0	18.8	1.8	9.8	2.7	1.8	100	112
	(8.5)	(48.6)	(19.3)	(1.8)	(15.7)	(2.9)	(3.2)	(100)	(3,072)
Housewares, except china	86.0	2.5	2.5	1.7	7.5	.0	.0	100	121
	(82.5)	(1.6)	(2.0)	(2.6)	(11.3)	(.0)	(.0)	(100)	(1,891)
Kitchen appliances	.0	52.9	25.5	3.9	17.6	.0	.0	100	51
	(.0)	(30.5)	(43.0)	(2.3)	(24.2)	(.0)	(.0)	(100)	(1,891)
Men's clothing	32.3	30.8	4.6	12.3	16.2	3.1	.8	100	130
	(21.9)	(21.8)	(5.9)	(23.4)	(20.0)	(3.9)	(3.1)	(100)	(4,103)
Notions	91.4	1.7	3.4	1.7	1.7	.0	.0	100	58
	(95.7)	(0.3)	(1.5)	(1.9)	(0.6)	(.0)	(.0)	(100)	(796)

Shoes	25.5	36.4	11.8	8.2	12.7	4.5	.9	100	110
	(16.1)	(24.3)	(17.6)	(10.9)	(21.7)	(7.3)	(2.1)	(100)	(4,512)
Sporting goods	76.9	4.4	1.1	8.8	8.8	.0	.0	100	91
	(69.8)	(4.5)	(0.9)	(12.1)	(11.7)	(.0)	(.0)	(100)	(2,514)
Electrical appliances	13.0	39.0	16.9	10.4	19.5	1.3	.0	100	77
	(6.4)	(31.6)	(20.0)	(12.5)	(28.1)	(1.5)	(.0)	(100)	(1,493)
Major appliances	8.2	32.7	28.6	12.2	6.1	8.2	4.1	100	49
	(2.3)	(37.2)	(21.3)	(15.9)	(4.3)	(10.3)	(8.6)	(100)	(741)
General sales	92.0	1.1	.0	1.1	3.4	.0	2.3	100	88
	(92.0)	(.0)[b]	(.0)	(.0)[c]	(4.0)	(.0)	(3.9)	(100)	(38,705)

SOURCE.—Bureau of Labor Statistics, *Industry Wage Survey: Department Stores, June 1981* (U.S. Department of Labor 1982).

NOTE.—Total N (salespersons) = 62,874; N (stores) = 178. S only = all employees in the store-occupation match are paid a salary; C only = all employees in the store-occupation match are paid a straight commission; $S + C$ only = all employees in the store-occupation match are paid a salary plus commission; S and C = two pay systems are present in the store-occupation match, so that some employees are paid a salary and others a straight commission; S and $S + C$ = two pay systems are present in the store-occupation match, so that some employees are paid a salary and others a salary plus commission; C and $S + C$ = two pay systems are present in the store-occupation match, so that some employees are paid a straight commission and others a salary plus commission; S, C and $S + C$ = all three pay systems are present in the store-occupation match, so that some employees are paid a salary, some a straight commission, and others a salary plus commission. Numbers in parentheses are the % distribution of workers, by occupation, on the stores, except where noted.

[a] Numbers in parentheses are worker N's by occupation.

[b] The cell contains a single store with 14 employees in general sales (i.e., .036% of 38,705).

[c] The cell contains a single store with 11 employees in general sales (i.e., .028% of 38,705).

tions the two methods of pay are mixed within the same store and occupation in less than 6% of the matches.[23]

These findings speak to hypotheses R2 and B2. The bargaining interpretation of incentive schemes predicts that straight commission and salary plus commission will not be used simultaneously in the same occupation and store. The workers are either paid a straight commission or a salary plus commission, but not some a straight commission and others a salary plus commission. (Each method of pay may be used concurrently with a straight salary.) Under the risk-sharing interpretation of incentive schemes no such prediction was obtained. The data square with the former interpretation. The within-store variation in the use of the two output-related payment systems is much smaller than the between-store variation, which squares with hypothesis B2 from the bargaining interpretation.

Turning now to the within-store regression analysis, I select the subset of employees working in stores where two or three pay schemes are used side-by-side in the same occupation. In this analysis, occupation-store matches where only one pay scheme is used contribute nothing to the analysis of within-store effects. Four possible combinations of pay schemes occur in an occupation within a store: (1) salary and straight commission, (2) salary and salary plus commission, (3) straight commission and salary plus commission, (4) salary, straight commission, and salary plus commission. For each of the four combinations within each of the 11 occupations the following fixed-effects regression equation is estimated:

$$\ln w_{ijt} = \boldsymbol{\theta}_{jm}\mathbf{x}_{ijt} + \alpha_{jtm} + \epsilon_{ijt}, \tag{6}$$

where the parameter vector $\boldsymbol{\theta}_{jm}$ is specific to occupation j and the combination m of methods of pay used in the occupation in the store ($m = 1, \ldots, 4$), $\mathbf{x}_{ijt}$ contains the method of pay and the full-time versus part-time status of the employee, and ϵ_{ijt} is a random error term. The fixed-effect is captured by the dummy variable α_{jtm}, which equals one for employees in occupation j in store t that uses the combination m of methods of pay, and zero otherwise.[24] It gives the effect of all variables having the same values for the employees in occupation j in store t and that affect their wages in the same ways. For instance, it captures store size, union status, area, capital concentration, monopoly power, and so on. The parameters are estimated using an analysis of covariance strategy, where an estimate is obtained only of the so-called within effect, θ_{jm}, but not of the fixed effect, α_{jtm} (see Judge et al. 1985, chap. 13.3). Thus we need not estimate the effect parameters for the occupation-store pairs. The estimate of θ_{jm} obtains by subtracting the relevant store and occupation means, given the combination of methods of pay, from all the variables in the regression equation in (6) and then applying least squares to the transformed variables.[25]

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[23] Goldin (1986, pp. 19–20) reports similar practices in American industries in the years 1895–96, where straight salary and piece rates rarely were used simultaneously in the same occupation and firm.

[24] Several other versions of (6) were estimated, for example, versions where $\boldsymbol{\theta}_{jm}$ did not vary by j and m, and versions in which α_{jtm} was treated as a random rather than as a fixed effect, in which case stores and occupations using only one method of pay also contributed. The overall substantive conclusions from these alternative specifications are the same as those reported below.

Table 7 gives the parameter estimates for each occupation and store-level combination of methods of pay. Consider first parts A and B, which give the results for stores in which a salary and a straight commission (A) and a salary and a salary plus commission (B) are used side-by-side in the same occupation. We see that in all 11 occupations, employees paid with one of the two incentive schemes earn more than employees paid a salary. Thus the effect of being paid by results as opposed to a salary holds also within stores. When a straight commission is compared with a straight salary we see further that the within-store differences are greater than the between-store differences in eight of the 11 occupations (cf. table 5 with part A of table 7).[26] Similarly, when we compare employees paid a salary with employees paid a salary plus commission, the within-store differences are greater than the between-store differences in nine of the 11 occupations (cf. table 5 with part B of table 7). This shows that the processes that occur within stores create more differences between employees than the processes that occur between stores.

I next turn to part C of table 7, which gives the results for stores using straight commission and salary plus commission within the same occupation. In four of the six possible comparisons we see that within stores, employees paid a straight commission earn more on the average than employees paid a salary plus commission. Thus the finding in tables 4 and 5—that employees paid a salary plus commission earn more on the average than employees paid a straight commission—does not hold when we study intrastore differences. These within-store results square better with the risk-sharing interpretation of incentive schemes. One

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[25] The SEs from the least squares output were recalculated since the within regression uses the incorrect number of degrees of freedom for the estimated residual variance.

[26] I here refer to the results in table 5 as between-store differences, though they more correctly are referred to as total differences because they do not distinguish within-store from between-store variation. In the current data the between-store and total differences are very close, since there is little within-store variation (for details on the relationships between total, within, and between differences in analysis of covariance, see Judge et al. [1985, p. 523]).

TABLE 7

WITHIN-STORE EFFECTS OF METHOD OF PAY AND FULL- VERSUS PART-TIME STATUS ON THE LOGARITHM OF HOURLY WAGES, FOR EACH OCCUPATION AND STORE-LEVEL COMBINATION OF METHODS OF PAY IN THE OCCUPATION

(Estimated Standard Errors in Parentheses)

	INDEPENDENT VARIABLES				
OCCUPATION	Straight Commission	Salary plus Commission	Full-Time[a]	*N* Stores	*N* Workers
A. Stores Using Salary and Straight Commission:[b]					
Floor coverings	.486 (.059)		.107*(.059)	7	145
Furniture and bedding	.120*(.209)		.717 (.159)	2	56
Housewares, except china	.366 (.047)		.118 (.028)	2	96
Kitchen appliances	.019*(.097)		.383 (.130)	2	44
Men's clothing	.473 (.019)		.155 (.017)	16	960
Notions	.466 (.052)		.179*(.065)	1	15
Shoes	.141 (.033)		.103 (.028)	9	492
Sporting goods	.409 (.038)		.071 (.033)	8	330
Electrical appliances	.309 (.078)		.351 (.064)	8	186
Major appliances	.128*(.083)		.434 (.075)	6	118
General sales	.643 (.226)			1	11
B. Stores Using Salary and Salary Plus Commission:[c]					
Floor coverings		.347 (.038)	.359 (.031)	19	7
Furniture and bedding		.380 (.039)	.245 (.035)	11	483
Housewares, except china		.429 (.029)	.245 (.035)	9	419
Kitchen appliances		.569 (.067)	.383 (.033)	9	457
Men's clothing		.635 (.023)	.177 (.021)	21	822
Notions		.535*(.600)	−.303*(.600)	1	5
Shoes		.453 (.023)	.203 (.020)	14	979
Sporting goods		.372 (.052)	.483 (.033)	8	294
Electrical appliances		.524 (.038)	.384 (.029)	15	419
Major appliances		.430 (.072)	.296 (.067)	3	32
General sales		.155 (.011)	.191 (.008)	3	1,530

C. Stores Using Straight Commission and Salary Plus Commission:[c]					
Floor coverings		−.740 (.134)	.159*(.240)	1	16
Furniture and bedding		.248 (.081)	.033*(.105)	2	88
Housewares, except china				0	0
Kitchen appliances				0	0
Men's clothing		−.306 (.077)	.176 (.072)	4	162
Notions				0	0
Shoes		−.203 (.043)	.170 (.033)	5	331
Sporting goods				0	0
Electrical appliances		.031*(.178)		1	22
Major appliances		−.053*(.086)	.331 (.125)	4	76
General sales				0	0
D. Stores Using Salary, Straight Commission, and Salary Plus Commission:[b]					
Floor coverings	.717*(.491)	.447*(.472)	.273*(.400)	2	26
Furniture and bedding	.917 (.229)	.621 (.209)	.326 (.159)	2	97
Housewares, except china				0	0
Kitchen appliances				0	0
Men's clothing	.581 (.211)	.188*(.096)	.320 (.098)	1	128
Notions				0	0
Shoes	1.213 (.29)	.703 (.078)	.091*(.071)	1	93
Sporting goods				0	0
Electrical appliances				0	0
Major appliances	.559 (.095)	.383 (.058)	−.126*(.075)	2	64
General sales	.354 (.038)	.266 (.019)	.145 (.016)	2	1,525

SOURCE.—Bureau of Labor Statistics, *Industry Wage Survey: Department Stores, June 1981* (U.S. Department of Labor 1982).

NOTE.—Total N (salespersons) = 62,874; N (stores) = 178. The dependent variable is the hourly wage (its logarithm). The estimates are from fixed-effects models where the fixed-effects pertain to the occupation and store, given the combination m of methods of pay. See eq. (6).

[a] Reference category: part-time employee (full-time = 1).

[b] Reference category: employees paid a straight salary.

[c] Reference category: employees paid a straight commission.

* Not significantly different from zero at the 5% level (two-tailed tests).

should note, as shown in table 6, that the two methods of pay are rarely combined in the same occupation and store. In four of the six occupations in part C of table 7, fewer than 100 employees are compared.

Finally, consider part D, which pertains to stores using all three methods of pay within a given occupation. Again, the differences between employees paid a salary and employees paid on one of the two incentive schemes are as predicted. In four of the six possible comparisons the within-store differences are greater than the between-store differences (cf. table 5 with part D of table 7). As in part C, employees paid a straight commission earn more than employees paid a salary plus commission in all of the six relevant occupations. Again, within-store processes differ from between-store processes. And, as in part C, the results square better with the risk-sharing interpretation of incentive schemes.[27]

In summary, the method of pay matters for wages also when within-establishment comparisons are made. The pay differences between a straight salary and the two output-related payment schemes cannot be reduced to differences in establishment-level characteristics; they also appear within establishments. Between establishments, the wage differentials come out as predicted by the bargaining interpretation, as shown in Section IV A above (tables 4 and 5). Within establishments, in contrast, the pay differentials come out as predicted by the risk-sharing interpretation, as shown in table 7. However, the within-establishment comparisons apply to very few stores and employees in the sample.

V. SUMMARY AND CONCLUSIONS

Output-related payment systems—such as commissions, piece rates, and production bonuses—are common in many jobs. Their central feature is that the wage of an employee is tied directly to her current performance, not to past performance and future potential, as in bureaucratic career systems (see, e.g., Stinchcombe 1974, chaps. 5–6). Focusing on salespersons in department stores, I have considered wage differences under three distinct payment systems: straight salary, straight commission, and salary plus commission.

In the conceptual analysis, I discussed four interpretations of the wage differences under the incentive schemes: a risk-sharing, a bargaining, a sorting, and a social status interpretation. The risk-sharing and the bargaining interpretations were singled out for special attention in the empirical analyses. Both interpretations predict that straight salary workers earn less on the average than workers paid according to the two incentive schemes, as the two latter groups are being compensated for working harder and for sharing some of the risks of the employment relationship. Under the risk-sharing interpretation it was argued that straight-commission and salary-plus-commission plans primarily reflect different risk-sharing arrangements. It was therefore predicted that straight-commission workers on the average will earn more than salary-plus-commission workers. The mechanism is that straight-commission workers are compensated for increased risk-bearing by receiving higher *expected* wages. That is, on the average they will do better than salary-plus-commission workers, but some may do considerably worse. Under the alternative bargaining interpretation of the two output-related reward structures, it was predicted that salary-plus-commission workers on the average will earn more than straight-commission workers, the mechanism being that the bargaining power of a worker or group of workers will allow them to draw a salary in addition to the commission.

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[27] Using the same sign test as in n. 21 above, we can reject, on the basis of the pattern of differences in part D of table 7, the null hypothesis of no differences between salary-plus-commission workers and straight-commission workers at a significance level of .015.

In the empirical analysis it was shown that, in all 11 sales occupations studied, workers paid according to a straight salary earn less on the average than workers paid according to one of the two output-related schemes. This holds for both between- and within-establishment comparisons. It was further shown that straight-commission workers on the average earn less than salary-plus-commission workers in nine of 11 occupations, when we do not distinguish within- from between-establishment differences, but control for several relevant establishment characteristics. In an analysis of within-establishment differences this pattern was reversed in the majority of occupations: straight-commission workers on the average earn more than salary-plus-commission workers.

Differences in wages between establishments seem therefore to square better with the bargaining interpretation of incentive schemes. Establishments using a salary plus commission on the average pay their incentive-rated employees higher wages than establishments using a straight commission. From a worker's viewpoint, therefore, it is desirable to be allocated to an establishment using a salary-plus-commission rather than a straight-commission system. However, were one to be allocated to an establishment where both payment systems are used side-by-side in the same occupation, it appears desirable, at least from the viewpoint of wages earned, to be paid a straight commission rather than a salary plus commission. Within establishments, therefore, the risk-sharing interpretation found in economic theories of the agency relationship carries more force.

However, there is not much intraestablishment variation to explain. The two output-related reward schemes are used simultaneously in the same occupation and store in only 27 of the 969 occupation-store matches in the sample. On this account, the bargaining interpretation is quite successful in predicting observed practices; according to this interpretation the two output-related schemes are not likely to be used side-by-side in the same store and occupation.

In conclusion, I stress three points. First, in terms of the effects of payment systems on wages, the major wage differences seem to occur between establishments, simply because there is less within- than between-establishment variation in the uses of different payment systems. Hence, in order to understand the part of inequality generated by different payment systems, we need first of all to understand the process by which establishments adopt specific forms of remuneration. Having understood this process, we need next to understand the allocation of workers on the establishments using different payment schemes.

Second, to the extent that establishments use two or more payment schemes for the same occupation, the within-establishment differences appear to be much more dramatic than the between-establishment differences. This means that when there is within-establishment heterogeneity, then that heterogeneity is larger than the heterogeneity found in the market as a whole. We need a theory for why the within-establishment processes differ from the processes that operate between establishments.

Third, and perhaps most important, in light of the empirical findings here, future research on labor-market stratification may be well advised to take account of the incentive structures that employees face. In many jobs, employees have considerable scope in affecting their own output and rewards—though probably rarely to the extent as among salesworkers. The theoretical scenario that has been at the heart of much recent sociological research, that positions are the primary determinants of rewards, may then be too restrictive. The analysis of the positional determinants of rewards should be supplemented with an analysis of how employees take advantage of the opportunities offered by different jobs and reward structures.

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Growth Effects of Foreign and Domestic Investment[1]

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Over the past few years, numerous studies in sociology have concluded that foreign investment harms poor nations. These studies have focused on the coefficient for foreign capital stock, controlling for "flow" (new investment), and—defying logic—have inferred that a negative coefficient for stock reflects "dependency effects" that retard economic growth. Since capital stock is the denominator for investment rate, the greater the stock, the *lower* the investment rate, for a given level of new investment. Hence the negative coefficient for capital stock found in dependency studies indicates a *beneficial* investment effect, not a *harmful* effect. This point is demonstrated by reanalysis of the data used in dependency studies. In these data, foreign investment *spurs* growth. Earlier analysts have concluded otherwise only because their conclusions have belied their findings.

Among the most fundamental principles in economics is the principle that economic growth requires capital investment. Indeed, capital is generally thought to constitute one of the three basic categories of the factors of production (the other two categories being land and labor). A long and hallowed tradition in economics assumes positive effects for all three. Other endowments being equal, the more land, the more output; the more labor, the more output; and the more capital, the more output.

Recent work in sociology breaks sharply with this tradition by arguing that a particular type of capital—foreign investment stock—retards and distorts development in the Third World (Bornschier and Chase-Dunn 1983, 1985; Bornschier, Chase-Dunn, and Rubinson 1978; Boswell and Dixon 1990; London 1987, 1988; London and Robinson 1989; London

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and Smith 1988; London and Williams 1988, 1990; Wimberley 1990, 1991). The claim is that capital-poor Third World nations actually tend to be better-off in the long run if they eschew foreign capital. To quote Boswell and Dixon (1990, p. 554), "Previous research has documented the negative effects of economic dependency [foreign investment] on domestic growth." Such claims are legion in sociology.[2]

In this four-part article, I test the adverse-effects claim. Section I reports estimates of the economic effects of foreign and domestic investment, based on data for 76 less developed countries (LDCs). These estimates are based on the data set that has been used in virtually all prior sociological studies of capital dependency. Yet reestimation is necessary because—as I explain in Section II—earlier studies misinterpret denominator effects as "dependency effects." Section III reestimates a prototypical dependency study to show the mischief caused by the misinterpretation. Section IV contains the conclusion.

I. ESTIMATES OF GROWTH EFFECTS OF FOREIGN AND DOMESTIC INVESTMENT

The Source of Capital: Does It Matter?

In the antiseptic world of some economic theories, capital and labor are completely mobile, political leaders are all wise and altruistic, and class interests do not exist. In such a world capital is capital; its source is irrelevant. However, in the real world of structural barriers, ineffective and corrupt governments, and class interests, the source of capital could matter—plenty (e.g., Amin 1974, 1976; Barnet and Mueller 1974; Bornschier and Chase-Dunn 1985; Hymer 1979).

There are certainly compelling reasons for expecting Third World countries to benefit less from foreign investment than from the indigenous kind. Compared with domestic investment, foreign investment is (1) less likely to contribute to public revenues, as transnational corporations are often able to avoid taxes through mechanisms such as "transfer pricing" (e.g., Streeten 1973), (2) less likely to encourage the development of indigenous entrepreneurship, (3) more likely to use inappropriate capital-intensive technology (though this is controversial; see White 1978), (4) less likely to reinvest profits in the host country, and (5) more likely to be "linkage weak" (Hirschman 1977).

These last two reasons are especially important. Foreign investment may be "outward looking" in two respects. Not only does the profit flow outward, but the products often do as well. In such cases output "can slip out of a country without leaving much trace in the rest of the economy," as Hirschman (1958, p. 110) so aptly puts it. Domestic industries, in contrast, are more likely to form links with other industries in the domestic economy. According to Hirschman (1977, pp. 80–81), such linkages are the stuff of economic development: "A linkage exists whenever an ongoing activity gives rise to economic or other pressures to lead to the taking up of a new activity," and "development is essentially the record of how one thing leads to another, and the linkages are that record." In a similar vein, Amin (1974, 1976) argues that poor nations tend to remain poor because their internal economic sectors are "disarticulated," and—like Hirschman—he believes that an outward-looking economy is more likely to be disarticulated. If these arguments are correct, then surely the growth effects of foreign investment fall well short of those of domestic investment.

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[2] By "previous research" Boswell and Dixon (1990) refer to research in sociology, not economics. Economists most often find that foreign investment *augments* domestic investment (see below), thus spurring growth.

Capital Dependency Research

It is one thing to claim that foreign investment is *not as good* as the homegrown variety. It is quite another matter to claim that foreign investment is bad. To actually retard growth, foreign investment must impede domestic enterprises. There are several possibilities. Transnational corporations could destroy nascent or even well-established local businesses (e.g., Amin 1976, chap. 4). Or they could co-opt local entrepreneurial talent. Or they could stimulate inappropriate consumption patterns, thereby lowering domestic savings rates. The result would be "investment decapitalization"—long-run decline in investment—if the shortfall due to lower savings exceeded the capital pumped into the economy from the outside.

Capital dependency or PEN research—so-called because it is based on the PEN (foreign capital penetration) measure of Bornschier and Chase-Dunn (1985)—*does* claim that foreign investment is pernicious for Third-World nations. Indeed, recent cross-national research in sociology appears to be preoccupied with demonstrating how foreign investment is a full-fledged menace in the Third World: how it reduces economic growth, increases inequality, impedes fertility reduction, promotes mortality, incites rebellion, fuels urban bias, and, in general, makes life miserable for residents. Lest there be any doubt as to whether PEN researchers really mean to say that foreign investment is bad (and not just "less good"), consider these conclusions culled from studies that use the PEN variable to measure investment dependence or "multinational corporation (MNC) penetration":

> [Foreign] capital formation (PEN) has clearly disadvantageous consequences for economic growth [in LDCs]. [Bornschier and Chase-Dunn 1985, p. 96]
>
> [PEN] clearly distorts development in ways that impede fertility decline. [London 1988, p. 615]
>
> This analysis shows that MNC penetration [PEN] promotes high mortality levels in the Third World. [Wimberley 1990, p. 87]
>
> Taken together, the findings above lead to the conclusion that it is multinational corporate penetration [PEN], not income inequality, that directly accounts for increased levels of collective political violence experienced by nations. [London and Robinson 1989, p. 307]
>
> Our overall conclusion is that dependency [PEN and military dependence] in the world economy and international state system gives rise to rebellion. [Boswell and Dixon 1990, p. 555]
>
> The results . . . reveal a significant positive relationship between penetration [PEN] and urban bias. [London and Smith 1988, p. 461]
>
> There is a significant negative relation between multinational corporate penetration [PEN] and basic needs provision. [London and Williams 1988, p. 761]
>
> This study adds strong empirical support to the world-system and dependency perspectives' claim that economic ties between the core and non-core harm the majority of noncore inhabitants. [Wimberley 1991, p. 427]
>
> Transnational corporate investment [TNC] dependence has an exceptionally strong harmful effect on [food] consumption. . . . [TNCs] promote immiseration in the non-core. [Wimberley and Bello, in press]

Unless they are used vacuously, "harm," "immiserate," and similar words must mean that foreign investment actually is bad for the Third World—and not just that it is "less good" than domestic investment. Yet what precisely do PEN researchers mean when they say foreign capital "immiserates"? Dependency writers of the 1960's "stagnationist" school appear to claim that dependency *precludes* growth (see Gereffi 1983, chap. 1). The growth of many LDCs since then has deflated that claim, so the revised claim is that dependency tends to reduce—not preclude—economic growth. Hence by "immiseration" PEN researchers appear not to mean that capital penetration renders LDCs incapable of growth; rather, their claim is only that capital-penetrated LDCs are poorer than they would have been without the foreign investment.

There are, then, three viable positions regarding foreign investment's effect on economic growth. (1) Capital is capital. Foreign investment's effect is positive and the same size as that of domestic investment. (2) Foreign investment on balance tends to promote growth, albeit its effect is often not as large as that of domestic investment. This middle-of-the-road position is popular in development economics (see Gillis et al. 1983, chap. 14). (3) Foreign investment reduces growth. This is the conclusion of PEN research. Distinguishing between numbers 2 and 3—growth-promoting and growth-reducing investment—clearly is not hairsplitting. Naturally, LDCs try to encourage growth-promoting investment and avoid the other kind. So it is important to know which of these three positions characterizes foreign investment.

Data and Measures

The three positions can be tested using existing data. To avoid the accusation that the results are stacked against dependency theory, I use the PEN data set (from Ballmer-Cao and Scheidegger [1979], supplemented by data in Bornschier and Chase-Dunn [1985]). As in Bornschier and Chase-Dunn, the dependent variable is economic growth rate. In the PEN data set economic growth rate is measured by rate of growth of per capita GNP over the period 1965–77, expressed as an annual percentage.

Investment rate (I_r) is the crucial independent variable. Here "rate" refers of course to change relative to initial level, so investment rate is change in capital relative to initial stock of capital. The PEN studies are based on foreign capital stock as of 1967 and 1973. To insure that my results do not depend on the way I calculate investment rate, I do it three ways. The first is simply the percentage increase from 1967 to 1973:

$$\%I_r = [(1973K - 1967K)/1967K] \times 100, \tag{1}$$

where K is capital stock. The second is annual rate of change, expressed as a percentage:

$$\text{ann}I_r = [\sqrt[6]{(1973K/1967K)} - 1] \times 100, \tag{2}$$

where $\sqrt[6]{(1973K/1967K)}$ denotes sixth root of $1973K/1967K$. The third measure is the continuous-time analogue to annual I_r. To distinguish it from (2), I refer to the continuous-time annual rate as "annualized I_r" (annzI_r). It is calculated as follows:

$$\text{annz}I_r = \{[\ln(1973K/1967K)]/6\} \times 100, \tag{3}$$

where ln is the natural logarithm.

From the PEN data, foreign and domestic investment rates can be calculated for 76 LDCs and 15 wealthy nations for a total of 91 nations (see App. A). To be consistent with earlier research, I include the conventional PEN control variables in my analysis, using the measures and data given in Bornschier and Chase-Dunn (1985). These measures are: (1) the logarithm of total energy consumption in 1967, to control for the effect of domestic market demand or "size"; (2) the average value of exports as a percentage of gross domestic product for the years 1965, 1970, and 1973, to control for the effect of export activity; (3) the logarithm of gross national product per capita (GNP/*c*) in 1965, to control for the effect of 1965 GNP/*c* on 1965–77 *growth rate* of GNP/*c*. Again, to be consistent with PEN research, I include this third variable as both log(GNP/*c*) and $[\log(\mathrm{GNP}/c)]^2$, so that I can model the presumed quadratic effect of initial GNP on subsequent GNP growth (Bornschier and Chase-Dunn 1985, p. 92). Appendix B, table B1, reports univariate statistics and bivariate correlations for these variables.

Results for Growth Rate

What do we find? There is strong support for the view that foreign investment is ***not as good*** as domestic investment (table 1). Other things constant, annual economic growth is boosted by an estimated .23% for every 1% annual increase in domestic investment, but only by .08% for every 1% annual increase in foreign investment (see table 1, col. 3). The difference between the slopes (domestic vs. foreign I_r) is significant ($P < .05$) in every instance (significance tests not shown). Regardless of measure—investment rate as percentage increase or annual rate or annualized rate—domestic investment is better.[3]

Hence the results contradict the view that "capital is capital." But they also contradict the PEN view that foreign investment impedes growth. In every instance the slope for foreign investment is ***positive*** ($P < .01$). This holds whether investment is measured as percentage increase (table 1, cols. 1 and 2), as annual rate (cols. 3 and 4), or as annualized rate (cols. 5 and 6); it holds when ***per capita*** investment rates are used (results not shown in table 1); it holds when collinearity is reduced by dropping $[\log(\mathrm{GNP}/c)]^2$ (cols. 2, 4, 6); and it holds when the PEN controls are dropped altogether (results not shown in table 1). The robustness of the foreign-investment effect is in sharp contrast to the usual instability of cross-national research. The positive investment effects reported in table 1 are typical of what I found in estimating literally dozens of models.

In contrast to the investment slopes, the slopes for the PEN controls tend to be small and nonsignificant. Aside from market demand, the coefficients for the PEN control variables all fall well short of statistical significance.

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[3] The smaller slopes for $\%I_r$ largely reflect metric differences ($\%I_r$ is total, not annual, increase). With regard to foreign investment, $\%I_r$ is skewed markedly to the right, so I logged it and reestimated models 1 and 2. The results are the same (App. table C1).

Foreign Investment's Long-Run Economic Effect

Using the PEN data set, I find that LDCs with greater rates of foreign investment tend to exhibit faster rates of economic growth, independent of the effects of domestic investment, export activity, and so on (table 1, above). Nevertheless, PEN researchers conclude that LDCs tend to be richer if they eschew foreign investment. The argument is based on a sharp distinction between foreign investment's short-run and long-run effects (Bornschier and Chase-Dunn 1985, p. xi): "We find that, while flows of foreign investment have short-run positive effects on economic growth, accumulated stock of foreign capital (indicating a high degree of penetration and control by transnational corporations) has a long-run retardant effect on economic growth."

The claim then is that, other endowments equal, LDCs with higher accumulations of foreign capital are worse off in the long run than those with lower accumulations of foreign capital. Growth rate per se is not the concern here, since rates reflect short-run effects; rather, I want to determine the *cumulative* economic effect of foreign investment. Will LDCs eventually prosper more—enjoy higher levels of income per capita—with or without foreign investment? To answer that question I must determine foreign investment's total (direct + indirect) long-run effects on GNP/*c*.

Without wishing to belabor the obvious, it is important to stress that a long-run effect is a cumulation of short-run effects. So if PEN researchers are right about the positive short-run effects of foreign investment—and table 1 certainly supports their view—then one wonders how positive short-run effects "add up" to a negative long-run effect.

The dependency answer lies in the notion of "decapitalization." Though decapitalization stories can be complex (Vernon 1971, chap. 5)—involving trade, balance of payments, and so forth—the PEN version is based on Bornschier's thesis that foreign capital harms LDCs by severely depressing their domestic investment. Lest there be any doubt, Bornschier (1980, fig. 1) formalizes his thesis by ordering the variables as follows: foreign capital→ domestic capital→ GNP growth. The critical test of decapitalization, then, is whether the path coefficient from cumulated foreign capital to cumulated domestic capital is negative (since all agree that domestic investment has a positive effect on GNP).[4]

Development economists most often tell a very different story. The famous Harrod-Domar capital theory, for example, assumes augmen-

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[4] Research in the PEN tradition keys on the Bornschier (1980) "depressed-investment" model, so that is the decapitalization model I test here. The PEN data do not permit tests of trade- or remittance-based decapitalization stories.

TABLE 1

ESTIMATED ECONOMIC EFFECTS OF FOREIGN AND DOMESTIC INVESTMENT: GROWTH RATE MODELS

	INVESTMENT RATE MEASURE[a]					
	$\%I_r$		$AnnI_r$		$AnnzI_r$	
INDEPENDENT VARIABLE	(1)	(2)	(3)	(4)	(5)	(6)
Foreign investment rate:						
Metric *b*	.005**	.005**	.077**	.076**	.084**	.083**
t-value	3.1	3.1	3.6	3.6	3.6	3.6
Standardized *b*	.29	.29	.33	.33	.33	.33
Domestic investment rate:						
Metric *b*	.029**	.029**	.233**	.233**	.253**	.254**
t-value	4.0	4.0	3.8	3.9	3.9	4.0
Standardized *b*	.39	.38	.36	.36	.36	.37
PEN control variables:						
Market demand:						
Metric *b*	.50	.50	.56*	.56*	.57*	.57*
t-value	1.9	2.0	2.2	2.2	2.2	2.3
Standardized *b*	.23	.23	.25	.25	.26	.26

Exports:						
Metric *b*	.004	.004	.004	.004	.004	.005
t-value	.3	.3	.3	.3	.3	.3
Standardized *b*	.02	.02	.03	.03	.03	.03
1965 GNP/*c* (log):[b]						
Metric *b*	−.35	.76	.87	.63	1.2	.61
t-value	−.1	1.3	.2	1.1	.2	1.1
Standardized *b*	−.07	.15	.17	.12	.22	.12
[Log(1965 GNP/*c*)]^2:						
Metric *b*	.21		−.05		−.10	
t-value	.2		−.04		−.1	
Standardized *b*	.22		−.05		−.11	
Adjusted R^2	.50	.51	.51	.52	.51	.51
Adjusted R^2 without control variables	.42		.42		.42	

NOTE.—In order to be consistent with PEN research, coefficients are reported for the model in which both log(1965 GNP/*c*) and its square are included (see cols. 1, 3, and 5). Because the two variables correlate +.997, results are also reported for the model without the squared term (see cols. 2, 4, and 6). Partial regression plots and diagnostic statistics indicate that results are not due to outliers. N = 76 LDCs.

[a] See eqq. (1)–(3) in text for definitions of the three rates.

[b] GNP/*c* denotes GNP per capita.

* $P < .05$ (two-tailed).

** $P < .01$ (two-tailed).

tation, not decapitalization (Gillis et al. 1983): "In a Harrod-Domar world . . . the role of foreign saving of all kinds is to *augment* domestic saving to increase investment and thus accelerate growth" (p. 374; emphasis added). Thus Harrod-Domar theory claims that foreign investment boosts total investment: foreign investment$\xrightarrow{+}$ total investment$\xrightarrow{+}$ economic performance. (Ironically, Bornschier and Chase-Dunn [1985, p. 82] appeal to Harrod-Domar theory to support a decapitalization view—but in doing so they disregard the theory's assumptions; see App. D below.) Contrary to what one might infer from PEN studies, the augmentation view is not bereft of empirical support. Indeed, Salvatore (1983) claims that "it has been conclusively confirmed empirically that foreign capital inflows . . . make a positive net contribution to the rate of capital formation" (pp. 69–70; see Sprout and Weaver [1990] for recent evidence supporting augmentation).

Because it contains *cumulated* data on foreign and domestic investment, the PEN data set is especially well suited for testing the long-run effects of capital. Stock in 1967 refers to cumulated investment *as of* 1967. Domestic stock goes back 18 years; hence 1967 domestic stock reflects investment cumulated from 1950 to 1967 (see Ballmer-Cao and Scheidegger [1979, p. 73] for the depreciation formula used). The foreign stock measure goes back even further: it refers to the net book value of foreign holdings. So the PEN data on foreign stock are ideal for determining foreign investment's long-run effects, since, in principle, book value reflects investment from day 1. Moreover, "day 1" for foreign investment is well before 1950 for many LDCs, so there is a built-in lag for the foreign stock relative to the domestic stock. This is a critical point in view of the claim that foreign investment reduces domestic investment over the long run. If that claim is true, the reduction certainly should be evident in the PEN data.

Table 2 summarizes the PEN-data results for decapitalization and augmentation. In the 1967 models, accumulated capital as of 1967 (domestic capital in the case of decapitalization, total capital in the case of augmentation) is regressed on accumulated foreign capital as of 1967. To repeat, foreign capital is exogenous in PEN decapitalization models (Bornschier 1980), so it is exogenous here. Capital is in dollars per capita (logged to reduce skew; see Jackman 1980). Results are given for regressions with and without the PEN controls.[5] The 1973 models use accumulated capital

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[5] I do not control for GNP/*c* because it is the criterion variable in both the decapitalization and augmentation models. Bornschier and Chase-Dunn fault Jackman (1982) for failing to control for the effect of "the level of economic development . . . [on] the degree of transnational penetration" (Bornschier and Chase-Dunn 1985, p. 86). Here, however, level of economic development is measured by GNP/*c* at time *t*, and transna-

TABLE 2

ESTIMATED EFFECT OF ACCUMULATED FOREIGN CAPITAL ON ACCUMULATED DOMESTIC CAPITAL AND ACCUMULATED TOTAL CAPITAL*

MODEL	DEPENDENT VARIABLE: Accumulated Domestic K (Decapitalization Test)	DEPENDENT VARIABLE: Accumulated Total K (Augmentation Test)
Accumulation as of 1967:		
No controls:		
Standardized b	.74	.78
t-value	9.5	10.9
PEN controls:[†]		
Standardized b	.66	.71
t-value	10.0	11.5
Accumulation as of 1973:		
No controls:		
Standardized b	.76	.80
t-value	10.1	11.7
PEN controls:		
Standardized b	.65	.70
t-value	10.3	12.0
Accumulation as of 1973, with additional lag:[‡]		
No controls:		
Standardized b	.73	.77
t-value	9.1	10.3
PEN controls:		
Standardized b	.63	.67
t-value	9.5	10.7

NOTE.—Standardized coefficients are used because they are handier here for calculating indirect and total effects (see table 3). Partial regression plots and diagnostic statistics indicate that results are not due to outliers.

* Accumulated total capital = total foreign stock plus an 18-year accumulation of domestic stock.

[†] Demand and exports, as in table 1. GNP/c is not controlled for, because both decapitalization and augmentation assume that GNP is endogenous with respect to accumulated capital.

[‡] 1973 accumulation is regressed on 1967 accumulation, allowing an additional six years for foreign investment effects to appear.

as of 1973, rather than as of 1967. If, perchance, book value of foreign holdings from day 1 fails to capture all time-delay dependency effects, the PEN data permit an additional six-year lag by regressing 1973 domestic cumulation on 1967 foreign cumulation.

The results are unambiguous: augmentation, not decapitalization. The coefficient for cumulated foreign investment is always positive, and always statistically significant at $P < .0001$ or better (see table 2).

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tional penetration is measured by *accumulated* foreign capital *as of t*. And GNP/*c* obviously does not work backward in time to determine earlier investment.

TABLE 3

ESTIMATED LONG-RUN ECONOMIC EFFECT OF FOREIGN INVESTMENT[a]

Model	Effect through Total Investment[b]	Direct Effect[c]	Total Effect[d]
Accumulation as of 1967:			
No controls	.83**	−.16*	.67**
PEN controls	.67**	−.06	.61**
Accumulation as of 1973:			
No controls	.88**	−.21*	.67**
PEN controls	.68**	−.11	.57**
Accumulation as of 1973, with additional lag:			
No controls	.81**	−.17*	.64**
PEN controls	.62**	−.06	.56**

NOTE.—Partial regression plots and diagnostic statistics indicate that results are not due to outliers. $N = 76$ LDCs.

[a] Total accumulation of foreign stock; 18-year accumulation of domestic stock (see text).

[b] Product of standardized regression coefficients for foreign accumulation→total accumulation→GNP.

[c] Expressed as a standardized regression coefficient. A negative coefficient is consistent with the view that foreign investment is *not as good* as domestic investment.

[d] Direct effect + indirect effect through total investment.

* $P < .05$ (two-tailed).

** $P < .001$ (two-tailed).

What about the long-run *economic* effect of foreign investment? As just observed (table 2), cumulated foreign investment boosts cumulated total investment. Of course, countries with more cumulated total investment tend to be richer than those with lower cumulations, so some of the long-run economic effect of foreign investment is indirect, through its positive effect on total investment. By contrast, the unmediated effect of foreign investment should be negative, reflecting the earlier finding that foreign investment is *not as good* as domestic investment.[6]

The dependent variable—logged GNP/*c*—is not cumulated output, but rather output for a single year. Unless vacuous, the claim that foreign investment has a long-run retardant economic effect must mean that at some point in time accumulated foreign capital demonstrably lowers output.

The estimates are given in table 3. By boosting accumulated total capital, accumulated foreign capital boosts economic level in the long run. This indirect effect is large. The unmediated long-run effect of foreign capital—reflecting the earlier finding that foreign investment is less beneficial than domestic investment—is negative, but much smaller (and statistically insignificant when the PEN controls are added). As a result, the total effect of foreign investment is positive; the estimates range from +0.56 to +0.67. In short, *according to the PEN data,* foreign investment has a *positive* long-run economic effect in the Third World. This conclusion holds whether I use capital cumulation as of 1967 or as of 1973; it holds whether or not the PEN control variables are included; and it holds when I allow an additional six-year lag.

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[6] When total investment is constant, the more foreign investment there is, the higher the ratio of foreign investment to domestic investment. Hence if foreign investment is not as good as domestic investment, the coefficient for foreign investment, total investment controlled, should be negative.

II. ACCOUNTING FOR THE CONTRARY CONCLUSIONS OF EARLIER RESEARCH

If the PEN data in fact show that foreign investment has a beneficial long-run effect, why do PEN studies conclude otherwise? The problem lies not in wrong coefficients but in wrong interpretations of coefficients. The PEN researchers arrive at conclusions that are at odds with their results. To understand how this has happened, we must first understand the importance these studies attach to the distinction between capital stock and capital flow.

Capital Stock versus Capital Flow

The long-standing economic distinction between capital flow and capital stock was popularized in sociology in an early dependency study by Bornschier et al. (1978). By distinguishing foreign capital stock—"total cumulated value of foreign-owned capital in a country"—from foreign capital flow—"current account inflows of foreign capital for some time period" (p. 661)—Bornschier et al. try to account for an interesting pattern in cross-national studies of foreign capital and economic growth. Studies using capital flow to measure the effect of foreign capital (e.g., Papanek 1973) typically find beneficial investment effects, whereas those using capital stock with flow controlled for (e.g., Stoneman 1975) typically find the opposite. "What this pattern suggests is that the immediate effect of inflows of foreign capital . . . is to increase the rate of economic growth, while the long-run cumulative effects operate to reduce the rate of economic growth" (Bornschier et al. 1978, p. 667).

The Bornschier et al. thesis about stock and flow effects spawned the PEN research that still dominates cross-national work in sociology. From 1987 to 1990 in the *American Sociological Review* alone there were eight studies that used PEN or its log. Such interest is understandable: the capital dependency issue is timely, the theory controversial, the studies often painstaking. Unfortunately, though, this line of research is based on a faulty premise. A negative coefficient for stock, controlling for flow, does *not* mean that investment has a long-run adverse effect.

This point is easy to demonstrate by using the formula for investment rate as percentage increase (eq. [1]). The numerator is change in capital stock, 1967–73; PEN researchers call this *flow*. The denominator is capital accumulation as of 1967, that is, *stock*. So, in the stock and flow terminology, investment rate is simply flow divided by stock:

$$\%I_r = (\text{flow}/\text{stock}) \times 100. \tag{4}$$

Observe that equation (4) still holds when flow and stock are each expressed as a percentage of GDP—that is, when total flow and total stock both are divided by GDP—as is typical in PEN research.

From equation (4) it is immediately apparent that, with stock constant, the faster the flow the *greater* the investment rate; and with flow constant, the larger the stock the *smaller* the investment rate. So when stock and flow are entered as separate variables in a regression equation it follows logically that

1. a positive flow coefficient and a negative stock coefficient indicate a beneficial investment effect;
2. a negative flow coefficient and a positive stock coefficient indicate an adverse investment effect.

These principles hold whether investment rate is measured as percentage increase (eq. [1]), as annual rate (eq. [2]), or as annualized rate (eq. [3]).

Now we see why PEN researchers get the results they do. When the flow coefficient (controlling for stock) is positive, and the stock coefficient (controlling for flow) is negative—precisely what PEN researchers find—investment has a *beneficial* effect.

III. REANALYSIS OF A PROTOTYPICAL PEN STUDY

The mischief caused by interpreting a denominator effect as a long-run dependency effect is best appreciated by reanalyzing a PEN study. To replicate PEN research, in this section I separate the numerator and denominator of investment rate and enter them as individual regressors in growth models. The 1985 Bornschier and Chase-Dunn (hereafter BCD) study is an obvious choice for reanalysis.[7] It is no doubt the most influential of the capital dependency studies, if only because subsequent studies use its PEN measure. The BCD model has the same dependent variable and control variables as the model in table 1. It differs from the table 1 model only in its measurement of investment. In table 1, I use foreign investment rate and domestic investment rate. The BCD model uses two investment rate numerators and one denominator. The numerators are foreign flow and domestic flow, each expressed as percentage of total GDP. The denominator is foreign stock, as captured by the famous PEN measure.[8]

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[7] An earlier study using similar methods (Bornschier et al. 1978) was critiqued by Szymanski (1983) for ignoring foreign stock's positive *indirect* effects. Szymanski did not, however, note that rate is flow/stock; so PEN's more fundamental denominator effects problem—not to be confused with the much-ballyhooed "ratio variables problem" (see Firebaugh and Gibbs 1985)—has gone unrecognized.

Notable for its absence in the BCD model is the other denominator, domestic stock. I add domestic stock in the reanalysis, thereby providing a critical test of the arguments here. If I am correct, domestic stock should have a negative slope, since it is a denominator. If PEN logic is correct, the slope should be positive, reflecting the beneficial long-run effect of domestic investment (no one argues that *domestic* investment retards growth).

Central Results

Central results of the reanalysis are reported in table 4. To facilitate comparison, the results reported by BCD (1985, p. 95) for their three-component PEN model are listed first (col. 1). The BCD two-numerator, one-denominator model is conventional in PEN research on economic growth. Thus PEN studies most often examine the effect of foreign investment rate, with domestic rate's *numerator* controlled. (Some PEN studies employ fewer than three components, but never all four.) The dependent variable is GNP/*c* growth rate, so the model is a growth rate model, not a "long-run effects model." As a result, the PEN model is ill-designed to test PEN's central tenet—that foreign investment is harmful over the long run.[9]

The BCD estimates are based on 88 LDCs, whereas my sample consists

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[8] To reduce PEN's skewness, BCD and subsequent PEN researchers recode PEN for 18 outliers. Thus Panama's PEN value of 653 is recoded to 105, Trinidad and Tobago's PEN is reduced from 506 to 105, Jamaica's from 388 to 104, Liberia's from 275 to 103, and so on. The recodings seem somewhat arbitrary, so I resurrected the original values and entered them—logged, and in raw form—in parallel analyses. Consistent with BCD's (1985, p. 105, n. 20) claim, results do not depend on the PEN recordings.

[9] It should be obvious that a single-equation model—as here, where the single dependent variable is economic growth rate—does not simultaneously yield short-run and long-run effects (as PEN researchers claim). Nonetheless, to proceed as PEN studies do, we must ignore that problem.

TABLE 4

ESTIMATED ECONOMIC EFFECTS OF FOREIGN AND DOMESTIC INVESTMENT: REANALYSIS OF A PROTOTYPICAL PEN STUDY[a]

	INVESTMENT RATE MEASUREMENT					
	BCD Three-Components Model[b]		Four-Components Model		$\%I_r$ Model	
INDEPENDENT VARIABLE	[1][c]	[2]	[3]	[4]	[5]	[6]
Foreign investment rate:						
Metric *b*					.021**	.022**
t-value					3.4	3.5
Standardized *b*					.32	.33
Numerator (flow effect):						
Metric *b*	.08**[d]	.08**	.07**	.07**		
t-value	3.0	3.2	2.7	2.8		
Standardized *b*		.32	.26	.27		
Denominator (PEN) (stock effect):						
Metric *b*	−.031**	−.029**	−.026**	−.024**		
t-value	−4.0	−3.6	−3.4	−3.1		
Standardized *b*		−.49	−.43	−.39		
Domestic investment rate:						
Metric *b*					.240**	.239**
t-value					3.9	3.9
Standardized *b*					.36	.36
Numerator (flow effect):						
Metric *b*	.06	.09*	.17**	.16**		
t-value	1.6	2.3	3.9	3.7		
Standardized *b*		.27	.50	.47		

Denominator (stock effect):						
Metric *b*			−.020**	−.020**		
t-value			−3.5	−3.5		
Standardized *b*			−.41	−.42		
PEN control variables:						
Market demand:						
Metric *b*	.91**	.70*	.62*	.64*	.50	.49
t-value	3.0	2.3	2.2	2.3	1.9	1.8
Standardized *b*		.32	.28	.29	.23	.22
Exports:						
Metric *b*	.05**	.02	.02	.03	−.002	.00
t-value	2.9	1.2	1.4	1.5	−.1	.0
Standardized *b*		.13	.15	.16	−.01	.00
1965 GNP/*c* (log):						
Metric *b*	9.9	11.0	10.0	1.3	4.4	1.1
t-value	1.6	1.6	1.6	1.8	.8	1.8
Standardized *b*		2.12	1.92	.25	.86	.21
(Log[1965 GNP/*c*])^2:						
Metric *b*	−1.7	−1.9	−1.7		−.64	
t-value	−1.6	−1.5	−1.4		−.6	
Standardized *b*		−1.95	−1.68		−.65	
Adjusted R^2	.40	.37	.46	.45	.46	.47

NOTE.—Partial regression plots and diagnostic statistics identify two outliers in the $\%I_r$ model. Results are the same with the outliers removed. In col. 1, $N = 88$; in cols. 2–6, $N = 76$. Coefficients for model containing both log(1965 GNP/*c*) and its square are in cols. 1, 2, 3, and 5. Results for model without squared term are in cols. 4 and 6.

[a] See notes for table 1. The dependent variable is economic growth rate, so the BCD study is based on a growth rate model.

[b] Two numerators and one denominator. Domestic stock is the omitted component (denominator).

[c] Statistics in col. 1 are from Bornschier and Chase-Dunn (1985, table 5). They do not report standardized coefficients.

[d] Bornschier and Chase-Dunn report .008, which is based on a different metric. To simplify matters I express both domestic and foreign flow as percentage of GDP, which changes the slope of foreign flow to .08 in the BCD table.

* $P < .05$ (two-tailed).

** $P < .01$ (two-tailed).

of 76 LDCs.[10] To see if that matters, I tried to replicate the BCD results with my sample (table 4, cols. 1 and 2). The key coefficients are consistent: The PEN slope is $-.031$ for the 88 nations and $-.029$ for the 76 nations, and the flow slopes are also similar. So if the final conclusions here are at odds with those of BCD, the divergence cannot be easily dismissed on the grounds of sample differences.

The next step is to add the second denominator—domestic stock (domstock in eq. [5] below)—to the PEN three-component model. As expected, both flow slopes are positive and both stock slopes are negative. This pattern obtains whether or not the squared GNP/*c* term is included (cols. 3 and 4 of table 4; recall that the squared term correlates .997 with GNP/*c*).

If we take PEN research seriously, the negative slope for domestic stock is baffling: Does domestic investment in fact inhibit long-run economic expansion in LDCs? Yet there is no real mystery here, since a negative stock slope paired with a positive flow slope implies a beneficent investment effect. The reanalysis of BCD, then, nicely confirms the earlier conclusion that domestic and foreign investment both promote economic growth.

Finally, the BCD investment data are expressed directly as rates, that is, flow is divided by stock. This is equivalent to the $\%I_r$ model estimated earlier (table 1 above). The coefficients most likely will differ somewhat from those of table 1, however, since the investment measures differ. Specifically, with respect to domestic rate, BCD base their measure of 1967–73 domestic flow on three years rather than six;[11] and with respect to the foreign rate, flow/PEN is not identical to flow/stock ($r = .95$; I explain below why $r \neq 1.0$).

The last two columns in table 4 report the results for the BCD $\%I_r$ model. My conclusions are the same as before. Foreign investment stimulates economic growth, net of domestic investment and the PEN controls. The standardized slope is $\beta = +.32$, as compared with $\beta = +.29$ in table 1 (the metrics differ, so this is the apt comparison). Domestic investment also spurs growth: $\beta = +.36$ as compared with $\beta = +.39$ in table 1. The investment rate effects dominate the model. The same results obtain when foreign investment rate is logged to reduce skewness and when two outliers—Indonesia and South Korea—are simply omitted (tables are available upon request).

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[10] Some of the investment data BCD use apparently are from sources they do not list. Using their listed source (Ballmer-Cao and Scheidegger 1979) I was able to find complete investment data for 76 of the 88 LDCs. Those 76 LDCs make up the sample here. Of the 12 nations I had to omit, 10 lacked data on domestic stock and 2 (Romania and Yugoslavia) were omitted due to undefined foreign investment rates (zero denominators).

[11] Bornschier and Chase-Dunn estimate domestic flow by taking the average for three years (1965, 1970, 1973) rather than simply subtracting 1967 stock from 1973 stock, as they do in the case of foreign flow. For the sake of reanalysis, I follow the BCD procedures scrupulously.

The PEN Measure Itself

Investment rate as percentage increase is (flow/stock) × 100. For foreign investment, flow/stock is approximated by flow/PEN (again, $r = .95$). Why is the correlation not perfect?

The answer lies in the way PEN is normed. In PEN research, the flow measures are normed (divided) by total GDP. Foreign stock (forstock in [5] below), by contrast, is normed by the square root of total domestic stock weighted by population. This quantity is of course highly related to total GDP ($r = .89$), but the two are not equivalent. As a result, flow/PEN ≠ flow/stock.

It is instructive to factor the PEN measure to see what it captures besides stock:

$$\begin{aligned} \text{PEN} &= \text{forstock}/(\sqrt{\text{domstock}} \times \sqrt{\text{pop}}) \\ &= (\sqrt{\text{forstock}/\text{domstock}}) \\ &\quad \times [(\sqrt{\text{forstock}} \times \sqrt{\text{domstock}}]/\sqrt{\text{pop}}). \end{aligned} \tag{5}$$

So the PEN measure reflects both stock *source* (foreign vs. domestic, factor 1 above) and stock *level* (amount of stock per capita). Each implies a negative coefficient for PEN. Because domestic investment is *better* than foreign investment, the ratio of foreign to domestic stock (factor 1) will have a negative slope. Because investment rate has a beneficial effect, stock per capita (factor 2) will also have a negative slope. The latter factor dominates (table available upon request).[12] The overriding point, however, is that either way—whether PEN is capturing the denominator effect, or the effect of foreign investment *relative to* that of domestic investment—PEN researchers have misinterpreted their results.

PEN Research on the *Noneconomic* Effects of Foreign Investment

The PEN studies of the *noneconomic* effects of foreign investment likewise routinely reach conclusions that are at odds with their findings.

A recent study well-illustrates the point. In a painstaking analysis of the effect of foreign investment on Third World mortality, Wimberley (1990, p. 89) concludes that "in the long run, the greater the penetration of non-core areas by the capitalist world economy, the greater the privation of the majority in those areas." His findings indicate just the opposite, however. Using the three-components PEN model, he consistently finds a positive flow coefficient and a negative stock (PEN) coefficient for life expectancy.

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[12] Along these lines, Crenshaw (1991, p. 1173) finds that PEN correlates .94 with foreign stock per capita.

Still other PEN studies of the noneconomic effects of foreign investment use *one*-component (PEN only) models. Because such one-component models have no defensible interpretation, conclusions from those studies remain suspect until they can be confirmed using sensible models of investment's effect.

IV. SUMMING UP

Piqued by dependency and world-system theory, sociologists have begun to study a subject previously thought to be the stuff of development economics; that is, foreign investment's economic and welfare effects in the Third World.[13] The sociological studies draw on published estimates of cumulated capital as of 1967 and 1973—the "PEN data." Analyses of this data set are not altogether satisfactory, however, because the analysts' findings and conclusions are antipodal.

Dependency theory is too important to be ignored (as it is, by and large, in economics). Regrettably, its study in sociology has used illogic. A fresh start is needed. Reanalysis of the PEN data provides such a start. The reanalysis yields three major conclusions.

1. From the host country's perspective, all capital is not equal; the source *does* matter. Homegrown capital outperforms imported capital. This is true whether investment rate is measured as percentage increase, annual rate, or annualized rate.

2. If the PEN data are reliable, foreign investment benefits LDCs. The coefficients for foreign investment rate are always positive and statistically significant. Contrary to the earlier conclusions of sociologists, for-

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[13] A common misperception is that most foreign investment goes to LDCs. Multinational corporations are much more likely to invest in rich nations, especially in recent years. From 1985–89, new foreign investment in *all* LDCs totaled $78 billion; in the United States alone the total was $231 billion (Turner 1991, tables 12 and 13). The United States in fact is the leading recipient of foreign direct investment by far, accounting for over 40% of the world total since 1985. While foreign investment in the United States exploded during the 1980s, such investment in LDCs stagnated, giving rise to a new worry about the adverse global effect of "reconcentration of industrial capital in core areas" (Sassen 1988, p. 188).

eign investment apparently promotes growth over the long run as well as over the short run. This is not to deny that particular nations at particular times for particular reasons are hurt by foreign investment (see O'Hearn 1989; contrast Barrett and Whyte [1982], and Bradshaw [1988]). There is nonetheless no evidence in the PEN data of a general tendency for foreign investment to retard growth.

3. The capital dependency tradition that has recently dominated cross-national research in sociology is based on an error. Although the error has methodological implications, it is more aptly described as an error in logic. The PEN researchers employ growth rate or panel models, that is, models based on change in the dependent variable over the short run. In those models, salutary investment effects have paraded as negative stock coefficients. This is just as it should be since—flow constant—the greater the stock the *lower* the investment rate. Researchers have overlooked that mathematical fact, however, and have concluded that negative stock (PEN) slopes somehow reflect harmful "long-run" effects.

I close on a more upbeat note. Sociological models of Third World development can be improved by a quantum leap merely by adding appropriate measures of investment. Investment rate should be included routinely in panel and rate models. In this study I use data from capital dependency researchers—a conservative strategy, since such researchers would hardly want to choose data that exaggerate the benefits of investment—yet in that data investment is by far the *most important determinant* of economic growth in the Third World. In efforts to show how our perspectives add to those of "mere economics" then, we sociologists must not eschew all things economic. Foreign and domestic investment have important effects, albeit not necessarily of the sort described in recent sociological research.

APPENDIX A

Nations in Study[14]

Seventy-six Less Developed Countries

Burundi, Cameroon, Central African Republic, Chad, Dahomey (Benin) Ethiopia, Ghana, Guinea, Ivory Coast, Kenya, Liberia, Madagascar, Malawi, Mali, Mauritania, Niger, Nigeria, Rwanda, Senegal, Sierra Leone, Somalia, South Africa, Zimbabwe, (South Rhodesia), Tanzania,

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[14] The 76 LDCs and the 15 rich nations are defined as such in Bornschier and Chase-Dunn (1985).

Togo, Uganda, Upper Volta, Zaire, Zambia, Algeria, Egypt, Morocco, Sudan, Tunisia, Costa Rica, Dominican Republic, El Salvador, Guatemala, Honduras, Jamaica, Mexico, Nicaragua, Panama, Argentina, Bolivia, Brazil, Chile, Colombia, Ecuador, Paraguay, Peru, Uruguay, Venezuela, Iran, Iraq, Turkey, Afghanistan, Burma, Hong Kong, India, Indonesia, Japan, South Korea, Malaysia, Pakistan, Philippines, Sri Lanka, Thailand, Greece, Ireland, Italy, Portugal, Spain, Papua New Guinea, Taiwan, and Trinidad and Tobago

Fifteen Rich Nations

Canada, United States, Austria, Belgium, Denmark, Finland, France, Germany (West), Netherlands, Norway, Sweden, Switzerland, United Kingdom, Australia, and New Zealand

APPENDIX B

TABLE B1

CORRELATIONS AMONG VARIABLES IN THE PEN MODEL

	$(GNP/c)_r$	Foreign I_r	Domestic I_r	Market Demand	Exports	1965 GNP/c	(1965 GNP/$c)^2$
Foreign I_r	.54						
Domestic I_r	.59	.46					
Market demand	.46	.16	.24				
Exports	−.003	−.06	.03	−.16			
1965 GNP/c	.34	.05	.10	.63	.15		
(1965 GNP/$c)^2$	.33	.05	.09	.64	.14	.997	
Mean	2.4	8.2	6.8	3.2	22.9	2.6	6.8
SD	2.0	8.8	3.2	.9	12.6	.4	2.1
Skewness	.6	.9	.3	.1	1.2	.2	.4

NOTE.—$(GNP/c)_r$ is rate of growth of GNP per capita over the period 1965–77 (dependent var.); foreign I_r is foreign investment rate (annual rate); domestic I_r is domestic investment rate (annual rate); market demand is total energy consumption (log); exports are total value of exports as percentage of total GDP; 1965 GNP/c is log of 1965 GNP per capita. N = 76 LDCs.

APPENDIX C

TABLE C1

ESTIMATED ECONOMIC EFFECTS OF FOREIGN AND DOMESTIC INVESTMENT USING LOGGED FOREIGN INVESTMENT RATE

Independent Variable	Complete Model	Model without $(\text{Log}[\text{GNP}/c])^2$
Foreign investment rate (log):		
Metric *b*	1.31**	1.31**
t-value	3.4	3.4
Standardized *b*	.31	.31
Domestic investment rate:		
Metric *b*	.028**	.029**
t-value	4.0	4.1
Standardized *b*	.38	.38
PEN control variables:		
Market demand:		
Metric *b*	.58*	.57*
t-value	2.3	2.3
Standardized *b*	.26	.26
Exports:		
Metric *b*	.005	.005
t-value	.3	.3
Standardized *b*	.03	.03
1965 GNP/*c* (log):		
Metric *b*	1.17	.60
t-value	.2	1.1
Standardized *b*	.23	.11
$(\text{Log}[1965\ \text{GNP}/c])^2$:		
Metric *b*	−.11	
t-value	−.1	
Standardized *b*	−.11	
Adjusted R^2	.51	.52

NOTE.—This table contains results for models 1 and 2 in table 1 with foreign investment rate (%I_f) logged to reduce skewness. Skewness is 3.7 before logging, 0.3 after logging.

* $P < .05$ (two-tailed).

** $P < .01$ (two-tailed).

APPENDIX D

The Harrod-Domar Capital Model of Economic Growth

Though originally intended to explain the relationship between growth and unemployment in rich capitalist countries, the Harrod-Domar model (Harrod 1939; Domar 1946, 1947) has been used extensively by policymakers as a simple way to determine capital requirements for LDCs. This model is key to Bornschier and Chase-Dunn's (1985) claim that foreign investment necessarily undermines domestic saving in the long run. Specifically, they argue that economic growth slows because of an inevitable slowing of foreign investment (1985, pp. 81–82). Because the economic growth rate falls, "either the propensity to save has to decrease, or capital productivity, or both at the same time" (p. 82). This follows, they claim, from the famous Harrod-Domar expression:

$$g = s/k, \tag{D1}$$

where g is economic growth rate, s is the *domestic* savings rate, and k is the capital-output ratio (K/Y—the inverse of capital productivity). From equation D1 it seems to follow immediately that g declines when, and only when, s declines and/or k increases.

However, the argument ignores the assumptions that underlie the expression $g = s/k$. One of the central assumptions is that there is *no net foreign investment* (Gillis et al. 1983). If that assumption is waived, equation D1 no longer holds. When there is foreign investment, growth rate over some finite interval is $\Delta Y/Y$, or (Gillis et al. 1983, p. 122):

$$g = (\Delta K/Y)/k, \tag{D2}$$

where ΔK—change in capital—is total investment (for the whole economy, change in K equals investment by definition; see Todaro 1981, p. 59). Since foreign investment is nonzero, ΔK is the sum of domestic and foreign investment, that is, $I_d + I_f$. So growth rate can be expressed:

$$\begin{aligned} g &= (I_d/Y + I_f/Y)/k \\ &= s/k + (I_f/Y)/k, \end{aligned} \tag{D3}$$

since $I_d/Y = s$, the domestic savings rate. From a Harrod-Domar perspective, then, we expect decline in foreign investment to adversely affect growth rate, but *not* s or k.

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Gender, Parenthood, and Job-Family Compatibility[1]

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This article explores the contention that the concentration of women in certain jobs that accommodate parenting can help explain both occupational gender segregation and the lower wages received by women employed full time. Evidence from the 1977 Quality of Employment Survey shows that the combination of both schedule flexibility and ease of job performance most clearly reduces job-family conflict for parents. However, mothers employed over 30 hours a week are not more likely to be in jobs with those characteristics, nor are predominantly female jobs in general likely to possess that cluster of characteristics shown to reduce job-family conflict.

The family and the workplace have frequently been characterized as "greedy institutions" (Coser 1974) because of the commitment of time and energy that each demands during the peak years of family formation and career mobility. The pared-down, highly mobile family of the late 20th century relies on fewer and fewer adults sharing the responsibilities of child rearing and household maintenance, while more and more adults are drawn into the labor force outside the home. The intensification of demands on parents has been especially acute for mothers since they are apt to retain primary responsibility for child rearing and homemaking even when employed full time (Hochschild 1989; England and Farkas 1986). The tremendous growth of single-parent families has also contributed to the overload on mothers (Garfinkel and McLanahan 1986). While the era of specialization and separate spheres for mothers and fathers is seemingly drawing to a close, the workloads and work commitments demanded of employees have scarcely decreased. In fact, some observers point to an increase in recent years (Hunnicutt 1989).

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How do parents cope? Given the necessity of earning a living, many scholars believe that parents (especially mothers) must limit their occupational choices to those jobs that accommodate outside responsibilities (Feiner and Roberts 1990; Zalokar 1988; Hall 1986; Filer 1985; Polacheck 1981). Thus, mothers self-select into jobs with characteristics or benefits that ease the strain of combining employment and parenthood. This selection process contributes to and encourages occupational sex segregation. In the remainder of this article, we will briefly review the theoretical debate on how domestic responsibilities contribute to occupational segregation and then empirically test the particular contention that jobs become female-dominated because their characteristics reduce job-family conflict.

BACKGROUND

The precise job characteristics thought to be important in accommodating parenting vary in emphasis among different theorists in the neoclassical tradition. The first emphasis, outlined in early work by the economist Solomon Polacheck (1976, 1981) highlighted job structures permitting intermittent labor-force participation with minimal wage loss and depreciation of human capital on reentry. In this view, predominantly female jobs are not necessarily compatible with simultaneous parenting, but are easy to quit and reenter after a period of nonemployment. While variation exists in the wage penalties for intermittency and ease of reentry into occupations, England (1982) and Gwartney-Gibbs (1987) have shown that this variation does not correspond to observed patterns of gender segregation and intermittent employment across occupations.

The second, less articulated accommodation is the availability of part-time work, which is correlated with female concentration in an occupation (Holden and Hansen 1987; Darian 1975). Part-time jobs are more compatible because they require fewer hours away from home, but the costs are great—not just loss of income, but also erosion of wages, benefits, possibilities for promotion, job training, and job security (Corcoran, Duncan, and Ponza 1984; Smith 1978). Some theorists argue that women rationally choose occupations with part-time opportunities to maximize their earnings or minimize depreciation from intermittent employment during the childbearing years. But the effects of part-time work experience on women's wages are nearly identical to the effects of time spent out of the labor force, mostly because part-time opportunities are concentrated in the sectors with the least-skilled jobs (Corcoran et al. 1984). Moreover, the view that women are attracted to occupations that offer part-time work assumes a labor supply of mothers who are able to cut their wages and earnings by trimming their weekly hours. Yet since 1980 no more than 29% of all employed mothers of preschoolers have worked less than 35 hours per week, a figure close to the 26% of all employed women who do so (U.S. Bureau of the Census 1990; U.S. Bureau of Labor Statistics 1989).

Because gender segregation and gender wage differentials persist as strongly in full-time jobs when part-timers are not counted and because most employed mothers work in full-time jobs, some neoclassical theorists have emphasized job accommodations to parenting other than severely reduced hours or exits from employment.[2] Killingsworth (1985) and Filer (1989) contend that compensating amenities that attract women can account both for occupational segregation and for most of the wage gap between men and women who have equivalent amounts of general human capital. In this view, the negative relationship between wages and female concentration in jobs occurs for two reasons. First (using neoclassical assumptions about preferences in an open market), women tend to take jobs that compensate workers with such work amenities as flexible hours or easy work, instead of with wage premiums for onerous working conditions. Second, mothers contribute an oversupply of labor to those jobs that accommodate parenting. Since mothers more often choose (or are forced) to trade wages for job flexibility, they soon dominate employment in jobs that can accommodate them.

The empirical evidence supporting the compensating differential argument is mixed. Early work by Darian (1975) showed that occupations with proportionately more part-time jobs and jobs located closer to their homes retained more women after childbirth. Women are likely to leave predominantly male occupations as they change jobs over time (Jacobs 1987), but are especially likely to do so following pregnancy (Wenk and Rosenfeld 1989). Mothers are also most likely to maintain continuous labor-force participation when employed in occupations that have larger than average proportions of mothers (Desai and Waite 1991). The studies described above indicate that voluntary segregation is at least a theoretical possibility. Yet they could also indicate that there is greater sex discrimination among mothers or that patterns of downward mobility follow temporary labor-force withdrawals. Other evidence (Glass 1990; Bielby and Bielby 1988; Barry 1987) suggests that predominantly female jobs are not more likely to offer flexibility, low levels of exertion, frequent breaks or holidays, and other characteristics that might facilitate parenting. In general, empirical tests of the theory that gender wage differentials can be attributed to the lesser hazards or positive working conditions of female-dominated occupations have shown little support for that argument (Jacobs and Steinberg 1990; Barry 1985).

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[2] These are best viewed as pseudoaccommodations to parenting since they both rely on a strategy of severely limiting or eliminating paid employment rather than integrating paid work with family work, as was done prior to industrial capitalism (Hareven 1982).

There are two problems with the existing research linking occupational segregation to gendered work needs or preferences. First, the presumed preferences of women workers are often vague or only loosely related to the family roles that supply-side theories say propel occupational segregation. Filer (1989), for example, considers any job characteristic that yields a wage premium and occurs disproportionately in male-dominated occupations to be a negative or hazardous condition requiring compensation. Empirical outcomes, rather than logic or theory, determine what are positive and negative working conditions and what male and female workers implicitly prefer.[3]

Second, research on sex-typing and occupational characteristics often deals with the effect of one job characteristic at a time. Jobs, however, come as clusters of characteristics. Some jobs may have flexible schedules or frequent breaks but are also stressful or physically exhausting as well. The combination of characteristics in predominantly female jobs may be more conducive to parenting, even though any one characteristic of them might be equally prevalent in mixed or male-dominated settings. Because the self-selection hypothesis asserts that the total package of characteristics in certain occupations disproportionately attracts mothers, analyses that isolate job characteristics cannot adequately test this hypothesis.

Our purpose in this article is, therefore, twofold: (1) to develop a theoretically informed, operational definition of "compatible" jobs, and (2) to use this measure of compatibility to determine whether compatibility differentially characterizes the jobs held by mothers employed full time, fueling occupational segregation. First, we will describe measures of occupational characteristics and combine them into various clusters. We will assess the validity of these clusters by determining whether reported work-family conflict is reduced for incumbents of jobs high on those characteristics. Finally, we will assess the personal and workplace characteristics associated with incumbency in those compatible jobs by using measures of human capital, family status, and female concentration. This will enable us to test the argument that, ceteris paribus, mothers disproportionately appear in compatible jobs. It will also provide a test of the extent to which job-family compatibility contributes to occupational sex segregation, by assessing the strength of the association between job-family compatibility and female concentration.

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[3] Why some physical requirements (e.g., strength) should command greater compensation than others that are equally sex-typed (e.g., manual dexterity) remains unexplained in this scheme. Moreover, the origins of gender differences in job characteristics such as requirements for authority skills or skill in interpersonal relations are largely ignored or assumed to reflect gendered personal preferences in Filer's explanation.

This analysis will not test the thesis that women are drawn to female-dominated jobs because of the availability of part-time work or intermittent employment without wage penalty. The compensating differential argument emphasizes positive work amenities as the force behind occupational segregation. Women who work full-time are presumably in greatest need of family accommodations because they spend so much time on the job, while part-time workers may be less responsive to these amenities because they work so few hours. Therefore, the sample of interest consists of women and men with comparable, roughly full-time labor supply. Indeed, reducing labor supply may be an alternative strategy for solving job-family conflicts when compatible employment is not available, making labor supply decisions endogenous to the processes determining working conditions in full-time employment.

DATA AND METHODS

The data used to address these issues came from the 1977 Quality of Employment Survey (QES). The 1977 QES continued the survey of working conditions conducted by the Institute for Social Research (ISR) in 1972–73. This nationally representative sample achieved a response rate of 77% and contains 537 women and 944 men (Quinn and Seashore 1979).

The eligibility criteria used in this survey excluded temporary workers, part-time workers working fewer than 20 hours a week, and unpaid workers who receive in-kind benefits (such as room and board). While the effects of these exclusions are not directly known, it is reasonable to suppose that such eligibility requirements both exclude more women than men and exclude workers in jobs of below average quality. These data were collected over a decade ago, but in this case the age of the data may be a comparative advantage. Recent popular attention to job-family stresses has led to some change in employment practices among progressive companies and unions that might muddy the historical distinction between male- and female-dominated jobs claimed by supply-side theorists (Bureau of Labor Statistics 1988). Among a cross-section of workers in 1977, measures of female concentration and the dispersion of women across job categories should be more likely to conform to the presence of compensating amenities, if in fact markets operate according to these neoclassical principles.

Variable Construction

Three variables were created to measure job dimensions from the 74 Likert format job description items asked on the QES. While these data come from employees' self-reports, they are in some ways preferable to more objective assessments from independent observers or summary ratings in the *Dictionary of Occupational Titles* (*DOT*). The latter ratings, while useful, are limited to the most rudimentary measures of work complexity and health hazards. In addition, aggregate mean occupational ratings obscure the diversity among the many jobs in a single occupational category. Finally, while researchers must be wary of response biases and measurement errors in self-report data, there are no guarantees that independent observers will produce less distorted job descriptions. Indeed, sex bias in "objective" *DOT* ratings has been widely commented upon (Treiman and Hartmann 1981).[4]

Since preferences for compatibility dimensions of jobs are believed to encourage gender segregation, we focused on that broad category. Compatibility dimensions refer to those aspects of jobs believed to make the combination of paid employment and domestic labor easier, less stressful, or more manageable. Three scales measuring compatibility dimensions are of particular interest here—"job ease," which measures the extent of work overload and physical/mental exertion in the respondent's job; "flexibility," which measures the rigidity of work rules and work schedules; and "support," which measures the social support received from supervisors and coworkers. These three scales were constructed by selecting from among the 74 job description items those questions that most closely corresponded to the concept being measured and keeping those items with the highest item-to-total correlations. Confirmatory factor analyses showed that the scales adequately fit the data and had reliabilities greater than .70.[5] The items themselves and their scale reliabilities are presented in table 1.

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[4] In a direct comparison of worker self-reports and OSHA records of occupational hazards, Viscusi (1983) reported that subjective self-reports and objective statistics showed a moderate positive correlation. Both measures produced the same relationships to income and other variables in an income determination equation. Smith's (1979) summary of prior work on self-reported occupational data provides additional reassurance, since the factors shown to produce subjective response bias (job dissatisfaction, social background) are gender neutral in this sample. Bielby and Bielby (1988) similarly show that while self-reports of work effort may be biased, those biases are not related to gender.

[5] Reliability coefficients for the flexibility and ease scales computed separately for women and men were slightly lower, but very close in size. The flexibility scale had a coefficient of determination of .64 for women and .62 for men. For job ease, the results were .67 for women and .63 for men.

TABLE 1

CONSTRUCTION AND RELIABILITY OF THE JOB DESCRIPTION SCALES

Scale	Reliability Coefficient	Job Description
Ease of work	.70	1. My job requires that I work very hard. 2. My job requires that I work very fast. 3. I have too much work to do everything well. 4. I never seem to have enough time to get everything done on my job. 5. I have a lot of energy left over when I get off work. 6. I am not asked to do excessive amounts of work. 7. I have enough time to get the job done. 8. My job requires a lot of (physical or mental) effort.
Flexibility	.70	1. I decide when I take breaks on my job. 2. The hours are good. 3. It would be very hard to change the days I wanted to work. 4. It would be very hard to change the hours I wanted to work. 5. It would be very hard to take time off during my work day to take care of personal or family matters.
Support	.74	1. My supervisor is very concerned about the welfare of those under him or her. 2. My supervisor is friendly. 3. The people I work with are friendly. 4. The people I work with take a personal interest in me.

Another compatibility variable, "break time," measured the total amount of unsupervised break time available to the respondent in minutes per work hour. The responses to three questions asking about breaks for meals, rest, or miscellaneous activities throughout the workday were summed, then divided by the usual number of hours worked each day.

While the compatibility dimensions selected for study do not represent an exhaustive list of working conditions, they do cover those aspects of jobs most often discussed in the literature on work-family conflict and occupational segregation (Love, Galinsky, and Hughes 1987; Reskin and Hartmann 1986; Pleck, Staines, and Lang 1980). This list may need to be amended as research continues. For example, Love et al. (1987) reported that employees cited sick leave when family members are ill, the possibility of temporary reductions in hours, home-based work, and on-site child care or referral services, as changes that would reduce work-family conflicts. These characteristics were not addressed by the QES. However, given the paucity of employers that provided any of these benefits in 1977 (see Bureau of Labor Statistics 1988), it is unlikely that any of these characteristics accounted for gender segregation or mothers' occupational choices by 1977.

To control for prior human capital differences among respondents that may affect occupational placement, we used the following variables as controls in most analyses: years of formal education, years of job experience, tenure at the current job, age, and average hours worked per week. Because women are more likely to be employed in smaller firms, a measure of workplace size was also included to avoid misspecifying effects of gender or female concentration in occupations. In this survey, firm size was coded into seven categories (1–7), using a monotonic transformation of the actual number of employees at the work site. This produced an approximately normal distribution and is theoretically sound, since increases at the lower end of the size distribution should have greater impact than those at the upper end.

To assess the validity of the compatibility variables used, a measure of work-family conflict was taken from the survey. This measure was the five-category response to the question, "How much would you say your job and your family life interfere with each other?" To test the compensating amenities argument, variables were developed indicating the respondent's gender (female = 1), parental status (presence of children under six and number of minor children in the home), and the interaction of gender and preschool children.

Because occupational segregation by sex is believed to result from differences in occupational characteristics, we used data from the 1970 census to identify the percentage of women in each of the three-digit detailed occupational categories, which were further subdivided into 37 major industry groups. This information was then merged to the QES survey data to represent the percentage female in each respondent's job. Descriptive statistics on all variables are shown in table 2.

All respondents working over 30 hours a week who were not self-employed or over 65 were included in the analyses. This insures that all comparisons were made on a random sample of preretirement aged, full-time, full-year employees. For all eligible respondents, missing values on items used in the multi-item scales were replaced with the mean on that item for the appropriate sex. Analyses were also performed after all cases with any missing data had been deleted, to insure that no substantive changes could be found. The final sample size was 1,287 cases.

TABLE 2

MEANS BY SEX FOR VARIABLES USED IN THE ANALYSIS

	TOTAL SAMPLE		PARENTS	
VARIABLES	Males (N = 793)	Females (N = 493)	Fathers (N = 427)	Mothers (N = 213)
Hours worked per week	45.20	38.30**	45.88	37.36**
	(10.58)	(8.32)	(9.71)	(8.5)
Years at present job	7.69	5.70**	7.77	4.52**
	(7.40)	(6.17)	(7.08)	(4.99)
Education (in years)	12.48	12.40	12.58	11.98**
	(3.02)	(2.98)	(3.06)	(2.66)
Age	37.54	36.27	36.64	33.99**
	(12.58)	(13.02)	(9.94)	(8.50)
Years of job experience	20.03	14.04**	19.04	11.77**
	(12.66)	(10.53)	(9.83)	(7.11)
% female by industry	.19	.72**	.17	.74**
	(.24)	(.28)	(.22)	(.28)
Size of firm	3.41	3.19*	3.50	3.15*
	(1.92)	(1.75)	(1.95)	(1.67)
Presence of children under 6 years old	.36	.20**	.67	.46**
	(.66)	(.49)	(.78)	(.65)
Total no. of children	1.10	.82**	2.05	1.90
	(1.30)	(1.16)	(1.08)	(1.02)
Marital status	.78	.49**	.96	.65**
	(.33)	(.48)	(.18)	(.48)
Breaks (in minutes per hour)	3.4	3.1	3.5	2.9
	(3.6)	(3.1)	(4.3)	(2.3)
Support	2.89	2.99*	2.89	2.97
	(.66)	(.65)	(.66)	(.65)
Ease of work	1.72	1.71	1.70	1.67
	(.60)	(.59)	(.59)	(.56)
Flexibility	2.39	2.32	2.39	2.30
	(.61)	(.65)	(.60)	(.66)
Maternity leave with pay	...	.29	...	.27
		(.41)		(.40)
Maternity leave	...	.74	...	.73
		(.40)		(.41)
Flexibility and ease (FLEXEASE)	.05	−.08	.00	−.18
	(1.54)	(1.56)	(1.53)	(1.47)

NOTE.—Numbers in parentheses are SDs.
* Gender difference significant at $P < .05$.
** Gender difference significant at $P < .01$.

RESULTS

Construction and Validation of the Job Compatibility Measure

The first step in the analysis involved the construction of an overall compatibility measure. Initial factor analyses showed that the intercorrelations between the variables for ease, flexibility, support, and break times were low, the largest correlation being .23 between flexibility and support. This evidence itself casts doubt on the claim that job characteristics cluster in a way that would make conscious self-selection into compatible jobs an efficient way to reduce job-family conflict. However, the fact that the set of all jobs did not neatly align along these compatibility dimensions was technically irrelevant as long as some jobs aligned more than others. This suggested a strategy of using linear combinations of characteristics to order jobs, regardless of the formal reliability of the resulting combination. For this reason, we transformed each respondent's scale score on ease, flexibility, support, and break time into its corresponding *z*-score to eliminate scale differences and reliably rank-order jobs along each separate dimension.

To judge the criterion validity of the possible combinations of compatibility characteristics, we performed analyses assessing the extent to which each characteristic, singly and in combination, reduced reported job-family conflict. Initially, we estimated an unconstrained model of job-family conflict that included human capital, family status, and the four standardized compatibility variables. Results showed that break time and support, though showing significant zero-order correlations, did not significantly reduce job-family conflict after we had controlled for job ease and flexibility, so we dropped these dimensions from further consideration. We then summed the remaining two standardized variables, flexibility and ease, and compared the regression results with the full model in which ease and flexibility were entered as separate variables. The increment to explained variance in job-family conflict was as large for the linear combination as that for the variables entered separately. The increment for the flexibility and ease combination (hereafter referred to as FLEXEASE) was also large and highly significant relative to the variance explained by the initial 11 human capital and family-status variables (.078 with 1 *df* vs. .107 with 11 *df*, respectively). These results are summarized in Appendix table A1.

Table 3 shows the complete model in which job-family conflict was regressed on FLEXEASE and control variables. Column 1 displays the results for the QES sample of parents of minor children. In addition to the large FLEXEASE effect, women, respondents working more hours, and those in larger firms reported significantly increased job-family conflict. While the gender and work-hours effects were not surprising, firm

TABLE 3

REGRESSIONS OF WORK-FAMILY CONFLICT ON MEASURES OF JOB COMPATIBILITY

	ALL PARENTS		
	Baseline	With Gender Interactions	MOTHERS ONLY
FLEXEASE	−.16**	−.18**	−.15**
	(.02)	(.03)	(.04)
Education	.01	.01	−.004
	(.01)	(.01)	(.02)
Work experience^2	.00	.00	.00
	(.00)	(.00)	(.00)
Job tenure	.00	.00	−.01
	(.01)	(.01)	(.01)
Hours worked/10	.02**	.02**	.02**
	(.003)	(.004)	(.007)
Age	−.00	.00	−.00
	(.01)	(.01)	(.01)
Firm size	.04*	.02	.10**
	(.02)	(.02)	(.03)
Female	.25*	−.74	...
	(.10)	(.62)	
No. of children	.04	.07	−.03
	(.04)	(.04)	(.06)
Presence of preschool children	.05	−.01	.16
	(.06)	(.06)	(.10)
Married	.12	−.66	.22$^+$
	(.12)	(.47)	(.12)
Interactions with gender:			
FLEXEASE	...	.03	N.A.
		(.05)	
Hours worked	...	.00	N.A.
		(.00)	
Firm size	...	.08$^+$	N.A.
		(.04)	
No. of children	...	−.08	N.A.
		(.07)	
Presence of preschool children	...	.20$^+$	N.A.
		(.11)	
Married	...	.82$^+$	N.A.
		(.49)	
R^2	.18	.20	.22
N	599	599	195

NOTE.—Numbers in parentheses are SEs.
$^+$ $P < .10$.
* $P < .05$.
** $P < .01$.

size exerted an unanticipated positive effect on the degree of work-family conflict, especially for mothers (col. 3).

Small firms disproportionately hire women and are apparently also more compatible with family life. This relationship suggests that perhaps the interpersonal relationships and informality of smaller workplaces allow employees more latitude for meeting family needs as they arise. For example, even in companies viewed as progressive in their family policies, employees frequently report that family concessions are still individually negotiated with supervisors or line managers and are difficult to secure outside clerical positions. Perhaps parents are better able to manage these individual negotiations in less bureaucratic work environments. Kingston (1990) shows that small employers are more likely to allow mothers phased-in or part-time returns to work after childbirth, which mothers apparently prefer to long, unpaid leaves, for example. Further, owners and managers in smaller workplaces may be more accessible and more familiar with their employees' needs than those in larger establishments, and this familiarity may foster a paternalistic climate in which family obligations are viewed as legitimate and unavoidable business costs. Small firms may also be more tolerant of individual infractions of work rules, for example, of excessive tardiness or absences, leaving work suddenly or early, since search and replacement costs are proportionately more of a burden for small firms without formal personnel departments. These explanations, of course, await empirical verification.

Given the significant gender effect on job-family conflict, in column 2 of table 3 we tested for the possibility of gender interactions with family status and significant employment conditions, to assess whether mothers and fathers differed in their determinants of job-family conflict. Three interactions were marginally significant and indicated a tendency for mothers of small children, mothers in larger firms, and married mothers to report greater job-family conflict than fathers in similar circumstances. However, FLEXEASE was equally important in reducing conflict for mothers and fathers.[6]

Since home care and child care are culturally assigned to mothers, mothers' reports of job-family conflict are more apt to be an accurate reflection of the difficulty of combining that job with involved parenting. Fathers may report low levels of job-family conflict because of their relatively low levels of family involvement. Therefore, in column 3, we present the main-effects model for the sample of mothers only as a further validity check. Since FLEXEASE continued to show a large and robust negative effect on job-family conflict, subsequent analysis focused on that variable.

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[6] Similar results have been reported by Moen and Forest (1990), who found that the ability to freely leave the workplace (which the authors define as autonomy) had a profound effect on distress experienced by employed mothers and fathers in Sweden.

It should be noted that results in table 3 may be affected by selection bias in the sample of employed mothers. Mothers who had left the labor force by 1977 may have had a different assessment of the job conditions that reduce work-family incompatibility. For this reason, we reestimated the model in column 3 (mothers only) including the responses of the few part-time workers in the QES and including a standard Heckman-type correction for sample-selection bias. The probit selection equation modeled exits from the labor force in the year preceding the 1970 census. Results in the corrected equation showed a slight drop in the FLEXEASE effect (−.13), a large and now significant effect of preschool children (.42), and small increases in the effects of work hours (.03) and firm size (.12).

Analysis of Compensating Amenities

Models estimating the effects of gender and family status on values of FLEXEASE reported by respondents are shown in table 4. Prior human capital differences were controlled in these equations, but uncontrolled results were essentially the same. Results in table 4 on the full sample of QES respondents clearly show that women full-time employees had not self-selected into jobs that were flexible and easy by 1977, nor were there apparent differences between single women, married women, and mothers of preschool children in these characteristics of employment. The large negative coefficient for gender in column 1 indicates that women were substantially less likely than men to be in jobs that were both flexible and easy, contrary to the compensating-amenity hypothesis. Moreover, mothers were not more likely to be in flexible and easy jobs than other women; all women regardless of family status shared the gender disadvantage, as shown in columns 2 and 3. Gender exerted the strongest effect on FLEXEASE of any of the independent variables overall. The other variables significantly affecting FLEXEASE were work hours and job tenure (both negatively) and education (positively).[7]

Finally, table 5 presents the effects of FLEXEASE, human capital, and major occupation sector on female concentration. The dependent variable for these analyses was the proportion female in each respon-

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[7] Models identical with those in table 4 were also run on the disaggregated flexibility and ease scales. Results for both scales were identical, with few exceptions. Age had a positive effect on job ease, while current job tenure had no effect. Years of work experience had a negative effect on job ease. In contrast, years of work experience had a positive effect on job flexibility, while firm size showed a clear negative relationship to flexibility.

TABLE 4

REGRESSIONS OF FLEXEASE ON HUMAN CAPITAL AND FAMILY STATUS VARIABLES

	Model 1	Model 2	Model 3
Education	.02$^+$	.03*	.03*
	(.01)	(.01)	(.01)
Work experience	.01	.01	.01
	(.01)	(.02)	(.02)
Work experience^2	−.00	−.00	.00
	(.00)	(.00)	(.00)
Job tenure	−.02*	−.02*	−.02*
	(.01)	(.01)	(.01)
Hours worked/10	−.03**	−.03**	−.03**
	(.005)	(.005)	(.005)
Age	.01	.01	.01
	(.01)	(.01)	(.01)
Firm size	−.04$^+$	−.04$^+$	−.04$^+$
	(.02)	(.02)	(.02)
Marital status	−.21$^+$	−.22*	−.20$^+$
	(.11)	(.11)	(.11)
No. of children	.003	.002	.01
	(.04)	(.04)	(.04)
Presence of preschool children	−.05	−.01	−.05
	(.12)	(.09)	(.13)
Female	−.43**	−.44**	−.43**
	(.10)	(.11)	(.11)
Female × preschool	...	.06	...
		(.17)	
Female × preschool children × married	...	...	.01
			(.25)

NOTE.—Model 1 is the baseline model; model 2 tests interaction for mothers with young children; model 3 tests interaction for married mothers of young children. Numbers in parentheses are SEs. $N = 1{,}258$. $R^2 = .06$ for all models.

$^+$ $P < .10$.

* $P < .05$.

** $P < .01$.

dent's occupation by industry cell. Given the negative effect of gender on FLEXEASE, it was unlikely that scores on FLEXEASE would predict female concentration in respondents' occupations. Column 1 of table 5 verified this with a model in which FLEXEASE was entered along with controls for prior human capital and workplace characteristics. Flexibility and ease were negatively, rather than positively, associated with incumbency in a predominantly female job.

However, the negative effect of FLEXEASE on female concentration may not be universal but confined to certain occupational sectors. To check, we reestimated the baseline model with dummy variables for ma-

TABLE 5

REGRESSIONS OF FEMALE CONCENTRATION ON MEASURES OF HUMAN CAPITAL, WORKPLACE CHARACTERISTICS, AND MAJOR OCCUPATION SECTOR

	Baseline Model (1)	Occupation Sector (2)	Interactions of FLEXEASE and Sector (3)
Education	.01**	−.00	−.00
	(.003)	(.00)	(.00)
Work experience	−.02**	−.01**	−.01**
	(.003)	(.002)	(.003)
Work experience^2	.00	.00	.00
	(.00)	(.00)	(.00)
Job tenure	−.004*	−.00	−.00
	(.002)	(.00)	(.00)
Hours worked/10	−.01**	−.01**	−.01**
	(.001)	(.001)	(.001)
Age	.01**	.01**	.01**
	(.00)	(.00)	(.00)
Firm size	−.01	−.00	−.00
	(.01)	(.00)	(.00)
FLEXEASE	−.02**	−.03**	−.09**
	(.006)	(.005)	(.01)
Managerial	...	−.19**	−.19**
		(.03)	(.03)
Clerical	...	.23**	.23**
		(.03)	(.03)
Blue collar/operative	...	−.26**	−.26**
		(.03)	(.03)
Service	...	.08*	.07*
		(.03)	(.03)
Interactions with FLEXEASE:			
Managerial	...	...	.08**
			(.02)
Clerical	...	...	.08**
			(.02)
Blue collar/operative	...	...	.05**
			(.01)
Service	...	...	.09**
			(.02)
R^2	.19	.43	.45

NOTE.—Numbers in parentheses are SEs. N = 1,225.

* $P \leq .05$.

** $P \leq .01$.

jor occupation groups (managerial, clerical, blue-collar, and service, with professional as the omitted category), and interactions between FLEXEASE and occupation group. The interactions adjust the negative overall effect of FLEXEASE for jobs in the different occupational sectors, using professional jobs as the reference category. Results in column 3 show that the negative effects of FLEXEASE on female concentration were most pronounced in professional occupations, since all the interactions in table 5 were positive and significant. The negative influence of FLEXEASE on female concentration was least pronounced in managerial, clerical, and service sectors. Blue-collar jobs fell in between; summing the main effect and interaction term still produced a moderate negative effect of FLEXEASE on female concentration in blue-collar jobs. To summarize, the negative influence of flexibility and ease on female concentration was confined almost exclusively to professional and blue-collar jobs. It must be remembered, however, that no sector actually showed a positive relationship between FLEXEASE and female concentration, as predicted by supply-side theory. In other words, with a substantial proportion of mothers already in the work force by 1977, jobs rated flexible and easy by their incumbents had not become female-dominated; in fact, exactly the opposite was true in professional and blue-collar employment.

The evidence presented here raises many questions. Given their levels of human capital, why are mothers unable to obtain those full-time jobs most compatible with parenting? What is the avenue of mobility into jobs conducive to parenting, and where are these compatible jobs located in the economy? Finally, what accounts for the concentration of women, especially mothers, in certain occupations if, in fact, female-dominated jobs generate as much or more work-family conflict as any other?

Preliminary answers to those questions can be found in the descriptive statistics for FLEXEASE. Breakdowns of FLEXEASE across gender and major occupational categories (professional, managerial, clerical, blue-collar, and service) show that clerical and service jobs had generally high compatibility scores, but particularly for women, who are severely disadvantaged relative to men when employed in professional or blue-collar/operative sectors. These results are summarized in table 6.

Looking at the personal characteristics of incumbents of compatible jobs, we see that, in table 7, breakdowns of FLEXEASE by gender, parent status, and employing firm size show that men without dependent children had the highest overall mean score (.11) while women with dependent children had the lowest (−.18).[8] The difference is not large,

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[8] Because demands on parents' time and energy are greatest when children are young, additional breakdowns compared parents of preschoolers with parents of older children and nonparents. Those results are quite similar to the results for all parents.

TABLE 6

MEANS FOR FLEXEASE BY GENDER AND MAJOR OCCUPATION SECTOR

SECTOR	MEN			WOMEN		
	Mean	SD	*N*	Mean	SD	*N*
Professional/technical	.08	1.46	137	−.38	1.56	98
Managerial	.04	1.66	103	.48	1.39	23
Clerical	.20	1.56	50	.24	1.37	163
Blue collar/operative	−.06	1.56	403	−.78	1.57	88
Service	.38	1.64	100	.13	1.64	121

NOTE.—Two-way analysis of variance showed significant main effects of gender and occupation sector and a significant interaction effect, all at $P < .01$.

but is the direct opposite of the order expected in the presence of compensating amenities. The impact of firm size on job compatibility among women is clear. Mothers, especially, show a dramatic decline in job compatibility when employed in large firms (defined here as establishments with more than 100 employees).

TABLE 7

MEAN VALUES FOR FLEXEASE BY GENDER, PARENT STATUS, AND SIZE OF EMPLOYING FIRM

FLEXEASE	Total	FIRM SIZE	
		< 100 Employees	100+ Employees
Women with dependent children:			
Mean	−.18	.01	−.45
SD	1.47	1.40	1.53
N	213	129	84
Women without dependent children:			
Mean	−.01	−.13	−.21
SD	1.68	1.67	1.55
N	280	168	112
Men with dependent children:			
Mean	−.00	−.01	.01
SD	1.53	1.48	1.59
N	427	230	197
Men without dependent children:			
Mean	.11	.14	.06
SD	1.55	1.54	1.58
N	366	206	160

DISCUSSION

The analyses performed here lend little support to the assertion that female-dominated jobs are chosen by women because of their greater compatibility with current or anticipated family demands. In fact, exactly the opposite was found in professional and blue-collar jobs where female concentration was negatively related to compatibility. Employer discrimination is implicated, but its precise form and causal logic need further exploration. One approach consistent with these results emphasizes the central managerial imperatives of maximizing efficiency and profit accumulation. These goals deter employers from placing women in positions that enable them to reduce work-family conflicts. Job flexibility, lax supervision, and control over the pace and timing of work-related tasks are potentially expensive concessions to employees, typically given as rewards or incentives for high productivity or workplace commitment. Employers may fear women would take advantage of the increased flexibility given to them in ways that men without outside competition for their time and interest theoretically would not.[9] It is clearly in the interests of employers to segregate responsibility for family care outside the domain of the employer, a point made by early Marxist feminists (Sokoloff 1980). Without perceived family competition for their time and energy, men then become preferred workers for positions with authority or control. In short, autonomy is given to those workers thought least likely to behave in ways that are incompatible with managerial or organizational goals. This has long been recognized as one of the primary rationales behind statistical discrimination in the placement of women in certain job categories within organizations (Bielby and Baron 1984).

The very job characteristics that would reduce stress and job-family tension among employed mothers are difficult for them to obtain because these rewards are linked to an authority and reward structure that places women in marginalized "women's jobs" outside central lines of authority. Bolstering this interpretation, other researchers have found that, within organizations, women are more likely to be closely supervised than men (Wolf and Fligstein 1979) and average over an hour and one-half less rest time at work per week than men (Stafford and Duncan 1978). Flexibility in predominantly female jobs may also be difficult to implement because those jobs are often structured to meet the needs of others higher up in the organizational hierarchy (Kanter 1977). Support personnel are often called upon to be responsive to other people's schedules, making flexibility and autonomous performance in those jobs difficult.

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[9] There is at least anecdotal evidence to suggest this. At the Bureau of National Affairs Conference on Work and Family Life in 1987, the personnel manager of a large insurance company stated that the company was interested in instituting flexible schedules, parenting leaves, etc., but feared that doing so would attract too many young mothers to the firm. This company already had a predominantly female work force. But in the absence of a competitive labor market where *all* employers would provide such services, this manager feared that instituting family policies would draw "beneficiaries" to his firm while letting other employers escape the costs of hiring mothers. This also explains why there is a slight negative relationship between female concentration and the provision of maternity leave with or without pay in the QES.

Rather than making mothers' lives easier, gender segregation appears to worsen their working conditions. Future empirical work should focus specifically on why this happens, as well as on how employed mothers attempt to ameliorate this disadvantage.

APPENDIX

TABLE A1

Variables in Model	R^2	R^2 Change/*df*
Controls only*	.1079	...
Controls + flexibility	.1557	.0478/1
Controls + ease	.1529	.0451/1
Controls + support	.1220	.0141/1
Controls + break time	.1134	.0056/1
Controls + flexibility, ease, support, and break time	.1861	.0782/4
Controls + flexibility, ease	.1847	.0768/2
Controls + FLEXEASE	.1847	.0768/1

* Controls include hours worked, tenure, job experience, age, education, firm size, marital status, number of children, the presence of children under six years of age, and gender.

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The Sociological Concept of "Group": An Empirical Test of Two Models[1]

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Two models of the structural form of small, informal groups are compared. One, derived by Winship, requires that patterns of social affiliation be strictly transitive. The other, based on Granovetter's ideas about weak and strong ties, requires only a special limited form of transitivity. When these alternative models are tested with data on human interaction, it turns out that the Winship model does not fit the data but that the model developed from Granovetter's work does.

I. INTRODUCTION

Over the years sociologists have distinguished various kinds, or what Simmel (1902) called "forms," of human groups. Among these, one form in particular has continued to interest investigators for more than a century. Groups that are relatively small, informal, and involve close personal ties—those that Tönnies ([1887] 1940) characterized as based on *Gemeinschaft,* Durkheim ([1893] 1933) portrayed as reflecting *solidarité organique,* and both Spencer ([1897] 1933) and Cooley (1909) described as *primary*—remain at the core of the discipline.

I am concerned here with groups of this kind, in which individuals are linked by regular interaction and by positive sentimental ties. In particular, I focus on the problem of specifying their structure.

Numerous attempts have been made to specify the structure of such groups in exact terms (Luce and Perry 1949; Festinger 1949; Luce 1950; Davis 1967; Alba 1973; Winship 1977; Seidman and Foster 1978; Mokken 1979; Seidman 1983*a,* 1983*b;* Borgatti, Everett, and Shirey 1990). None of these attempts, however, has succeeded in capturing sociologists' intuitive ideas about group structure. Typically, the groups uncovered by these models overlap too much to satisfy intuition. Moreover, they fail

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to display the internal structure that groups are generally believed to possess.

Homans (1950, p. 85) spelled out the sociological intuition about overlap. He suggested that, from an individual-centered perspective, groups would almost certainly overlap. Any particular individual would, for example, be likely to be a member of a group at work and another at home in a family setting. But Homans argued (see also Feld 1981) that sociologists do not focus on individuals, but on contexts. Sociological groups are located in particular contexts, like work or home, and are limited to particular time frames, like working hours or evenings. When groups are defined in terms of contexts, they usually overlap very little if at all. This view is widely shared: similar arguments about overlap have been made by Alba (1973), Alba and Moore (1978), Seidman and Foster (1978), Mokken (1979), King and Nakornchai (1982), Seidman (1983*a*, 1983*b*), Yan (1988), and Arabie and Carroll (1989).

Davis, Gardner, and Gardner (1941, p. 150) outlined the sociological intuition about the internal structure of groups. In their study of groups of Southern women, they distinguished between "core members" who participated together most often, "primary members" who participated on some, but not all, occasions, and "secondary members" whose participation was infrequent. This kind of characterization of group structure is common (Lewin 1951, pp. 146–47; Homans 1950, p. 85; Shaw 1981, p. 8; Romney and Faust 1982).

But most attempts to specify group structure represent interpersonal linkages in binary, or on/off, terms. Such binary models are unable to capture the variation in peoples' tendencies to get involved with the group. Festinger (1949), Lévi-Strauss (1963), Hubbell (1965), Doreian (1969), Lorrain and White (1971), Alba (1973), Peay (1974, 1976), Seidman and Foster (1978), Marsden and Laumann (1984), and Yan (1988) have all commented on the limitations of binary models of group structure.

What is needed, then, is a representation that both eliminates overlap and permits the display of the internal structure of groups. Here, I describe and test two such models.

Of the models so far introduced, only the one proposed by Winship (1977) and later by Yee (1980) meets both of these conditions. I begin, in Section II below, by reviewing that model. In Section III, I show that Granovetter's (1973) ideas about weak and strong ties can also be used to develop a model of group structure that meets both of these conditions. There I derive a new model that provides an alternative to the one defined by Winship. Finally, in Section IV, I draw upon some available data to determine whether either of these competing models is satisfactory when it comes to representing the structure of empirical groups.

II. WINSHIP'S MODEL

As one of a family of distance models, Winship introduced a representation that specifies exactly the conditions that partition people into nonoverlapping groups and at the same time permits the display of internal group structure. It begins with a set of individuals, $P = \{x, y, z, \ldots\}$. These individuals are partitioned into a hierarchical structure by forming a nested sequence of k distinct levels of equivalence classes, $E'_i, E''_i, \ldots$, $i = 0$ to k. At any given level i, the elements in P are partitioned in the usual way, so that E'_i, E''_i, and so on are mutually exclusive and collectively exhaustive. Thus, at any given level, any pair of objects x and y are either equivalent or they are not.

But in addition, in a hierarchical partitioning the equivalence of a pair of elements at some level, $(x, y) \in E''_i$ implies that they are equivalent at every higher level, $j > i$. Thus, two hierarchical equivalence classes at different levels, E'_i and E'_j cannot overlap except by containment; if

$$E'_i \cap E'_j \neq \varnothing,$$

then either

$$E'_i \subseteq E'_j,$$

or

$$E'_j \subseteq E'_i.$$

A hierarchical structure of partitions yields a tree—an ordered sequence of equivalence classes. At the lowest level, E_0, each object is equivalent only to itself: each person stands alone as an individual. Equivalence classes define nested groups at intermediate levels. And, at the highest level, E_k, all objects are defined as members of a single equivalence class or overarching group.

In order to construct this kind of hierarchy we would need a quantitative measure of people's social affiliation—some sort of *social proximity function.* Let s be a social proximity function, a *mapping* from each ordered pair $\langle x, y \rangle \in P$ to an element in the real numbers in the interval between 0 and 1. Thus, for each ordered pair of elements $\langle x, y \rangle$ in $P \times P$, there is an associated real number, $0 \leq s_{xy} \leq 1$, based on some observable index of the frequency, amount, or depth of affiliation between person x and person y; it is a measure of how close x and y are.

Social proximities can be used to organize a collection of people into hierarchical partitions only if those proximities are arranged in a form called *ultrametric.* And, to be ultrametric, the function s must meet three conditions:

$$s_{xy} = 1 \Leftrightarrow x = y, \qquad (1)$$

each person must be closer to himself or herself than to anyone else;

$$s_{xy} = s_{yx}, \tag{2}$$

the proximity of one person to another must be the same as the proximity of that other to the original person;

$$s_{xy} \geq \min\{s_{xz}, s_{yz}\}, \tag{3}$$

in any triple of persons no pair may be less proximate than the minimum of the other two pairs.

This third condition can be understood as a generalized prohibition against intransitive triples. Consider a general procedure for dichotomizing s. One can begin with $s = 1$ and work down progressively until one hits its minimum value. At each level $s = i$, one can construct a binary relation R_i such that $s_{xy} \geq i \Leftrightarrow (x, y) \in R_i$; that is, only the pairs of people that are socially proximate at a level greater than or equal to i are in the relation at that level. Thus, there are as many dichotomizations as there are levels of s.

Condition (3) is no more and no less than a statement of the rule that no set of three individuals at *any* level of s can exhibit intransitivity. Specifically, it prohibits the case in which, at a given level, an individual x is linked to another y, y is linked to a third z, but x and z are not linked.

Winship's model, then, provides an explicit characterization of the intuitive concept of nonoverlapping groups with internal structure. It embodies the key ideas of the traditional sociological conception. And, at the same time, it yields an explicit, testable hypothesis about the patterning of social proximities.

Winship's model, however, is not the only formal model that can capture the essential features of the sociologists' intuitive notion of groups. In his article on weak and strong ties, Granovetter (1973) introduced an alternative conception of the patterning of proximities. In the next section I review that conception and use it to develop an alternative model of group structure.

III. A MODEL BASED ON WEAK AND STRONG TIES

Granovetter's ideas about the patterning of affiliation grew out of a statement by Rapoport (1954) that "the likely contacts of two individuals who are closely acquainted tend to be more overlapping than those of two arbitrarily selected individuals." Granovetter (1973) extended this idea. He began with Rapoport's notion of "closely acquainted" and distinguished between what he called "strong" and "weak" affiliative ties linking people. By definition, strong ties are those in which individuals are closely affiliated, while weak ties involve less intimate connections.

Like Winship (1977), Granovetter was concerned with transitivity. But Granovetter qualified that notion. Following Rapoport's reasoning, he proposed that if an individual is *strongly* tied to two others, the two others should be *at least weakly* tied to each other; he was arguing, in effect, that a friend of a friend should be *at least* an acquaintance.

Granovetter went on to suggest, moreover, that when one or both of the ties linking an individual to two others is weak—when they are only casual acquaintances—there will be no such pressure toward linking the two others. At this level, intransitive triples would be expected to occur.

Granovetter's ideas about tie strength and social structure can be modeled in a way that parallels Winship's model of hierarchical partitioning. Again let $P = \{x, y, z, \ldots\}$ be a collection of individuals. And again define an index of affiliation-based social proximity s as above:

$$s_{xy} = 1 \Leftrightarrow x = y, \tag{4}$$

$$s_{xy} = s_{yx}. \tag{5}$$

But while the Winship model prohibited *all* intransitive triples, a model based on Granovetter's conception would prohibit only those containing two strong ties and one complete lack of tie. Thus, we would not expect the social proximities to be ultrametric, and condition (3) would not apply.

Instead, I will substitute a condition that prohibits some, but not all, intransitive triples. There must be some constant ℓ that divides weak and strong ties. Thus, there can be no triples of three individuals x, y, z in which x and y are tied more strongly than that constant, y and z are tied more strongly than that same constant, and x and z have no tie at all. So, based on Granovetter's conception, we can specify the required alternative:

$$\exists s = \ell \mid s_{xy} \geq \ell,$$

and (3′)

$$s_{yz} \geq \ell \Rightarrow s_{xz} \neq 0,$$

where $s_{xy} = 0$ if and only if x and y have no affiliation.

This condition captures Granovetter's weaker version of Winship's prohibition against intransitive triples. I call triples that violate this rule G intransitive. The problem, of course, is to determine whether an appropriate value of ℓ exists so that Granovetter's condition is met.

To do this, one can again use the general procedure for dichotomizing introduced above. I proceed from top to bottom as before, beginning with $s = 1$ and dichotomizing at successively weaker levels of affiliation.

If this model is correct, the top levels—where the ties are strongest—will not exhibit any G intransitivity. We can continue working down, then, until we can proceed no further without encountering a G-intransitive triple. At that point, we have defined the critical level ℓ that distinguishes between strong and weak ties. Any pair of individuals who are linked at any proximity greater than or equal to ℓ are strongly tied. Any pair who are at any proximity less than ℓ, but greater than 0, are weakly tied. And the others are not tied.

If we dichotomize at level ℓ, and draw a graph at that level, we can see that some pairs of points are adjacent; they are directly connected by a line. Other pairs may be indirectly linked by a *path* beginning at one point, passing through one or more intermediaries, and finally connecting with the other. All points that are connected, either directly or indirectly are *reachable* from each other.

So, at level ℓ, the set P is partitioned into *components,* or subsets within which all points are reachable from each other. And, of course, components cannot overlap. No member of any component is linked to (is affiliated with) anyone outside that component at a level as great as ℓ. Therefore, as was the case for Winship's partitions, these subsets are nonoverlapping. And, as was the case for partitions, each component will contain one or more points. Only here, unlike partitions, the points in a component are not necessarily all directly connected to each other; here there may be triples that are—strictly speaking—intransitive.

But from the way we constructed the dichotomy it is clear that condition (3′) holds and that none of these intransitive triples is G intransitive. One could simply add in the weak ties, those in the range $0 < s < \ell$, and that would automatically eliminate all of the intransitive triples created by the strong ties; all the missing third lines would be filled in. Thus, we know that in any subset that contains more than one individual, each subset member is related to (is directly affiliated with) *at least one* other subset member at a level $s \geq \ell$ and with *every* other member of that subset at a level $s > 0$.

But if one really did add the weak-tie lines to the graph, not only would one eliminate intransitive triples created by the strong ties, but one would take the risk of creating new intransitive triples. Indeed, in his own treatment of this structure Granovetter stressed the importance of intransitive weak ties as communication "bridges" linking the components described above.

So here then is an alternative model of group structure. Like Winship's model it captures the essential features of the intuitive conception of groups. And, also like Winship's model, it embodies a testable hypothesis about the patterning of affiliation. This new model makes less stringent demands on the data than those made by the Winship model. Nonetheless, it does require that there be no *G*-intransitive triples among the individuals who are affiliated at the closest levels. And that is a testable proposition.

Winship himself suggested that the demand for complete transitivity required by his model implied social arrangements that were not "typical." But that question, and the parallel one for the Granovetter-based model, are essentially empirical problems. They can be addressed only by testing the models with systematic data on social proximities. Only by examining such data can we determine whether the conditions specified by either of these models are seen in actual human social arrangements. That is my next task.

IV. A SYSTEMATIC TEST OF THE TWO MODELS

In order to test whether either of these models corresponds to the human social world, we need systematic information about people's affiliation patterns in particular social contexts. We know that affiliation includes both interaction and positive sentiment, but it is generally agreed that interaction is a necessary, if perhaps not sufficient, condition for affiliation (Cooley 1909, p. 24; Homans 1950, p. 84; Olmstead and Hare 1978, pp. 10–11; Shaw 1981, p. 8). For the present purpose, then, I searched for data sets that provided records of relatively long-term interaction.

I was able to locate seven such data sets: (1) Old South (Davis, Gardner, and Gardner 1941), (2) Lunch (Freeman 1987), (3) Beach (Freeman, Freeman, and Michaelson 1988), (4) Karate (Zachary 1977), and (5) Frat, (6) Tech, and (7) Office (Bernard and Killworth 1977; Bernard, Killworth, and Sailer 1980). These seven data sets represent a broad spectrum of different ways of recording social proximity. However, they are all based—directly or indirectly—on tabulations of interaction. The ways in which interaction was observed, the ranges of variation in interaction, and the numbers of individuals involved are all summarized in figure 1.

The Winship model specifies that there be *no* intransitive triples at any level of interaction. If any are observed, we should reject the model. In practice, of course, there is apt to be a certain amount of error in observing and recording the data. That being the case, we should probably be willing to accept a small number of intransitive triples in any particular data set. It is difficult to specify exactly how many intransitive triples would be acceptable, but their number should certainly be relatively small.

Counts of the numbers of intransitive triples at each level for each of the seven data sets are shown in figure 2. On the face of it, the numbers of intransitive triples recorded in the table are so large that we simply cannot reasonably conclude that they stem from errors in observing or

Data Set	Number	Range	What was observed
Old South	18	0- 7	Number of informal social events jointly attended
Lunch	28	0- 10	Number of informal lunches jointly attended
Beach	52	0-205	Number of minutes observed in conversation
Karate	34	0- 7	Number of contacts observed in non-club contexts
Frat	58	0- 30	Number of conversations observed in the public areas of the house
Tech	34	0- 14	Number of conversations observed in the workplace
Office	40	0- 16	Number of conversations observed in the workplace

FIG. 1.—Numbers of individuals, ranges of variation, and what was observed in seven data sets.

recording data in a situation where there is no intransitivity. Indeed, the data suggest quite dramatically that human interaction frequencies do not fit the Winship model.

As they are represented by Winship's model, then, groups simply do not appear in the world of experience. The available data on interaction suggest that intransitivities are always present and present in considerable number.

Let us now turn to the alternative model based on Granovetter's weak and strong ties. It will be easiest to illustrate how this model may be tested by beginning with an example. The Old South data provide records of women's coattendance at a series of small, informal social events. We will examine these data from the perspective of this model.

None of the triples of women who coattended four or more social events is G intransitive. There are, however, G intransitivities in the triples of women who coattended three or fewer events. In this case, then, $\ell = 4$, and I will consider any pair of women who coattended four or more events to be strongly tied.

Figure 3 shows the strong ties linking these 18 women. In the figure there are two large groups and three singletons. If we compare this structure with that described in the ethnographic report, it turns out that the group containing women 1–9 (with the exception of woman 8) corresponds to Davis, Gardner, and Gardner's Clique I. Moreover, the group made up of women 10–16 corresponds to their Clique II.

Strength	Number	
6	2	
5	0	
4	28	Old
3	76	South
2	201	
1	146	

Strength	Number	
9	0	
8	2	
7	2	
6	7	
5	23	Lunch
4	31	
3	30	
2	44	
1	283	

Strength	Number	
10	0	
8	2	
7	11	
6	28	
5	43	Tech
4	81	
3	145	
2	269	
1	1038	

Strength	Number	
13	0	
12	0	
11	1	
10	3	
9	6	
8	12	
7	12	Office
6	18	
5	35	
4	103	
3	257	
2	579	
1	1764	

Strength	Number	
5	7	
4	33	
3	123	Karate
2	324	
1	393	

Strength	Number	
30	0	
26	0	
25	0	
23	2	
20	2	
19	8	
18	7	
17	11	
16	15	
15	30	
14	57	
13	58	
12	108	Frat
11	154	
10	238	
9	324	
8	422	
7	639	
6	949	
5	1443	
4	2272	
3	3863	
2	6598	
1	10013	

Strength	Number	
130	3	
121	6	
110	7	
109	11	
90	11	
86	16	
83	22	
81	27	
79	35	
78	35	
76	44	
75	44	
73	45	
69	45	
67	47	Beach
66	50	
58	50	
55	52	
52	50	
46	55	
45	65	

Strength	Number	
44	65	
43	69	
42	71	
40	72	
39	76	
36	84	
35	108	
34	109	
33	125	
32	156	
31	159	
30	173	
29	196	
28	209	
27	209	
26	214	
25	218	
24	252	
23	265	Beach
22	273	
21	284	
20	316	
19	339	
18	361	
17	397	
16	423	
15	491	
14	505	
13	508	
12	586	
11	622	
10	665	
9	730	
8	793	
7	853	
6	998	
5	1254	
4	1454	
3	1730	
2	2110	
1	2897	

FIG. 2.—Tie strength and numbers of intransitive triples in seven data sets

It is clear that the structure shown in figure 3 corresponds quite closely to that described in the ethnographers' accounts. As they are presented, however, the groups of figure 3 include triples that are, strictly speaking, intransitive. The triple containing women 3, 7, and 9 is a case in point. But it must be remembered that, though they are intransitive, they are not *G* intransitive. Thus, there is a weak tie linking woman 7 and woman 9. And, correspondingly, there are weak ties linking every unlinked pair of points within each of the groups in the figure.

But, if we add *all* the weak ties, we not only complete the two groups, we introduce a huge number of bridges between them and between the isolates and the groups. The weak ties are shown, along with the strong

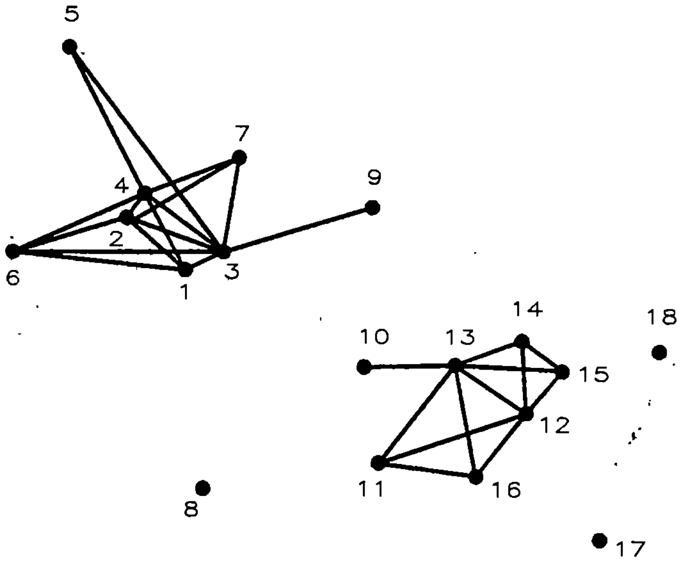

FIG. 3.—Strong ties in the Old South data set

ones, in figure 4. The Old South women are organized into two groups, but those two groups are intimately linked together by the weak ties that bridge both groups. Overall, then, the model suggested by the Granovetter conception of groups seems to be able to uncover the structure of groups in the Old South data quite well.

This same procedure was applied to all seven of the data sets used here, and the applications met with varying success in uncovering group structure. Acceptable triples are those that meet the Granovetter criterion, the one in which two pairs are linked by strong ties ($s \geq \ell$) and the third pair is linked by at least a weak tie ($s > 0$). To the extent that such triples are found at high interaction levels, the data fit the model. But if those individuals who are involved in the most frequent interactions are linked in a way that exhibits unacceptable (G-intransitive) triples, the data do not contain groups in the Granovetter sense.

Table 1 shows the numbers of acceptable triples at each proximity level (working down from the highest level) for each of our seven sets of data. Four of the data sets, Old South, Frat, Lunch, and Beach, display relatively large numbers of acceptable triples before an unacceptable one is found. But two data sets (Tech and Karate) have G-intransitive triples at even the highest levels of social proximity. And, in the case of the Office data, only four acceptable triples are recorded before a G-intransitive triple is encountered.

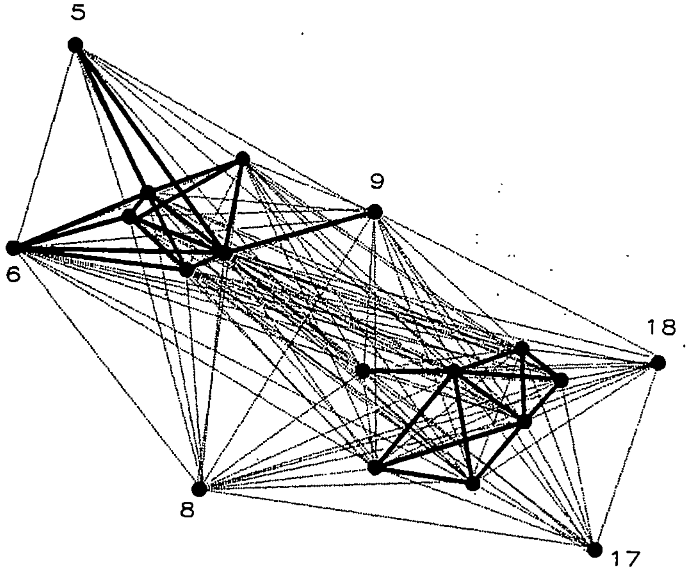

FIG. 4.—Strong and weak ties in the Old South data set

Since we can count the total number of acceptable triples for each of our data sets at all levels, we can evaluate the magnitudes of the numbers in table 1. For any particular data set, we can calculate the probability that a randomly selected triple is acceptable. Then, by multiplying these probabilities, we can calculate the probability of encountering a series of acceptable triples as long as the one observed, by chance alone. As the table shows, these probabilities are dramatically small for the Old South, Frat, Lunch, and Beach data sets. But the Office, Tech, and Karate data

TABLE 1

NUMBER OF ACCEPTABLE TRIPLES TABULATED BEFORE ENCOUNTERING A *G*-TRANSITIVE TRIPLE

Data Set	No. of Triples
Old South	62**
Office	4
Frat	108**
Tech	0
Lunch	51**
Beach	104**
Karate	0

** $P \leq .01$.

sets all yield probabilities too large to be significant. Thus, the model based on Granovetter's conception of weak and strong ties reveals groups in some, but not all, cases.

V. CONCLUSIONS

The results of confronting the Winship model of group structure with data on human interaction are unambiguous. Interaction is simply not patterned in the neat way suggested by that model. But the results of confronting the Granovetter-based model with data show that the model reveals groups in at least some of the data sets.

Marsden and Campbell (1984) have provided a clue that may help to explain this result. They have proposed that the kinds of records of interaction frequencies used here provide poor measures of social affiliation. They argue that interaction frequencies are inadequate because they tend to reflect the effects of external constraints. People who are neighbors or co-workers, they suggest, may interact frequently, but their interaction need not embody the sort of intimacy envisioned in the group concept.

Whether this is a problem or not probably depends on the context in which interaction frequencies are observed. When the context permits observation of interaction that is voluntary and informal and ranges over a broad spectrum of activities, there is no reason to suspect that the individuals involved are interacting in terms of external constraints.

Indeed, if we assume that interaction frequencies provide a reasonable index of social affiliation only in settings where interaction is relatively unconstrained, the present results make sense. The Old South, Frat, Lunch, and Beach data all exhibit a clear group structure. And they are all based on records of interaction in informal, voluntary settings. In contrast, the Office and Tech data sets do not exhibit group structure. Both sets are based on observations of interaction in the workplace, where we would expect a good deal of interpersonal interaction to be determined by the formal tasks of the organization.

The fact that groups are not present in the Karate data is more difficult to interpret. On one hand, the data consist of records of the number of different nonclub contexts in which the members of the karate club were observed together. This would suggest that the data should embody patterns of informal voluntary interaction. But, on the other hand, the ethnographic report stresses the fact that, during the time the data were collected, the club members were involved in a major conflict that ultimately led to its breakup. The poor fit observed here might be a consequence of the fact that outside contacts among the club members were made in an instrumental effort to further organizational interests.

In any case, the model based on Granovetter's ideas does reveal groups in more than half of the data sets. And the cases in which it does are exactly those in which there are no evident constraints on interaction. These results suggest that Homans (1950) was probably wrong when he proposed that sentimental ties *necessarily* emerge from sustained interaction. Indeed, the amount of correlation between interaction frequency and strength of sentimental links between persons might be expected to vary with setting. In settings that are entirely free from constraints, the correlation is likely to be high; in those where constraints loom large, the correlation should be low.

Moreover, the patterning of interaction appears to be more complex than that envisioned by Homans (1950, p. 84) when he argued that *all* group members interact with other group members more than they do with *any* outsider. But here we have shown that nonoverlapping groups can be specified in such a way that individual group members may interact as much or more with outsiders than they do with some of their fellow group members. The important feature of the groups specified by the Granovetter model is that any group member interacts more *with at least one* fellow group member than with any outsider.

Finally, the formal model introduced here reveals an important implication of Granovetter's original statement. In his own treatment, the discussion of tie strength seemed to be somewhat vague. He distinguished between "strong" and "weak" ties, but did not suggest how we might determine which was which in any given situation. The model developed here, however, shows that a systematic way to distinguish between weak and strong ties was already implicit in his original formulation.

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Book Reviews

Crime and the American Press. By Roy E. Lotz. New York: Praeger, 1991. Pp. xv + 192. $39.95.

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Crime and the American Press is a reminder that crime and its control are constituted by social institutions as well as the criminal law. Roy E. Lotz focuses on how the mass media, and newspapers in particular, constitute both crime and the roles of legislators, police, courts, and prisons in dealing with it. He recognizes that most people most of the time experience crime and its control vicariously through its dramatization in the mass media rather than through direct experience.

Lotz combines content analyses of newspapers with secondary literature on mass media, politics, and criminal justice. He begins by documenting that, regardless of market orientation, American newspapers are replete with stories related to crime. For example, crime appears more frequently on front pages than any other single topic. Lotz speculates on the reasons for this by considering how crime news fits the social organization of news work (e.g., the police provide copy routinely) and of cultural criteria (e.g., crime is informative and entertaining). On the basis of a content analysis of selected newspapers from 1895 to 1915, he argues that front-page stories on crime were as frequent then as now, even though the crime rates were much lower, to emphasize that something other than the level of police-recorded crime must explain high rates of crime reporting.

Following an excursion into the secondary literature on the effects of television on behavior, Lotz considers possible sources of bias in crime news that derive from the routines of news work. He attributes bias to news values, format requirements, and source selection. He then uses a content analysis of news items in three newspapers to decipher whether there is a left-liberal bias in news of crime control, and in political reporting more generally, and decides that there is not. A similar effort to detect bias among columnists writing for a number of newspapers from the mid-1970s to the mid-1980s concludes that, except for occasional hints of left-wing sentiment, right-wing views predominate.

Lotz reports a content analysis of various newspapers published in 1986 to show that, regardless of the city in which they were published or their market orientation, they all employ similar key words and attendant stereotypes about crime and criminals. He also notes the lack of explanations of crime in the news media.

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Lotz's final consideration is newspaper accounts of the courts and prisons. He documents that coverage of prisons is more extensive than previous research has indicated and observes that prison news coverage focuses more on prison as an institution and is therefore more sociological than court coverage, which maintains a single case, event orientation.

This book has many limitations. Much of the analysis is based on the peculiar problematic of whether news mirrors reality. Lotz juxtaposes long reviews of criminological knowledge with his readings of newspaper content on the topic to make judgments about whether popular culture mirrors what we know from social science. For example, he offers a nine-page, textbook-like review of the sociology of prison life, followed by six pages on whether some of the aspects identified in the academic literature are evident in news accounts. The value of such an exercise is not clear. In any event, why would anyone expect the cultural products of the mass media to reflect the social reality of crime and law-enforcement processes as depicted in social science? Social science and the mass media are different cultural institutions—each with its own routines, formats, and criteria of rational acceptability—which inevitably produce quite different symbolically constituted realities of crime and its control. The juxtaposition of social science and news accounts offered by Lotz provides no insight into the institutional routines and classifications of news operatives compared with social scientists who might explain their different renderings of crime and criminal justice.

Beyond the thread of the mirror hypothesis running through the book, there is no systematic theory or framework. Much important sociological theory and research on the news media is not addressed, for example, the work of David Altheide, John Fiske, Stuart Hall, Joshua Meyrowitz, and Philip Schlesinger, among others. In place of a rigorous development of a thesis, we are given myriad popular notions about crime and the press from narrowly selected previous research and the author's speculative readings of content. The author frequently diverges on tangents that have nothing to do with newspapers (e.g., chap. 3 focuses on psychological research literature concerning the effects of television coverage of crime and violence) or with crime (e.g., chaps. 2 and 4 include consideration of reporting on U.S. presidents and reporting on the Vietnam War). Rather like in the news itself, we are offered bits and pieces, a mosaic, but not analytical rigor.

Lotz's empirical materials are very limited. Whether quantitatively assembled or qualitatively presented, the data provide only his own speculative readings of content; speculative because he offers no overall theory of significance for what he decides to include. In some cases this speculative approach means that Lotz fails to answer the very question he poses. For example, chapters 5 and 6, in which he attempts to read dominant ideology from content, cannot offer any insight into whether newspapers are saturated with particular dominant mentalities and sensibilities that are accepted by people and thereby effect hegemony. An adequate answer to this question requires empirical research on the activities of mass media operatives, their sources, and their readers regarding how hegemony is accomplished. That is, the question begs analysis of the social contexts of mass media production and reception and of a view of dominant ideology as the outcome of strategies and struggles rather than as an a priori effect of preordained features of news.

Lotz blames newspapers for shortcomings that are also evident in his own work. For example, he complains that a newspaper article he quotes uses a "sample of one" to make an argument and does not include systematic trend data (p. 152), but he himself often uses a single newspaper article as "a sample of one" in analysis. Lotz also makes assertions without supporting evidence. For example, he says that "newspapers in the rest of the world pursue crime less tenaciously and write about it less extensively" than American newspapers (p. 2), but he offers no data or research literature to support his claim. He states that "newspapers receive about 10 times as much information as they can process," (p. 10) but offers no operational definition of "information," let alone supporting evidence, to help us understand how it might even be possible to count information in such terms. Assertions such as these leave the reader with the impression that he is reading a newspaper, not sociology.

Developing Variations: Style and Ideology in Western Music. By Rose Rosengard Subotnik. Minneapolis: University of Minnesota Press, 1991. Pp. xxxiv + 372. $16.95.

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Musicologist Rose Rosengard Subotnik explores the ground between critical musicology and the sociology of music. It is only in the presence of such challenging work as hers that the theoretical issues of this developing and interdisciplinary field can be considered seriously. *Developing Variations* spans 12 years of essays, some previously published, on topics such as Adorno's 19th-century music criticism, musical semiology, individualism in Western art music, and critiques of Mozart and Chopin. In the process, Subotnik references Kant, Adorno, and Lévi-Strauss, initially arguing from within dialectics but eventually moving toward a poststructuralist position. Her primary concern is a reevaluation of the claim of autonomy that underlies Western art music as a genre and musicology as a discipline. Challenging musicology's assumption that meaning in music is derived solely from intrastructurally significant forms and their elucidation through analysis, she argues instead for a contextualized music criticism. The key problem, shared by many scholars in the sociology of culture, becomes framing the context within which both musical production and music criticism take place. Subotnik's turn to philosophy provides her with a paradigm with which music or musical development may be compared, but not a context from within which criticism with substantial explanatory power can be generated.

Subotnik brilliantly elucidates Adorno's essays on 19th-century European classical music, which are an important source of his conception of bourgeois humanism. However, in appropriating Adorno through a critique of his position, she fails to appreciate the degree to which modernity is both the actual subject and the historical context of his critical theory, even when the ostensible subject is the 19th century. A positive view of bourgeois individualism does not make Adorno into a liberal ideologue. Rather, given his thesis of the liquidation of the subject in modern conditions, he regards the survival of the individual subject as a form of resistance and the capacity for intersubjectivity as the only conceivable basis for collective freedom. Likewise, a concern with the structure of language does not demonstrate that Adorno is postmodern; rather, it is tied to his thesis of the decline of collective forms of communication in mass cultural conditions. The terms of any critical theory are meaningful only in context.

The problem of context becomes particularly evident in Subotnik's essays on Mozart and Chopin. The structure of these critiques is derived from Adorno, positing a metaphor, or a homology, between musical compositions and some aspect of the larger social world—specifically here, the dominant structure of Enlightenment or Romantic thought. The attempt to link music and philosophy has many precedents and is based on music's lack of objective content and its formal similarity to logic. Yet this formulation, as a substitute for historical contextualization, must be subjected to serious scrutiny. First, the extent to which modes of thought effectively permeate a culture or are necessarily reproduced in its artistic output is open to debate. Second, dominant forms of thought imply relations of power and privilege that are never precisely transferable to artistic forms. In any given sociohistorical situation, the contexts of social action and artistic creation are not identical. Third, homology, as a structural equivalence, does not destroy but merely reconfigures the concept of autonomy against which Subotnik argues. Instead of cultural products having a constitutive role in social life or in the construction of social reality, they are simply documents of it. A metaphor suggests that two things are alike, not that they invent each other.

Just as artistic norms do not necessarily reflect the predominant worldview, artistic innovation is not always its critique. There is simply no evidence, as could be documented in correspondence or journals, to support the claim that Chopin makes "a post-Kantian critique of the norm of the autonomous semiotic universe idealized in classicism" (p. 139) or that Mozart used formal elements as metaphorical signs of belief in universal reason. What these statements demonstrate are the tensions between the limitations of a philosophical paradigm and the impulse to critique. It is, rather, *Subotnik* who develops such a critique in interpreting Chopin and who constructs an illuminating metaphor to explain something about Mozart. But this is musicological interpretation with a philosophical referent, not criticism contextualized in an existing social reality.

Criticism is not just methodologically different from analysis but is situated in the dynamics between music and society. Subotnik clearly assumes this larger project in defining "style" as a mode of social communication and "ideology" as the social setting in which music takes place and using them in the book's subtitle. Yet, in the last essay, the "social irrelevance" of contemporary music is analyzed through its claims to individual autonomy, rather than in the social context of patronage or the culture industry. Subotnik's valuable work would benefit from a theoretical framework that considers context more seriously and therefore more adequately supports her claims.

Discursive Acts. By R. S. Perinbanayagam. Hawthorne, N.Y.: De Gruyter, 1991. Pp. xii+211. $36.95 (cloth); $18.95 (paper).

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In *Discursive Acts,* R. S. Perinbanayagam undertakes a synthetic treatment of discourse as the central medium through which the self is objectified and connected with others and with social organization. Drawing on the traditions of pragmatism (Mead and, especially, Pierce), dramatism, Bakhtinian literary criticism, and, to a lesser extent, structuralist and poststructuralist writings on semiosis, he seeks an enriched conceptualization both of the human self and the stuff of its social transactions.

After a brief introduction, Perinbanayagam begins with a view of the self as an entity that is both produced through and realized in dialogue. In interactions based in signs, the self is made available to the other through objectified actions and responses. Correspondingly, relations between selves are *interobjective* in character (p. 8). In turn, the traditional Meadian conception of the self can be augmented by construing it as an assemblage of signs shaped by interaction, culture, history, and its own initiatives.

Discursive acts, Perinbanayagam argues in chapter 3, involve the production of signs that make one's intentions interpretable to another and he proposes a number of different dimensions of semiosis that are involved in this process: phonological (here discussed primarily in terms of prosody), grammatical, categorical (concerned with structural relations between signs), and symbolic. The interpretive reception of these acts (chap. 4) is, in turn, always creative and (re)constructive and, for these reasons, contingent and uncertain. Perinbanayagam describes the "fit" between utterance and response as "semantic interlocking" and argues that a response may be over- or underinterpretive of what it deals with. Reception and response may also be evaluated in terms of such dimensions as informed/ignorant, barren/fertile, internal/external, thick/thin, and arresting/continuous—all of which are illustrated here.

Discourse does not proceed in a social vacuum, and chapter 5 addresses some constraints on discursive acts arising from the social context (illustrated by organizational frameworks and social status), and social decorum and topic. Perinbanayagam here draws on Goffman's discussions of the role of rules in protecting the selves and social identities of interactants.

In chapter 6, the author assays sketches of some 18 types of discursive acts (confusingly termed "forms of discourse") ranging from such widely discussed actions as "requests" to more shadowy action types as "deceptions" and "scoldings." These activities, as chapter 7 usefully asserts, are permeated with emotional content that is "*simultaneously* cognitive and physiological . . . interpretive and felt" (p. 165). This emotional component of action is contrasted with *zweckrational* calculation and the author more questionably suggests that the affect-laden character of action means that social acts are "not calculated efforts, routinely based on rational and purposive considerations" (p. 165). In the final chapter, Perinbanayagam argues for the relevance of dramatism as a resource for understanding dialogue and its consequences for the transformation of self and other.

This is a sophisticated book, allusively and reflexively written and embodying a broad range of humanistic scholarship. At the same time, *Discursive Acts* is a book set squarely within the genre of speculative social theory and it is here that reservations may emerge among more empirically oriented readers. In the period since Anselm Strauss's *Mirrors and Masks* (Glencoe, Ill.: Free Press, 1959) so forcefully renewed the case for the sociological study of language, there has been a revolution in our understanding of how language functions and of how it is used in social activity. The revolution embraces the emergence of Chomskian transformational grammar, which set the stage for Roger Brown's pioneering studies of language development and the progressive recognition of the role of discourse in the development of language and self. It includes the development of Searleian speech act theory, which established a set of provocative interconnections between many different types of social actions in terms of underlying presuppositions and real-world conditions. There have also been Grice's "co-operative principle" (itself anticipated in Garfinkel's writings on "trust") and the "relevance theory" that has developed from it, the "politeness theory" of Brown and Levinson (derived from Goffman's analysis of face) which is concerned with building cross-culturally valid analyses of how social relations are encoded in utterances, and, within sociology, research in the Goffmanian tradition, ethnomethodology, and conversation analysis. Each one of these fields is focused on empirical aspects of language-as-action in detail and deals with its materials in ways that link discourse, social relations, and social identity. These more recent perspectives are not much addressed in this book. This is a pity: one could have wished for some discussion of them and that Perinbanayagam's evocative descriptions had been brought into contact with the several different conceptual outlooks they embody.

Kamikaze Biker: Parody and Anomy in Affluent Japan. By Ikuya Sato. Foreword by Gerald D. Suttles. Chicago: University of Chicago Press, 1991. Pp. xviii+277. $34.95.

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On Saturday nights in Tokyo, Osaka, Kyoto, and elsewhere in urban Japan the *bosozoku* ride. By the dozen, or hundreds, these members of Japanese adolescent gangs take to the streets aboard customized motorcycles and cars to cruise, drag race, pirouette, parade, fight, obstruct traffic, and above all attract attention to themselves. There is much that is American in the appearance of the *bosozoku.* They call themselves "Yankees," modify their vehicles to look like Hell's Angels' Harleys and Detroit "muscle cars," and congregate at the local Kentucky Fried Chicken, McDonald's, or Mr. Donut.

Given portions of Ikuya Sato's rich descriptions, it would be convenient also to explain away *bosozoku* in American terms, as the by-product of structural strain, set against the background of affluent modern Japan. *Bosozoku* youth are primarily high-school age, live with their working-class parents, attend low-rank technical schools, or work as unskilled laborers, store clerks, and food-service help. They complain, in a fashion reminiscent of Stinchcombe's high school rebels four decades earlier, that parent and teacher emphases on upward mobility through formal education is poorly aligned with their own likely career trajectories. Also like Stinchcombe's rebels, they have fashioned what they perceive as alternative routes to adult respectability via fighting courage, gang loyalty, and punctilious costuming. But here the similarity with North American youth and the utility of strain accounting ends. While Yankees may have only a feeble grasp on the means of production, they hold a firm grip on capacities for impression management. Sato's book is important in its treatment of alternatives to materialist actualization concerning play and its corruption in postmodern society.

Sato presents *bosozoku* "action" as play of a particular sort; sometimes fast, occasionally furious, but seldom fateful. In North American vernacular, *bosozoku* participation is less comprised of hot-rod thrills than low-rider promenades. Few are the Yankee's opportunities for uncompromised tests of individual skill and will, for *agôn,* for "flow" as Csikszentmihalyi puts it. What *bosozoku* participation provides most abundantly is carnival, drama, and a collective means of soliciting attention from peers, the police (albeit negatively), and the media. And by North American standards, attention is readily available.

Sato points out that, in contrast to the United States, with its multiplicity of ethnic groups, steeper stratification gradient, and other cultural diversities and conflicts, there is a basic sameness and overreaching constraint in Japanese social life. In venturing forth to test the limits of propriety, Japanese youths journey only a relatively short way from the life-style of the *ippan shimin,* the "square" or "ordinary citizen." By comparison with North American youth, Japanese delinquency seems subdued and constrained. Fights involve fists and feet, but not firearms, and are normatively restricted to age peers. *Bosozoku* sniff paint thinner or take amphetamines, but shun heroin and cocaine. A majority of Japan's older adolescents attend high schools governed by a national policy specifying standardized class size, curriculum, sex mix, and dress codes. Against this homogeneous background the nascent *bosozoku* achieves notoriety by purchasing school uniform jackets with lengthened hems and nonstandard lining colors, or combing his hair backward instead of parting it on the side.

Outside of school and work, after hours and on weekends, *bosozoku* apparel becomes more distinctive. Yankee costumes are embroidered with complex linguistic characters, gang logos, nationalist icons, and names like the "All Japan Kyoto Violent Racing Club," "Midnight Crazies," "Vampires," and "Killa." Club symbols and names also appear on matchboxes, lighters, and name cards carried by members, and on adhesive stickers festooning their vehicles.

All of this, the disparate clothing, dramatic self-referents, and especially the massed cacophony of *boso* riding, Sato tells us, is for effect, to create "unnaturalness" (p. 68), to shock and offend conventional sensibilities. Yankees fancy themselves as blends of the picaresque hero who transcends conventional morality, and the self-controlled, disciplined champion of chivalry—Mad Max and Lancelot in one. But the totality of *bosozoku* performance conveys another image, less unnatural and contrary than unauthentic and simulated. In their preoccupation with appearance, both *bosozoku* youth and the titillated public they seek to offend are caught up in aspects of an advanced consumer capitalism neither clearly perceives.

Cultural representations and artifacts such as imaginary identities and fantasy self-images have become economic commodities, merchandise to be marketed. The *bosozoku* symbol system becomes a collage of simulacra, of referentless signs that stand for nothing but themselves. Auto parts dealers sell *bosozoku* enthusiasts expensive, "high tech," custom-vehicle exhausts that increase decibels but not horsepower. The paraphernalia of protest, gang costumes, and other distinguishing accoutrements, become "ethnokitsch," commercially designed and profitably mass produced. Publishers do a brisk business with Yankee customers in picturebooks of obliging *bosozoku* posed on street corners in menacing *koha* postures. Yankees, who are adamant in their disdain for politics, wear club jackets called *tokofuku,* literally meaning the uniform of perhaps the most committed of all political parties, the kamikaze. On inspection, the *boso* ride itself is revealed as a pseudoevent, choreographed by group leaders, led by flag carriers, punctuated by intermissions for regrouping, and, when the opportunity arises, prescheduled for the convenience of TV news videotaping. In the end, even Yankees give up the game, in coming-of-age ceremonies at age 20, handing down their jackets to younger club members, moving stickers from vehicles to scrapbooks, and sometimes formally surrendering their club flags to the local police.

If Huizinga is right, if culture is formed first and seen most clearly in play, then Sato's book tells us not merely about some unrepresentative Japanese youth, but about the larger cultural landscape of Japan and, by comparison, illuminates our own. If Baudrillard is right, if we in postmodern cultures are displacing sociality with cynical attempts to live out the commercial images we sell ourselves, then Sato's study gives us a valuable glimpse of future portents. In both instances *Kamikaze Biker* is a book to be read.

Playing with Power in Movies, Television, and Video Games: From Muppet Babies to Teenage Mutant Ninja Turtles. By Marsha Kinder. Berkeley and Los Angeles: University of California Press, 1991. Pp. xi+266. $22.50.

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The landscape of popular youth culture has traditionally been dotted with a broad spectrum of commodified creatures, ranging from anthropomorphized animals (Mickey Mouse, Donald Duck), to fantasy figures (Superman, Batman), to idealized, gendered icons (Barbie, GI Joe). In the 1980s, a new cast of characters appeared on the scene: Muppet Babies, Garfield, and the Teenage Mutant Ninja Turtles. Marsha Kinder, a professor of critical studies at the University of Southern California, turns her discriminating eye on these phenomena, explaining their rise and proliferation within postmodernist society. Drawing on a case study of Victor, her own son (eight years old at the time of the writing), she examines Saturday morning television and its impact on the youngsters who are exposed to the barrage of these media images. This creative methodology, a "mystory" (a bit of "mystery," part "mastery," with a dash of biography), allowed her to share with her readers rich anecdotal data of Victor's interplay with the mass media.

Chapter 1, a rather long-winded, jargonized excursus that attempts to paint the theoretical framework from which Kinder works, places this study within the developmental, cognitive, and psychoanalytical works of Piaget, Bruner, Vygotsky, Lacan, and Freud. Sociologists may be disappointed not to see the application of some of our ancestors, such as Mead and Cooley, whose ideas could fruitfully have been used here.

Chapter 2 is largely a deconstruction of episodes of two of Victor's favorite Saturday morning shows, "Muppet Babies" and "Garfield." Kinder's analysis focuses on "transmedia intertextuality," the distribution of material objects, through various media sources, as they symbiotically feed off of each other. For instance, the comic strip begets a TV show, which begets a feature-length movie, which begets consumerist campaigns featuring toys, cards, games, and the like. Children, then, are enticed on a variety of fronts into wanting these constructed, commodified objects. In chapter 3, the rise of the interactive video system (e.g., Nintendo) is discussed. Here, Kinder treats us to a brief, but insightful, understanding of the history of the Japanese entry into this industry and how these toys were deliberately marketed to American youth. Believing that these video games were geared primarily to young boys, those with the most fear of castration, Kinder advances a theory of the oedipalization of these games. Most sociologists will find her psychoanalytical bent too heavy-handed, especially her notion that these games "help boys deal with their rebellious anger against patriarchal authority" (p. 104). More on target, though, is her contention that gearing these games more toward boys gives young males an advantage in an increasingly computerized, technological society. Similarly, given the prohibitive cost of the software, race and class biases are also prevalent. The chapter ably shows how the ideological messages inherent in such popular packages as "Super Mario Brothers" encourage masculine dominance. According to Kinder, the "Teenage Mutant Ninja Turtles" (TMNT), the locus of chapter 4, represent a supersystem of intertextuality constructed around a set of fictionalized figures. Kinder shows how these turtles were constructed to appeal to diverse audiences (differing in age, class, ethnicity, and generation) and proliferated through media events that sped up their desirability among the masses. The TMNT phenomenon is explained, showing how and why this particular constellation of events was so successful in capturing the fancy of the American public. Chapter 5 is a misplaced discourse on global economics, in which Kinder allows herself to indulge her desire to extrapolate on the rising Japanese influence in American media industries. Also, for good measure, she throws in opinions about the coverage of the Persian Gulf War, the increasing role of the Cable News Network (CNN), and the power of the newest media technology, high-definition television (HDTV). Rather than return to her roots in developmental and cognitive theory, where she could have advanced knowledge about children's socialization, she ends the book on an overblown note about the "new world order."

Despite these flaws, the book is worthwhile for scholars interested in communication, cultural studies, and child development. That her approach only intimates sociological sensibilities, rather than fully capitalizing on them, may be more testimony to our rigid disciplinary boundaries than to Kinder's shortcomings. Those readers interested in a postmodern discussion of these youth fads and the media may, at times, be annoyed, but they will find enough here to satisfy them.

Social Science and the Self: Personal Essays on an Art Form. By Susan Krieger. New Brunswick, N.J.: Rutgers University Press, 1991. Pp. x+273. $35.00 (cloth); $14.95 (paper).

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The so-called crisis of representation that transformed anthropology in the 1980s arrived late and without much force to sociology, where field study is less central to inquiry. Yet while Clifford Geertz, George Marcus, Paul Rabinow, and many other anthropologists questioned the purpose and process of the ethnographic project, sociologists were busy making parallel, constructivist interrogations of science. In their ethnographic studies of scientific work, G. Nigel Gilbert, Bruno Latour, Michael Mulkay, and Steve Woolgar portray the contingent, extrarational aspects of science that mirror the anthropologists' self-critiques; both groups of writers seek to debunk the formidable rhetorical edifice of scholarly "objectivity," to reveal the human actors who labor to maintain it, and to articulate the political contestations that induce scholars to image a positivist distance between the researcher and the subject in their writing.

Sociologist Susan Krieger's *Social Science and the Self* seems to lie in the raw space between these nascent traditions and that romantic genre of sociological field reflections perhaps best typified by John Van Maanen's *Tales of the Field* (University of Chicago Press, 1988). A remarkably creative, staunchly independent fieldwork sociologist herself, Krieger's earlier empirical studies of a radio station in San Francisco (*Hip Capitalism* [Beverly Hills, Calif.: Sage, 1979]) and a midwestern lesbian community (*The Mirror Dance* [Philadelphia: Temple University Press, 1983]) were controversial experiments in polyvocality and narrative form that presaged the vogue of such innovations in contemporary anthropology. Less willing to tinker with their own rhetorical techniques, many field sociologists discounted Krieger's texts as interesting if unscientific at best, analytically vacuous and downright unreadable at worst. This book will likely incite a similar reception. Deeply self-reflexive and highly personal, these 15 essays touch upon topics as diverse as the author's experience of graduate school, psychotherapy, Pueblo pottery, and Georgia O'Keefe. The different writings loosely cohere around a rather startling thesis: "My central argument in this book is that when we discuss others, we are always talking about ourselves. Our images of 'them' are images of 'us.' Our theories of how 'they' act and what 'they' are like, are, first of all, theories about ourselves" (p. 5). Significantly upping the ante on the self/other quandaries that continue to be key concerns of feminist methodologists and critical ethnographers, *Social Science and the Self* tenderly calls for a new and difficult model of social inquiry.

More than this argument, though, the author's own experience is the central guiding principle of *Social Science and the Self.* Initially we hear

Krieger tell of the twists and turns she took on her way to a doctoral program in communications, through graduate seminars and into her dissertation defense. We read of her short-term teaching appointments in different regions of the country, her fascination with Pueblo pottery, her struggles in the classroom, her ruminations when she went to "write up" fieldwork data, her impressions of writers Carolyn Heilbrun and Clifford Geertz. Krieger's style is lucid and conversational throughout, quite beautiful in places: "As an undergraduate," she writes, "I did not expect to become an academic. I majored in sociology as a route to becoming a city planner. I thought that a doctor was the thing to be, but I did not like blood so I would save cities" (p. 11). In a final section called "Other Voices" Krieger borrows extensively from interview notes with eight other social scientists who share the author's concerns about the role of personal experience in the project of academic writing.

Unfortunately, the epistemological bravery of Krieger's introductory thesis is not effectively elaborated in the proceding essays. The author's eye-opening claim that we write ourselves when we write about others gets lost in more or less uncritical meanderings through the worlds of art and academia. Krieger voices her frustrations with the organizational and textual conventions of social science which, in her words, encourage social scientists to "keep the self out" of their studies, but she is unwilling to examine the political exigencies of the knowledge industry that keep those conventions in place. Discrete, ostensibly verifiable truths are the social scientist's stock and trade; they constitute the capital with which individuals buy space for themselves in the occupational and prestige structures of the academy. Elaborate systems of depersonalizing convention rationalize these economies and enable social science disciplines to present broader powers with something to show for it all: knowledge that, at times, is of use to the profit maximizing agendas of business and the control objectives of the state. It is a less than pretty picture that Krieger refuses to critique. Instead, the author searches for more fluid, personal models of writing in the realms of art and craft, harnessing the myth of the "free" artist to advocate a freer academic milieu.

In short, *Social Science and the Self* is so uncritical of the structure it questions that it is difficult to appraise the text as a sociological act. The book does not conform to analytic conventions, nor does it seem designed to do so. Still, as a painfully honest portrayal of what it feels like to bump into the hard edges of the academy, *Social Science and the Self* stands as a tender service to its public. Many of us can remember strong feelings of inadequacy and confusion when, overwhelmed by the cacophony of the field (or the numbers, or the literature), we sat down and faced the task of writing up our first study. Who were we to explain how the world worked? To hide our uncertainties and cover our tails, we emulated favorite authors, wrote like our teachers, cited famous sources, and used the third person, hoping that somehow the discomfort of writing would pass. In *Social Science and the Self,* Susan Krieger conscientiously explores that discomfort and—perhaps unwittingly—reveals an affinity between the emotional and political dilemmas inherent in the task of making knowledge.

Feminism and Youth Culture: From Jackie to Just Seventeen. By Angela McRobbie. Boston: Unwin Hyman, 1991. Pp. xx+255. $49.95 (cloth); $16.95 (paper).

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Angela McRobbie played a key role in introducing a feminist perspective to the Centre for Contemporary Cultural Studies (CCCS) and thus in ensuring the subsequent interdisciplinary and worldwide appeal of the brand of cultural studies that developed there. Her sociology continues to be informed by the powerful mix that emerged at CCCS of British and continental Marxian social theory, of the "culturalism" of Raymond Williams and E. P. Thompson, and of the ethnographic inquiry of Goffman, Becker, and other American sociologists. These influences and the driving project of feminist scholarship shape the articles and essays that make up *Feminism and Youth Culture.*

The book collects eight of McRobbie's articles and, through its valuable introduction, describes their personal and intellectual contexts. Five of the articles were published (or circulated by the CCCS) during her years in Birmingham; the remainder, previously unpublished, were written in the late 1980s after her move to London.

McRobbie's general project is a feminist one: to redress the undervaluing of women and gender in social and historical analyses and to overthrow those ideologies and (research) practices that oppress women. Her own research has centered upon women growing up in contemporary Britain; her pervasive themes "*lived experience, . . . popular culture* and . . . *subcultures*" (p. ix).

Three articles from the earlier period concern youth subcultures (in the European sense of the ways of life built around expressive traditions and styles). Here McRobbie analyzed the stark absence of girls and young women from the literature on subcultures and countercultures, and, particularly, took her male colleagues at CCCS to task for their insufficient appreciation of the pivotal role of gender relations.

Another set of articles examines forms of popular culture. The two articles on British magazines for girls show these magazines' remarkable changes between McRobbie's first look in the 1970s and her second in the late 1980s. Among other alterations, the magazines had reduced their narratives, mostly stories of romance, and increased the visuals, often pictures of pop stars. The emphasis shifted from traditional gender roles and relations toward the cultivation of the self.

McRobbie primarily directs her analytic energy to what might be called the "psychocultural." She adroitly reads the ideological messages embedded in the magazines and in dance narratives and plausibly depicts individual girls' engagements with these forms as means of resolving their psychic and social dilemmas. She concludes (p. 217), for example, that in the dance narratives of films (e.g., ***Fame***) "it [art] acts as a participative myth, a fantasy of achievement and a way of taking one's destiny into one's own hands." Her earlier work on the culture of working-class girls similarly emphasized the fantasy potential of romance.

McRobbie goes a good way to avoid the frequent thinness of psychocultural analyses. She does this by situating the readers of the magazines and the watchers of the films in the historically specific social and economic formations that frame their lives. She relates her textual analyses to a society that she characterizes by drawing upon an array of social theorists from Foucault, Baudrillard, and Bourdieu to former CCCS colleagues and feminist theorists. She is less assiduous, however, in delineating the space between the individual and the broad, historically derived structures that these theorists describe. We learn too little about the local collectivities—the peer groups, the communities of adults—that mediate these larger structures. We are left wondering how girls' magazines fit in everyday social practices. We learn even less about the ways in which the glorification of the self plays out in peer cultures or even what sorts of symbolic capital (e.g., attractiveness) remain important in contemporary gender relations in the Britain of the 1980s. Lacking an account of how popular culture figures in and is figured by these local collectivities, McRobbie's analyses must remain fundamentally incomplete.

McRobbie's accounts of the (re)production of gender privilege reflect a move away from institutional sites such as schools and sites of everyday interaction, such as peer groups, toward the popular media. Nonetheless, one of the later articles indicates her continuing concern with the harder edges of state control (the welfare system) and with the limited material life brought about through high unemployment. In the book's last article, McRobbie describes the world of family and unemployment as viewed by teenage mothers in a particularly depressed industrial area. Here McRobbie's emphasis on lived experience comes in strongly. We gain a vivid picture of the sense that the women try to make of their lives and the sense that teenage motherhood makes to them.

This book has a significance beyond the scholarly. We see, through its embedded dialogues, a distinctive practice of sociology and the rich life of a scholar/activist. There are responses to colleagues from CCCS, to "feminists from within" (in this case feminists on the staffs of girls' magazines), to feminists who question the priority of scholarly work (see, e.g., "The Politics of Feminist Research"), to those in "panic" over teenage pregnancy, and to sociologists of dance. McRobbie's sociology is an ambitious one that engages a broad array of demanding audiences. Because she is candid about the dilemmas of such work and open to criticism and exchange, her book is a valuable resource for those who find a narrow definition of scholarship stultifying and/or immoral.

In Defense of Modernity: Role Complexity and Individual Autonomy. By Rose Laub Coser. Stanford, Calif.: Stanford University Press, 1991. Pp. xi+199. $29.50.

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Finally, a sociologist of insight and courage offers a salute to gesellschaft and exposes the warts and deficiencies of gemeinschaft. For some, it is as shocking as revealing that childbirth can be painful (even for those with good attitudes), and that many mothers do not like caring for children—a subject best kept secret or acknowledged only as characteristic of pathology. Myths, after all, serve important social functions, particularly those that keep people in their place, and Rose Laub Coser spotlights how modernity—and life in urban environments—frees people from them.

Marshall McLuhan once wrote that Americans see the world through rearview mirrors. Thus they are entranced with the romance of cowboy culture—not with the drudgery, monotony, and loneliness of itinerant farm hands, but with a romanticized version stressing acts of skill and heroism. In a similar vein, William J. Goode noted that American social scientists had not been much better than ordinary people in their view of past golden eras in which a "classical family of western nostalgia" thrived and grandma's farm was a place where the family knew their designated places in society and the division of labor. The romanticized past is today seen in the advertising campaigns (geared to women) that focus on "the new traditionalism" and on Victorian and Edwardian styles of clothing and house decorations. Writers and social policymakers too, assure us that a return to the nuclear family, complete with the authority of a male breadwinner, will solve the problem of youthful violence and the other social ills that plague our cities. The new stay-at-home Mom (perhaps pregnant as the courts whittle away abortion rights) will have left her "male-model" career to find fulfillment in raising children. She and the children, now educated at home or in parochial, private, separate-sex schools (perhaps financed by education vouchers) illustrate the panaceas to be achieved by looking backward. Only a few authors, such as Wendy Kaminer (*A Fearful Freedom: Women's Flight From Equality* [Reading, Mass.: Addison Wesley, 1990]) and Susan Faludi (*Backlash* [New York: Crown, 1991]), have stood against the surge of books that seek comfortable solutions for today's problems by a return to an imagined past and its social arrangements.

Rose Laub Coser, in her sharply analytic sociological analysis of misguided emphasis on socially constructed pasts and her insight into the restrictiveness of narrow family- and community-oriented life, draws on a wide body of sociological studies. Referring to work on organizational behavior, personality and social structure, and modernization, including her own work on the family and on immigrant Italian and Jewish women, she shows how the move to complex and modern society has resulted in an emphasis on personal rights and independence and has offered the opportunity to distinguish the personal from the universal. This has consequences for people's ability to conceptualize and to develop their intellects. Coser shows the power of structural analysis and status set (or role) theory—often disparaged these days by those who fail to understand its dynamics. Outlining the autonomy attained when an individual enlarges his or her status set by moving beyond the family and into the public world, Coser points out the ramifications for improved self-image, intellectual capacity, and tolerance.

Some feminist scholars today argue that a focus on individual rights emanates from a male model and that an alternative female model would focus on relationships and community. This is a simplistic notion that is ultimately self-defeating. Not only does it fail to recognize that individual rights are compatible with community responsibility, but it fails to note that too strong an identification with community or a network of intimates may limit the person's ability to empathize with those outside the group. This is precisely the problem displayed by many antiabortion activists whose horizons are limited to a narrow concept of traditional roles and families. Coser keenly notes that limitations imposed on women and minority groups to keep their statuses and role sets restricted come from a wide array of policymakers from the president of the United States to local legislators. Such policies as not providing child care (making it difficult for women to work far from home or to work for pay at all) and resisting busing conspire to limit visibility of outsiders and to curb the interaction of groups. Citing the work of Scribner and Cole, the author notes how education in school does not merely consist of learning facts and skills; it brings about a separation between the intellectual and emotional domains and teaches a new concept of relationships. The modern person, it is found (relying on Inkeles and Smith's study of six cultures) thinks differently about women's rights and political events than do those growing up in an environment that lacks social complexity, and getting ahead (according to Granovetter) is dependent on a person's acquisition of many loose ties and his or her escape from the restrictions imposed by the strong ties of family membership. For people in many occupations, phoning a colleague one knows slightly may result in important information for the conduct of work, as we know from research on "invisible colleges."

Coser notes how workers at the bottom of hierarchies, people from particular ethnic backgrounds and women, are situated so as to limit their acquisition of broadened role sets and opportunities to engage in multiple and complex social relationships. Far from plunging the subject into role strain, such associations, she points out, would offer them much more flexibility and many more options.

This gem of a book should be a springboard for examining the recent changes in women's lives and the changes for immigrants who have come to the United States and demonstrated their ability to transform their lives with the power of expanded role sets. Coser has provided some useful conceptual tools with which to study the powerful effects of modernity and freedom.

Rachel's Daughters: Newly Orthodox Jewish Women. By Debra Renee Kaufman. New Brunswick, N.J.: Rutgers University Press, 1991. Pp. xv+243. $30.00 (cloth); $11.95 (paper).

Tradition in a Rootless World: Women Turn to Orthodox Judaism. By Lynn Davidman. Berkeley and Los Angeles: University of California Press, 1991. Pp. xi+254. $24.95.

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Rachel's Daughters and *Tradition in a Rootless World,* two qualitative studies of *ba'alot teshuvah,* women with limited Jewish backgrounds who choose as adults to abide by Orthodox Jewish practice, reach very similar conclusions although they draw on their intellectual and empirical resources very differently. Both Debra Renee Kaufman and Lynn Davidman set their story within the seeming paradox of women raised and fully experienced in modern, secular American culture who "return" to Orthodox Judaism, despite its gender-based restrictions. Each study demonstrates that these returnees see their new position not as one of submission to the traditional patriarchy but rather as offering strength, opportunity, and value to the community. As supported here, this story holds significant implications for our understanding of both traditional religion and contemporary feminism.

Lynn Davidman examines the conversion process itself and divides her research time and analysis between two very different settings within the Orthodox Jewish world. Davidman characterizes the women who become involved in each setting and demonstrates the fit between their preconversion lives and expectations and how they become socialized to different forms of Orthodox practice. The Lincoln Square synagogue in Manhattan, founded in 1963, reaches out to single women and men who lead independent, career-oriented lives in New York City. The programs it offers to reeducate these neighborhood singles to the language, practices, and customs of Orthodoxy were formally incorporated into an institute in 1971. In the same year, Bais Chana, the other setting for Davidman's research, held its first institute geared exclusively toward women. Its founders, sent in 1965 to Minneapolis/St. Paul by the Lubavitch sect's spiritual leader to draw assimilated Jews into traditional religious practice, created a residential program "where girls of extremely limited background in *Yiddishkeit* [traditional Judaism] could pursue purely Torah studies" (p. 52, quoted, annotated by Davidman). By 1983, both programs had grown: Bais Chana had coordinated its annual one- and two-month programs with college vacation times, thus including 50 (mostly younger) *ba'alot teshuvah* at a session; at Lincoln Square, about 70 people attended the innovative weekly Beginners' Service and several hundred attended religious classes at the synagogue.

Davidman carefully organizes her material so as to make these two settings parallel. Her delineation of the theoretical issues along with the diverse material she has collected makes this approach unusually effective. Rather than simplifying the material through this organization, Davidman uses it to yield a convincing three-dimensional picture of one phenomenon. Davidman's methodological discussion conveys her unique research experience, including its unpredictability, while successfully achieving the difficult task of connecting initial research choices with subsequent analyses and conclusions. She combines stylistic directness with contextualized paragraph-length quotes from her subjects to open an analytical window; thus, her quotes not only support her arguments but also provide a rich source of objective material for the reader's consideration.

Davidman presents the data carefully and speaks clearly to larger issues. Her analysis draws on a nuanced view of modernity in relation to tradition rather than on a sharply segmented one, as she discusses how the *ba'alot teshuvah* and their teachers consider and use both. Comparing the two settings, Davidman differentiates between a modern language of choice versus one of internal and external compulsion as different types of *ba'al teshuvah* make their decisions. Davidman also treads carefully in uncertain areas. As a feminist she conveys throughout that, while this traditional choice is not a necessary one, it does have a logic and an internal consistency worth understanding.

While I did not always agree with Davidman's interpretations and was frustrated at some points in her brief historical introduction, her essential sociological observations and conclusions are well drawn and vividly illustrated. For lay and professional readers, Davidman demonstrates a subtle knowledge of the literature and points to its significance without overwhelming those unfamiliar with it. Davidman's book would make an excellent undergraduate text and also offers much for more advanced researchers and others interested in its general ideas.

Reflecting similar themes, Debra Kaufman frames the *ba'al teshuvah* return within the larger context of a search for roots, meaning, order, and a "coming home." Kaufman also emphasizes the search for female identity, family, and domesticity. Kaufman interviewed 150 women in various cities, most of them married, some years after their religious return. Working from these "structured conversations" (p. 52) she cites her subjects as indicating that Orthodox Judaism's appeal lay originally in "the nature and description of 'feminine' and the female" (p. 141) and concludes that "these so-called antifeminist women focus clearly and decidedly on gender as the predominant principle in their lives and as the cornerstone of their ideological and political stances" (p. 156).

Writing freely and with extensive footnotes, Kaufman explores several sides of how a modern feminist might view these women's relationship to traditional law and patriarchal community. As does Davidman, Kaufman relates the *ba'al teshuvah*'s frequently rigid view of feminism, particularly in the personal domain of marriage and family. While Davidman indicates that some women accepted feminism's workplace goals, Kaufman stresses that it is in the public arena in particular that Orthodox women suffer from gender-based religious law.

In this vein, Kaufman explores one area in particular that demands sociological attention. After a discussion of feminism's history and contemporary feminist thought, she sets her subjects' ideas about women's strengths and resources alongside those offered by contemporary radical feminists. She also provides data on how Orthodox leaders interpret Jewish law significant to women, the anthropological and historical literature on how sex segregation produces women's culture, and recent research on New Right women in American culture. While each of these discussions reflects extensive reading, Kaufman unfortunately fails to draw specific links between her data and these other findings. As a result, where these connections might strengthen her argument and add texture to the world she has observed through interviews, they do not. Her wide-ranging references also limit her critical audience for each of these sections to those most familiar with that area and ultimately obscure which points and definitions she considers key to her arguments.

One significant difficulty with Kaufman's analytic approach lies in ongoing assumptions that are only elucidated later in the discussion of feminism and women's roles. Through much of the data presentation, Kaufman appears to be at odds with what she has found without explaining why. Kaufman often writes that her subjects "claim" something instead of assuming that they may be sincere. For example, in the discussion of sex and Jewish Orthodoxy, her subjects speak openly and with relative comfort about their sex lives, pointing to a beautiful irony inherent in women returning from modernity to tradition in order to feel more comfortable with their heterosexual relationships. Unfortunately, Kaufman undermines this discovery by conveying disbelief. Had she made her assumptions along these lines explicit, she would have challenged both herself and her readers more effectively.

Kaufman brings together a number of significant issues, but her unspoken assumptions and analytic indirectness made this book hard to read. Her open-ended approach does suggest further questions in this area concerning how Orthodox men and those women who were brought up in the tradition would speak about the issues raised with the *ba'alot teshuvah*.

Meanings of Life. By Roy F. Baumeister. New York: Guilford Press, 1991. Pp. x+426. $35.00.

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The title of this book clearly indicates that the author tackles a "very broad problem that is, indeed, a philosophical one" (p. v); he does so, however, within the confines of the social sciences, or more specifically, of empirical findings. The author reviews "facts, not theories" (p. 10); he warns the reader that theorists, for example, James or Freud, will be cited only "insofar as they are linked to factual evidence" (p. 10). Finally, we are told that this work, in the making for seven years, "has been the most difficult but also the most stimulating and satisfying research project of [the author's] career" (p. x). It is only fair to review the achievement in the light of its goals and aspirations, and, in my opinion, the author has succeeded pretty well.

Part 1 has two excellent polemical chapters: "Who Says Life Has No Meaning?" and "The Myth of Higher Meaning." These pages describe a common tendency to assume that life has one single purpose or an overarching central meaning. I am not convinced that such a tendency is best described as a "myth." "Empirically, people's lives are full of relatively short-term projects and strivings" (p. 21); most low-level meaning systems are socially defined (symbols, institutions, norms, etc.). However, as the author suggests, meaning is best described metaphorically as a web that progressively integrates all aspects of one's life. The philosophical problem of meaning is precisely that of unity within the fragmentation of everyday life. This issue is addressed only indirectly in the analysis of today's "value gap."

Having opted for an empirical study of the meanings of life, the author comes up with "an existential shopping list," namely four basic needs for meaning (chap. 3): purpose, value, efficacy, and self-worth. I would not argue about the cogency of these needs, but about their mutual relationship. Value and self-worth concentrate on the self; purpose and efficacy relate the self to the environment; there is no necessary relationship among these four needs. The main thesis of the book is that in recent times, the self has become our ultimate value base. This thesis, however, does not articulate the description of the four needs of meaning. Having opted for an empirical study of meaning, the author has given up any theoretical context into which to integrate empirical findings.

The best chapter of the book, the one that summarizes much of the author's research, deals with "Self-Identity and the Value Gap." The thesis of is that "modern Western culture has struggled to establish the self as a major value base" (p. 78). What is new here is not the thesis itself, but the tone: Baumeister presents this achievement as something positive, while many of the sources quoted (e.g., Robert Bellah et al.'s *Habits of the Heart* [University of California Press, 1985] and Christopher Lasch's *The Culture of Narcissism* [Norton, 1978]) present the same thesis but with acrimony or condescension. What is new is that Baumeister makes it clear that for us the self is our only common value base and we had better make it our starting point.

If one looks for answers (title of pt. 2), one is likely to be disappointed. Religion and morality are not major value bases anymore, nor is the work ethic (chaps. 6 and 8). Empirical research on happiness does not provide much of an answer. For moderns, death stands as the negation of self-fulfillment; hence it has to be denied or avoided (chap. 11). This book raises many more questions than it can answer.

Yet this work is as challenging for the reader as it has been for the writer. Although the issue of meaning has traditionally been a philosophical one, it is increasingly a universal question that cuts across disciplines. The author is a psychologist and claims that "in the final analysis this book is a work of psychology" (p. vi); yet the content is rather interdisciplinary and ecumenical. One may argue that the question of meaning belongs to psychology more than to any other discipline; yet the thesis of the self as the base value points to a new direction: the study of values as "core" of any curriculum. It is to this core that Baumeister makes a valuable contribution.

Living in the Children of God. By David E. Van Zandt. Princeton, N.J.: Princeton University Press, 1991. Pp. vii+232. $24.95.

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Living in the Children of God is a competent ethnographic study based on a 1985 doctoral dissertation, from the London School of Economics and Political Science, for which nearly all of the research was done in the 1970s. There are nine substantive chapters, plus an introduction, a brief postscript updating the status of the movement to early 1991, good bibliographies (one specific to the movement and one on new religious movements more generally), two appendices, and an index.

The introduction gives an overview of the Children of God (COG), also called the Family of Love, and of the nature of the research project. Chapter 1 summarizes the COG ideology and proselytizing system, chapter 2 contains a short history of the COG, chapter 3 describes the organizational context, chapter 4, the social relations of everyday life, chapter 5, the process of "litnessing" (contacting and soliciting with literature), chapter 6 deals with the conversion process, chapter 7 with the COG literature and the part it plays in movement life, chapter 8 with the practical measures for maintaining the COG plausibility structure and construction of reality, and chapter 9 with the socialization process (of both new converts and of children). The principal data for all chapters come from participant observation, but the postscript is based on second-hand information obtained from key informants knowledgeable about movement developments during the past decade or so.

Appendix A is a thorough and interesting account of the "career" of the project and of the implications of the author's evolving status from a prospective convert to a sympathetic participant to a more-or-less objective observer. Included is an interesting, but not entirely persuasive, resolution of the ethical implications of gaining entrée by posing (briefly, at the beginning) as a genuine seeker or potential convert. Appendix B contains some sample "Mo Letters" and cartoons, which constitute the main form of communication between the founder—prophet Moses David (or David Berg) and his followers.

The main strength of the book is the richness of the ethnographic detail. These data are given significance beyond the descriptive level by frequent references to the theoretical literature in phenomenology and ethnomethodology (e.g., Schutz, Goffman, Garfinkel, Cicourel, Sacks, Schegloff, and Zimmerman). These are, however, only passing theoretical allusions, for there is no overarching theoretical framework guiding the study as a whole. Chapter 3, on organizational matters, would have benefited by the imposition of some framework drawn from organizational behavior theory, but it remains entirely descriptive. Chapter 6, on conversion, makes some use of phenomenological theory, but it ignores most of the social psychological work on conversion from the sociological mainstream (e.g., Lofland, Stark, Richardson, Bromley and Shupe, and others).

This is one of a handful of scholarly studies of the COG and the most thorough and intimate we are ever likely to get. Although the description on the book jacket resorts to a rather salacious tone in an effort to attract readers, the book itself is a remarkably sober and balanced treatment of one of the most controversial of the "cults" coming out of the Jesus Movement of the 1960s. This book, written from a perspective that is at once "inside" and scholarly demonstrates once again the essential normality (even banality) of life within a new religious movement, whether at the individual, family, or organizational levels. It offers still another refutation to those who find it difficult to understand any of today's new "cults" without reference to theories of psychopathology or to the sociology of the bizarre.

Pray TV: Televangelism in America. By Steve Bruce. New York: Routledge, 1991. Pp. xii+272. $59.50 (cloth); $16.95 (paper).

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Since Alexis de Tocqueville's exalted journey to the United States in 1831, American intellectuals have trusted foreign visitors to offer insights about our culture that have escaped us because we, as participants, are prone to miss that which is right under our very noses. Steve Bruce is a Scotsman who fancies himself a latter-day Tocqueville who has carved out a mission of telling the English-speaking world all about the strange religious practices of conservative Christians in America. In two previous books he has addressed the faith and politics of American fundamentalists. Now he comes to tell us of the world of religious broadcasting—a world dominated by fundamentalists and pentecostals. My reading of this latest treatise, no less than of the previous two, leaves me baffled as I try to understand how someone with such condescending attitudes toward the subject matter, which he seems to consider utterly trivial, could go to all of this bother.

Until Ronald Reagan teamed up with some politically minded television preachers during his presidential campaign in 1980, a large majority of Americans had missed the growth of televangelism over the previous decade. After his election, Reagan's cozy relationship with Moral Majority leader Jerry Falwell and the presidential ambitions of another broadcaster, Pat Robertson, kept the televangelists periodically in the news during much of the 1980s. It was not until sex and money scandals rocked the religious broadcasting industry in 1987, however, that televangelists became regular fodder for mass media news coverage and most Americans became at least familiar with the names of a few of the stars of this unique genre of inspiration and entertainment.

Bruce has logged some time in front of a television set watching the religious broadcasters, and he claims that some fundamentalists and pentecostals have invited him into their homes to talk about such broadcasting. There is no evidence, however, to suggest that he has ever met a televangelist or even an executive of a religious broadcasting organization. As for the viewing and interviewing of those who also watch, there is not a single quotation in the volume that bears testimony to any systematic observation. In the preface he counsels his readers to take his word "on trust." "Any reasonable person looking at and hearing the same things would have come to the same conclusion," Bruce argues (p. x).

Bruce begins his inquiry by noting that, while Europeans have become accustomed to the "brashness" and "commercialism" of American television, "they have yet to view American religious television with anything other than horror or morbid curiosity" (p. 1). Reading *Pray TV* is not likely to change Europeans' perceptions much. It could hardly be otherwise. The author's own abhorrence of the subject would not permit him to get close enough to see anything other than the repugnant stereotypical images that initially informed the inquiry. Bruce's inimical posture toward the televangelists is exceeded only by the antagonism and rancor with which he treats the several scholars he has drawn upon so heavily to write his own book.

Pray TV covers much of the same territory as several other books about religious broadcasting published over the past several years. They all (1) locate religious broadcasting in American religious history, (2) profile the leading televangelists, (3) discuss beliefs of the broadcasters, (4) describe the organization of their broadcast empires, (5) offer viewer profiles, (6) disclose audience sizes and, of course, (7) discuss these broadcasters' ventures into conservative politics.

There is no new factual information in this volume. The secondary factual data mustered substantially from a half-dozen key sources provides a backdrop for the author's pugnacious pontifications. Bruce's twisting of information he has taken from others to fit his own arguments often leaves the reader with considerable misinformation and misdirection. *Pray TV* is not a serious scholarly work. It is a highly opinionated introduction to a world the author does not respect. As Bruce notes, media does not change opinions so much as it reinforces the prejudices people already hold. This book should serve well the purpose of reinforcing the views of Americans and Europeans alike who equate television preachers with snake oil salesmen. Those who are interested in understanding this uniquely American phenomenon will have to look elsewhere.

Acts of Compassion: Caring for Others and Helping Ourselves. By Robert Wuthnow. Princeton, N.J.: Princeton University Press, 1991. Pp. viii+334. $24.95.

Between States and Markets: The Voluntary Sector in Comparative Perspective. Edited by Robert Wuthnow. Princeton, N.J.: Princeton University Press, 1991. Pp. 318. $55.00 (cloth); $16.95 (paper).

Joseph Galaskiewicz
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Sociologists have paid scant attention to the voluntary sector in the past 20 years. Yet 45% of the adult population in the United States volunteers regularly, and the total value of voluntary action is approximately $150 billion annually. In *Acts of Compassion* Robert Wuthnow attempts to unravel the paradox of so much voluntarism and compassion in a society that values individual freedom, success, and self-interest so highly. He offers provocative insights, and I give the effort high marks. This is an important book for students of contemporary American culture and voluntary, altruistic, and prosocial behavior.

Using survey data and in-depth personal interviews, Wuthnow found that voluntarism and acts of compassion as practiced in contemporary American society are not inconsistent with the emphasis upon individualism in our culture. In contrast to a voluntarism and compassion that is rooted in moral imperatives and demands commitment and empathy with those who are helped, there are very few demands placed on the modern-day Good Samaritan. Many acts of compassion take place through roles in voluntary associations and are thus segregated from the rest of the volunteer's life. Many who are helped are strangers whom the volunteer never sees again and to whom she or he has no further obligation. Volunteers seldom immerse themselves in the social situation of those they serve and thus never see the impact of bigotry, prejudice, and discrimination. Volunteers typically relate to the shortcomings or misfortunes of individuals, not to the collective victims of social injustice. Words like sacrifice, empathy, unconditional love, social justice, and commitment are absent from the modern volunteers' vocabulary. They "do good," but on terms favorable to them. According to Wuthnow's respondents, today's acts of compassion are an avenue of self-expression and even a form of rebellion against the sterile repressive subcultures of government and business bureaucracies. People said they do good because it makes them feel good, gives them an opportunity to express caring sentiments, and is a way for them to get in touch with their humanity. In other words, voluntarism and compassion are ways for people to express their own individuality.

Yet volunteers' interpretations of their behavior are heavily influenced by the larger culture. In our culture how else can one interpret compassionate behavior except as an expression of individuality? Can volunteers speak or even think about caring behavior in any but individualist terms? As people tried to explain their good deeds, they talked about how it made them "feel good," or some vague payoff they expected in the future, or some special situation they were in. They rarely said they were being altruistic or it was just the right thing to do. Perhaps these words have no meaning for people anymore; such language may be too deviant.

But it is more than just a problem of language. This inability to think about compassion in other than individualistic terms can be linked directly to the pluralism found in our society. The only cultural absolute is that there are no absolutes. Thus without a set of moral standards to dictate right from wrong, every individual is on his or her own. Given the cultural context, many rationalize in terms of their own well-being. But even those whose giving is religiously motivated couched their explanations in secular terms and were careful to point out that religious motives were only *their* reason for giving. An individualistic culture not only forces us to use a certain vocabulary, it also negates the possibility that there is some reality or truth beyond the individual experience. Therefore, how could people say they acted in any other way than selfishly and still respect the individuality of others?

Wuthnow also found that voluntarism and compassion were important cultural symbols for his respondents. They symbolized what society could be and were a source of hope. The Good Samaritan story teaches that in a world of strangers, people can count on others. Compassion can overcome deep social divisions. Voluntarism is also a kind of insurance that one can count on when political and economic institutions fail. The "goodness" embodied in the volunteer effort will not solve systemic problems nor is using volunteers to deliver health and human services efficient or effective, but it at least gives hope that the "people" will respond when all else fails.

Nonetheless, Wuthnow cautions that we cannot solve social problems without focusing on the context of these problems and making a sustained commitment to social justice. Unless compassion is translated into social action, it will either be restricted to narrow roles within nonprofit organizations or be episodic. Indeed, if personal fulfillment is all-important, what happens to the voluntary sector when today's volunteers find other things—besides compassion—personally fulfilling? Only by viewing compassion as an exercise in community building—and not just as an individual act of caring, will we get a broader picture of the interdependencies in our society and develop a deeper commitment to furthering the collective interest.

As I said earlier, this is a very important book. To rattle off its problems is not to discredit the author, but only to be honest with the reader. I was uncomfortable with his selective use of interview data to illustrate his points, the preachiness in parts of his narrative, and his completely ignoring the issues of class and of the role of cultural elites. Yet to dwell on these shortcomings is to do the author an injustice. This is not a research monograph but rather an exercise in interpretative sociology that seriously explores the meaning of voluntarism and compassion within the context of contemporary American culture.

Between States and Markets is a much different enterprise. This edited volume contains articles written by scholars on the role of voluntary associations in Great Britain (James Beckford), West Germany (Helmut Anheier), Sweden (John Boli), France (Jack Veugelers and Michele Lamont), Italy (Ted Perlmutter), Israel (Eliezer Jaffe), Japan (Helen Hardacre), and the United States (David Watt). Wuthnow outlines the book's agenda in the first chapter and summarizes its main themes in the final chapter.

This volume examines the roles that voluntary associations play in preserving democracy and contributing to public discourse. The authors do not limit themselves to strictly charitable organizations, but also consider political parties, social-movement organizations, religions, and self-help groups among others.

The book contains a wealth of information on each of these countries. Each author (with a few exceptions) provides a brief overview of the recent political and economic history of his or her country, describes state-sponsored social welfare efforts, discusses the role of parties and social-movement organizations over the century, examines the interface between religion and political and economic institutions, and describes the role, if any, of charitable organizations. Space limitations disallow me a detailed description of each country, but I should note there are many more differences than similarities across these eight modern, industrialized democracies—many more than I think Wuthnow acknowledges.

Wuthnow makes a number of important observations in his concluding chapter. For example, the voluntary sector can offer an array of values that are antithetical to bureaucratic political norms and mercantilism and thus provide cultural alternatives to statism and industrial capitalism if it is able to withstand cultural colonization from these two sectors. At the same time, the future of the voluntary sector is dependent on the growth of the state and capitalism, and there will be more and more partnerships between the three sectors incorporating even social-movement organizations. Furthermore, he points out how difficult it will be for the voluntary association to be an important "player" in society, while at the same time remaining small enough to ensure the involvement and enthusiasm of its members. These are all important and valid points.

However, Wuthnow stubbornly refuses to acknowledge that much of the business of voluntary associations has absolutely nothing to do with caring, serving the poor, or solving social problems. In every industrialized society studied voluntary associations are vehicles to further different private interests whether they be business (as in West Germany) or labor (as in Italy) or women who want access to abortions (as in the United States). Thus to expect that they will contribute to discourse on collective values is unrealistic. In fact, most contemporary voluntary associations are vehicles by which special interests circumvent the democratic process to ensure that their interests are represented before those of elites. This is hardly what Tocqueville had in mind.

This volume is well worth reading, and I recommend it highly to students of voluntary associations, religion, and political sociology.

The Rise of Radical Egalitarianism. By Aaron Wildavsky. Washington, D.C.: American University Press, 1991. Pp. xxxvi+266. $54.95 (cloth); $24.95 (paper).

James V. Spickard
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Aaron Wildavsky is upset about the rise of "radical egalitarianism" in America. This is not much news. For many years the Berkeley political scientist has written humorously and at length on the follies of what passes for the American Left. He does not doubt that Left's sincerity; he distrusts it on cultural grounds. He believes that leftists' attempts to make all people equal—an effort well-embedded in American culture—sets them at odds with two equally American cultural traditions: individualism and hierarchism. What used to be a cultural balancing act has turned into a cat fight. Wildavsky seeks to show us when, where, and why. While doing so curmudgeonly, he uses this book to gore some sacred cows that deserve it. More important, in the book, Wildavsky provides an outline of the theory of cultural politics that is much more interesting than what he makes of it.

Let me outline this theory. Wildavsky bases his analysis on Mary Douglas's typology of cultural biases. (Their jointly authored *Risk and Culture* [University of California Press, 1982] covered similar ground.) He—and she—argues that people tend to sort themselves into four camps.

Individualists favor unfettered opportunity and competition for success. Hierarchists favor authority and expertise. Fatalists see themselves as powerless to influence events. And egalitarians strive to make everyone alike. The last distrust both institutions and the market, believing them to be sources of unwarranted privilege. Douglas says that these biases arise from the experiences people have with different social institutions: bureaucracies, for example, both generate and attract people who will defer to experts—not the least because bureaucracies generate expertise by their specialization.

Wildavsky does not focus on the structural underpinnings of cultural biases, as does Douglas. Instead he presents individualism, hierarchy, and egalitarianism as three continuing strands in American culture—sui generis. America's revolution and its early decades were marked, he says, by an alliance between individualists and egalitarians against hierarchical authority. Recent decades have united individualists and hierarchists against egalitarians. The former two groups have found a home in the Republican party; the latter live as Democrats. Not that voters have so clearly divided themselves into camps. Rather, party activists push Democrats toward making people equal and Republicans toward freeing markets or supporting authority. Thus the Democrats identify with "the poor and oppressed" and the Republicans favor the free market (on the one hand) and the established rich (on the other). Although he recognizes the flaws in the latter's policies—and the internal squabbling that an allegiance to two cultural traditions brings—Wildavsky finds the former's egalitarianism more dangerous. Thence this book.

What exactly does Wildavsky fear? He succinctly tells us in the following: "I believe that rising egalitarianism will lower our standard of living, decrease our health, debase public discourse, lower the quality of public officials, weaken democracy, make people more suspicious of one another, and (if it be possible) worse. Worse is the constant denigration of American life—our polity, economy, and society—with no viable alternative to take its place" (p. xxx).

Quite a laundry list! At its heart is a diatribe against what Wildavsky sees as the leftist desire to remove all distinctions between people: to make sure people have not only equal opportunity, but equal success. He is quite right to label this desire a cultural urge. The attempt to give all people total control of their lives has an ideological, not a structural, source.

Despite jettisoning the truly interesting part of Douglas's theory—its grounding of culture in institutions—Wildavsky's schema has merit. Distinguishing individualism from egalitarianism, for example, improves on Bellah et al.'s analysis of American political culture (in *Habits of the Heart* [University of California Press, 1985]). As usual, more concepts make for better analysis. At least they would make for better analysis if Wildavsky bothered to present any more than impressionistic evidence. Aside from one decent study of party activists and a poorly presented study of risk perception, however, Wildavsky's evidence is anecdotal. This is not enough on which to base a serious work.

Herein lies the book's greatest flaw. Although he traces the rise of "radical egalitarianism" in diverse venues, from the aforementioned political party activism to the American Catholic bishops' letter on the economy to the Bork nomination, Wildavsky nowhere proves his case. At times, this material is interesting—for example, the data on Republican and Democratic activists. At other times it is just wrong—his assertion that the only thing that changed about the homeless between the 1950s and the 1980s is that we used to call them bums, drunks, and winos, and now we use a term that implies they are entitled to a home whether they want one or not. A more thorough look at the literature (e.g., P. Rossi's *Down and Out in America* [University of Chicago Press, 1989]) might have eliminated such egregious errors. It would take more than this, however, to make Wildavsky's argument strong enough for serious scholarly consideration.

To some degree this is the fault of the book's form: Wildavsky put it together from a series of lectures at American University, augmented by other of his writings (some quite old). The result, which is often entertaining and sometimes tantalizing, unfortunately leaves me unsatisfied.

Goals in Space: American Values and the Future of Technology. By William Sims Bainbridge. Albany, N.Y.: State University of New York Press, 1991. Pp. 268. $17.95.

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The American public has had a capricious relationship with spaceflight, as is shown by the wide fluctuations in the popularity of science fiction in various media, the NASA budget, and a host of other indicators. With *Goals in Space,* William Sims Bainbridge has elicited, documented, and codified the rich and complex texture of what various modern groups expect from spaceflight and the relative values they assign to these goals.

In a previous, and too little noted work (*The Spaceflight Revolution: A Sociological Study* [New York: Wiley, 1976]), Bainbridge advanced an intriguing and radically revisionist notion. Common interpretation saw the military and governments of Germany, the Soviet Union, and the United States as co-opters of the spaceflight pioneers and rocket engineers. Bainbridge stood this on its head, and argued that the spaceflight pioneers had in fact co-opted their governments into providing the means for their transcendent goal of interplanetary travel. They had, in effect, disguised their space rockets as weapons in order to get them inside the city walls. Emerging from the Trojan Horse, however, the spaceflight advocates were compelled to stand on their own and develop convincing justifications for society's spending large sums on flight through space. It is these justifications, filtered through decades of experience with spaceflight, to which Bainbridge has now turned his attention. Just what does spaceflight mean to modern American culture, and what value does it have?

In the present work, Bainbridge examines how both transcendent and mundane justifications for spaceflight have penetrated recent popular culture. Adapting the concepts of Thomas S. Kuhn (*The Structure of Scientific Revolutions* [University of Chicago Press, 1962]), Bainbridge distinguishes normal from revolutionary motives for spaceflight (p. 7). The former require broad popular support and provide for a sustained space program. The latter need not achieve wide popular acceptance to be influential, and can, for brief periods, energize a society to spend lavishly on spaceflight. It is the state of the normal motives that he seeks to assess in this work, with comparisons to the prevalence of the revolutionary motives.

The first chapter includes an analysis of existing survey data, and explains the motivations for his two-stage survey strategy. First, free-form questions elicit reasons for supporting spaceflight in the respondents' own words; then, modified forms of these reasons are fed back for judgment in a quantitative survey where respondents rate the importance or value of these various justifications.

The second chapter reports on a pilot study made up of two sets of surveys conducted in the 1970s. In the first, science-fiction enthusiasts were interviewed to elicit a variety of descriptions of space goals. A questionnaire based on these interviews was then mailed to another similar group with the request that respondents indicate their degree of support for each of the goals. In the second survey, Bainbridge elicited descriptions of space goals from members of the American Institute of Aeronautics and Astronautics, the professional association for aerospace engineers. From this he extracted 49 goals, which he asked a random sample of registered voters in Seattle, Washington to assess in a similar way. Factor analysis produced correlations, which were then used to arrive at five categories of goals. In order of popularity among the Seattle voters, they were: information (technical and scientific knowledge is expanded by spaceflight), economic-industrial, military, colonization, and emotional-idealistic. The first three he considers "normal" justifications, and the latter two "revolutionary." The aerospace engineers were mostly associated with the normal goals, the science-fiction enthusiasts with the revolutionary goals.

The bulk of the book, chapters 3–8, reports on surveys conducted in 1986 and based on methodology verified in the pilot surveys. First, 1000 Harvard University undergraduate students were given a free-form survey, from which 125 space goals were extracted. These became the basis for a second survey in which respondents were asked to rate the value of each goal on a 0–6 scale. Factor analysis was not used here, as it was in the pilot study. Rather, a novel technique called block modeling (developed in part by Bainbridge himself) was employed. In general, the results were similar to those from the pilot study. Each of the main clusters of goals that emerged is analyzed in a separate chapter: technical, scientific, and industrial; economics; idealism and emotional; military and international competition; colonization of near-earth space and other planets; and communication with extraterrestrial intelligence. Each of the chapters is rich in detail, containing specific responses as well as astute analysis.

In general Bainbridge is very open and forthcoming about methodology, his own doubts, his justifications for a host of choices made during the course of the studies, and many other issues. This provides a wealth of information allowing the reader to judge evidence and conclusions critically. Beyond clever methodology, rich data, and insightful analysis, he has also given us a book that is a pleasure to read.

Demanding Democracy after Three Mile Island. By Raymond L. Goldstein and John K. Schorr. Gainesville: University of Florida Press. Pp. xxi+246. $34.95 (cloth); $16.95 (paper).

Eugene A. Rosa
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In the decades following World War II, the United States was in its cups over technology, especially technology's ability to ensure continued prosperity and economic leadership. Yankee ingenuity and inventiveness, having just realized the alchemist's dream of transmutation (with fission technology), would continue to create increasingly sophisticated tools, machinery, and production processes. An ethos of techno-optimism, questioned by few Americans, prevailed. This ethos, by stirring intoxicating beliefs in unlimited material growth, veiled fundamental changes in the character of technology. Technologies, owing to vast increases in size and sophistication, were becoming more complex and more difficult to manage—indeed, appreciably more risky. Beyond a circle of technical experts, few noticed.

Then, conspiring against this insouciance, came Three Mile Island (TMI), Bhopal, Challenger, and Chernobyl: the rage quartet. Coming first, as it did, the accident at the Unit 2 reactor of the Three Mile Island nuclear power plant near Harrisburg, Pennsylvania, reawakened national sensibilities to the risks associated with nuclear power; it also set in motion a public mistrust, to be deepened later by the other three members of the quartet, not only of risky technologies but also of the people who manage them. With TMI as its leader, the quartet played to a public rage that eroded technological optimism.

Citizens living in the immediate vicinity of such technological disasters, there is little doubt, experience the greatest rage. *Demanding Democracy after Three Mile Island,* by Raymond L. Goldstein and John K. Schorr, is an attempt to capture local rage with an in-depth examination of the effects on two adjoining towns adjacent to Three Mile Island: Newberry Township and Goldsboro. Most dramatic and noteworthy of the accident's immediate effects was the evacuation by a majority of community residents, even though an official evacuation order was never issued. The longer term effects were assessed over a seven-year period, 1979–86, during which the authors collected, and attempted to triangulate, data from a variety of sources, including six community surveys conducted using author-trained volunteers from a community-action group. The book's goal is to demonstrate that the social, psychological, and political effects on these communities—and the consequent disenchantment with industry and government—reveal fundamental flaws in the democratic control of technology. Correcting these flaws, it concludes, calls for greater grassroots involvement in decisions over risky technologies—a more direct, participatory form of democracy. With goal and conclusion stated at the very outset, the book's larger theme is revealed, namely the intent of Goldstein and Schorr to contribute, not only to the sociological understanding of technological risks, but also to a public philosophy of risk.

Because community effects and consequent community transformation are the book's main analytic foci, the authors must first establish a preaccident baseline for gauging the variety and degree of impact. Although brief, the baseline (via historical and contemporary evidence) is interesting and revealing; especially the fact that the siting of nuclear plants in the area met with virtually no local resistance: residents were typically unmindful, indifferent, or in some cases, unabashedly proud of the reactors. Thus, when the most serious nuclear mishap in America's history happened on March 28, 1979, the community was taken totally unaware. With the ensuing fear and uncertainty community support all but vanished—replaced by angst and mistrust.

To understand the communities' transformation the authors attempt to unpack the layers of citizen concern, principally through in-depth interviews with Newberry residents. Ten, "representative," verbatim interviews are reproduced in an early chapter. This is undoubtedly the most engaging section of the book. These vividly descriptive interviews are also a useful reminder of how knowledgeable and articulate "ordinary folk" can quickly become, once politicized and enabled. Unfortunately, the chapter also begins to reveal a chain of methodological indiscretions, none of which are fatal in themselves, but which taken together, seriously vitiate the validity of the book's conclusions. For example, the reader must largely accept on faith the authors' claim that the 10 selected interviews "offer a sense of the people of Newberry" (p. 36), a claim embarrassed by the exclusion of residents not concerned about the accident. In effect, the authors have selected on the dependent variable they wish to test. Aside from this damaging flaw, the sample contains other key problems: among these is the overrepresentation of women (seven vs. three), including those either pregnant or with small children—all key factors known to accentuate fears and distrust of nuclear technology. Other flaws include, for example, the uncritical acceptance of newspaper accounts (sometimes sensational ones) to support key points.

Following the interviews, summaries are presented of the six surveys conducted over the seven years of the study. They are intended to document perceived danger, health concerns (including, interestingly, concerns for farm animals), psychological distress, and loss of trust among residents. But here, too, methodological flaws—such as the failure to compare their findings with comparable results in general and community samples—derail the conclusions. Indeed, the authors' analytic style here leaves the impression that the data's key purpose is neither to test hypotheses nor evaluate propositions, but to reaffirm a preordained conclusion.

Their account of the community response to the TMI mishap, the authors believe, will both reveal issues common to all communities at risk and deepen our growing understanding of the well known NIMBY (not in my backyard) phenomenon. At broad levels of abstraction this belief is unassailable. At the same time, however, it masks important nuances—the differences, for example, between risks from nuclear versus nonnuclear facilities, or between risks from different kinds of nuclear facilities (e.g., power plant versus waste dump)—whose examination is pivotal to an understanding of the topic. Curiously—in view of their aim of revealing generic issues—the authors leave unaddressed the pervasive finding of community stigmatization after a technological disaster.

Despite a beginning of great promise and despite the authors' honest commitment to address a serious and growing social problem (the democratic governance of risky technologies), the book is ultimately disappointing. The chief source of the disappointment is a failure to explicate the scope of investigation and to specify the operational correspondence rules for disciplining the investigation with the data presented. Furthermore, the adoption of a triangulating orientation, rather than strengthening the analysis, seriously detracts from the book's overall coherence. Thus, the reader is left, not with a sharp, unified picture, but with a muddled melange (pastiche, for postmodernists) of snippets of insight from logically unconnected domains of inquiry. Consequently the book will probably not appeal to a general audience, although it may be of some use to scholars deeply focused on nuclear power, risky technologies, communities, or the democratic governance of technology.

Order without Law: How Neighbors Settle Disputes. By Robert C. Ellickson. Cambridge, Mass.: Harvard University Press, 1991. Pp. ix+302. $39.95.

Jason B. Jimerson
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"Members of a close-knit group develop and maintain norms whose contents serve to maximize the aggregate welfare that members obtain in their workaday affairs with one another" (p. 167; italics in original). This is the hypothesis in *Order without Law: How Neighbors Settle Disputes* and Robert C. Ellickson convincingly presents it in 16 chapters that are divided into three sections.

He opens the first section with a critique of Coase's "The Problem of Social Cost," in which Coase argues that when transaction costs equal zero, ranchers who let their cattle stray will cause legal authorities to prohibit trespassing or cause their neighbors to pay them to prevent cattle from trespassing. Or, as Ellickson puts it, "when transaction costs are zero a change in the rule of liability will have no effect on the allocation of resources" (p. 2). He then describes how events in Shasta County, California enabled him to test Coase's theorem. Shasta County has different rules of liability in the forms of open and closed ranges and laws pertaining to traffic accidents and property violations. The first section records trespassing violations and reactions to those events. Ellickson finds that most ranchers in Shasta County avoid trespassing on their neighbor's land, even in the absence of specific laws and penalties for doing so.

In the second section he presents his theory of norms. He opens with taxonomies of social control in chapter 7. This chapter, which consists of definitions, raises as many questions as it answers, because the book focuses on one form of social control—norms. Chapters 8 and 9 review other theories of social control and game-theoretic discussions about cooperation. The chapter to read is chapter 10, in which Ellickson defines, explains, and qualifies the hypothesis that norms maximize aggregate welfare, regardless of how participants distribute that welfare among themselves. The last chapters in section 2 discuss types of welfare-maximizing norms: (*a*) substantive norms, (*b*) remedial norms, (*c*) procedural and constitutive norms, and (*d*) controller-selecting norms. Throughout the book, Ellickson uses empirical examples to inform and reinforce his theory.

The third section contains two chapters. Chapter 15 discusses the possibility of welfare-reducing norms and other kinds of contrary evidence. Chapter 16 discusses the implications of Ellickson's theory and findings regarding the relation of formal legal controls to informal normative orders. Anyone studying norms should read *Order without Law: How Neighbors Settle Disputes* for both its theory and its examples.

In fact, the examples undermine the theory. Ellickson defines norms as "the rules that emanate . . . from social forces" (p. 127). This implies that he considers norms socially recognized rights to control other people's actions; an implication supported by his discussion of norm-enforcement activities. This leaves Ellickson's theory of norms vulnerable to the criticism that in such theories sanctions, not norms, cause people to cooperate. Furthermore, in most encounters, people maintain control over their actions. Residents of Shasta County complained about Frank Ellis's norm violations but they did not control his actions through norms (e.g., p. 63). People do not impose norms in the same way that governments enforce laws. Visibly refusing to comply with laws will result in one's own incarceration or the confiscation of property by police. Visibly refusing to comply with norms will result in social sanctions that ultimately rely on one's own acquiescence.

Ellickson discusses the need for self-acquiescence in his exposition of escalating sanctions (pp. 57–64). Yet he overemphasizes sanctions as a means of insuring conformity. He underemphasizes the strategic interaction occurring between deviants and sanctioners, as they both try to influence each other's activities. Chapter 12 on social redress is disappointing. Ellickson asserts that remedial norms contain penalties for violators and sanctioners alike; that is, sanctioners will be punished if they exceed penalty limits. Aside from downplaying the rationality of deviance, Ellickson's observation that remedial norms typically require sanctioners to punish norm violators without rewarding (and perhaps at a cost to) themselves is insightful (p. 216). He demonstrates that norms increase welfare, but how does he know they maximize welfare? He also gives little detail about how people "gave notice" to alleged norm violators. What diplomacy did potential adversaries utilize?

Order without Law: How Neighbors Settle Disputes uses theory and ethnography to explain norms in a manner that sociologists would do well to imitate. Robert Ellickson presents evidence in an objective style that allows readers to reach their own verdicts, and his skillful storytelling accentuates his theoretical acumen. This book is an important contribution.

Manufacturing Knowledge: A History of the Hawthorne Experiments. By Richard Gillespie. New York: Cambridge University Press, 1991. Pp. x+282. $39.50.

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McMaster University

"How did the activities of five women workers engaged in assembling intricate electrical components in a huge Chicago factory come to be seen as representative of the behavior and aspirations of all workers?" (p. 264). The enduring salience of the Hawthorne research—in academic sociology, in introductory texts of industrial psychology, and in popular business books—is the central *explanandum* of this meticulous work. Gillespie's thesis is that knowledge is manufactured and that the Hawthorne researchers and subsequent interpreters did especially well, in part fortuitously, in stabilizing the intermediate results of the Hawthorne research to generate a polished, coherent, and consistent final product.

As the title suggests, *Manufacturing Knowledge* is about two interrelated production processes: the production process at the Hawthorne plant that turned out electrical components, and the more diffuse process of manufacturing a body of findings—knowledge—from the often haphazard schemes and partial plans that came to be known as the Hawthorne experiments. Although Gillespie is most naturally interested in the latter production process, and remains rather reserved about his own interpretation of the incidents in the Relay Assembly Room, the wealth of archival detail and the care with which the material is marshaled yield an account of activities in the Hawthorne plant that will have lasting value. Indeed, I suspect the interested reader can find valuable supplementary and often corrective material to all the published work on Hawthorne.

The focus of the book, however, lies in the production of knowledge, much of it subsequent to the period of experimentation itself, and is closely tied in with the unusual career of Elton Mayo. Insightful yet shallow, powerful yet often marginalized by many of those he sought to convince, Mayo's paradoxical character is important throughout many of the events leading to the final publication of the Hawthorne results. Although he, himself, began the period as an advocate of bizarre physiological explanations of behavior, as interested in vascular skin tests as in the sociology or the anthropology of industry, and even though the small amount that he did write was often dismissed—"the sociologically obvious reported with an air of breathless discovery," as Princeton sociologist Wilbert Moore put it (p. 260)—Mayo's ability to revitalize the flagging Relay Room investigations (if belatedly), and, most of all, his success in the development of a network across corporate, financial, and intellectual worlds, were critical in the success of the production (and perhaps one should also say in the marketing) of the Hawthorne experiment. Gillespie's account presents a vivid portrait of Mayo and the milieu which he in large part developed.

What is finally surprising, however, is how little direct effect Mayo had on the published material that has come to comprise "The Hawthorne Experiments." Although Mayo wrote *The Human Problems of an Industrial Civilization* (New York: Macmillan, 1933), and although he influenced his junior Harvard colleague T. N. Whitehead's simple but lengthy statistical account, *The Industrial Worker* (Cambridge, Mass.: Harvard University Press, 1938), the classic account was left to Mayo's protégé Fritz Roethlisberger and Hawthorne personnel manager Bill Dickson: it is through their *Management and the Worker* (Cambridge, Mass.: Harvard University Press, 1939) that the importance of the Hawthorne research has persisted to the present. Of course, this detailed exposition of events at Hawthorne and the description of the researchers' own processes of stumbling toward the truth parallels Gillespie's book. However, as Gillespie makes clear, Roethlisberger and Dickson's ex post and largely linear reconstruction of a process of steady research, sudden revelation, and subsequent refinement of the research focus does not square with the confusions, false starts, and conflicting conclusions that abound in the archival material. The distinctive feature in this case lies not in the actual path of research (compare, e.g., Peter Galison's *How Experiments End* [Chicago: University of Chicago Press, 1987]) but rather in the degree to which Roethlisberger and Dickson sought to document their learning through mistakes in the sequence of Hawthorne experiments. As Gillespie nicely puts it, "the objectivity of the findings seems to be guaranteed by the researchers' confessions" (p. 3).

Two areas of Gillespie's account have some weakness. First, he explicitly eschews an extensive consideration of other interpretations of Hawthorne (pp. 89–95), so that the treatment of the experiments by the social scientific community receives comparatively brief attention. To the extent that academic respectability may underlie the wider influence of the studies, both directly in the MBA classroom and by more diffuse means, this is a significant omission. Second, and more generally, Gillespie does not pursue at length a depiction of the social context in which the Hawthorne research was marketed. Almost all of the factors developed so carefully in the book—Mayo's appeal, the Harvard-based networks in academia and industry, the cogency of Roethlisberger and Dickson's classic account—are supply factors, and it seems that, alone, they would not have given the Hawthorne research wide currency had there not also been a demand for the types of finding upon which the research eventually stabilized.

Between Markets and Politics: Co-operatives in Sweden. By Victor A. Pestoff. Boulder, Colo.: Westview Press, 1991. Pp. ix+244. $43.75.

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University of Minnesota

Sweden's renowned experiment in social democracy as a middle way between decentralized capitalist markets and the (now largely defunct) communist command economies makes considerable use of the co-operative form of social organization. By 1988, the nation's major consumer co-operative (Koopertiva Förbundet), composed of some two million members in a society of only eight million, was also the nation's third largest employer (ahead of Volvo) and its seventh largest in terms of sales (p. 82). The nearly 900,000 members of some 600 producer co-operative associations accounted for almost all the farming and nearly half the food processing (p. 112). Clearly, these organizations warrant study for their substantial economic impact. Their sociological value, however, lies in the unique structural features that directly unmask the classic debates over organizational size, commitment, governance, and citizenship.

Victor Pestoff's descriptive approach is primarily historical, tracing the spread of the Swedish co-operative social movement from the beginning of this century, detailing the drift from member-participatory to leader-dominated organizations, and examining the formal patterns of intra- and interorganizational relations. His theoretical stance draws heavily on Albert Hirschman's exit-voice-loyalty dilemma and Mancur Olson's speculations about the size-apathy connection in stifling collective action. His empirical evidence relies on secondary data sets—ranging from member surveys, to membership records, to interlocking directorate networks and organization charts—with their various intrinsic limits to interpretation. Perhaps the weakest ingredient in an otherwise comprehensive story is the author's infirm grasp of statistical techniques, most notably in the causal diagrams whose numerical values turn out to be correlations rather than path coefficients (pp. 94–95). The 10 chapters display enough redundancy to suggest that they were spliced together from previous articles and working papers without a thorough editorial integration. On the whole, Pestoff presents a compelling case that consumer and producer co-operatives face numerous dilemmas threatening their future viability. Yet, he fails definitively to establish the specific conditions under which each predicament emerges.

The interactive model guiding the book consists of four resource environments confronting co-ops: competition, personnel management, members, and public policy influence. How co-operatives deal with these factors results in a unique organizational form distinct from other types. As economic actors, co-op products and services must compete in capitalist markets against those of private commercial firms. As employers of hired labor, they must bargain with their own workers and professionals over labor-management issues. As membership associations, co-ops encounter the well-known dilemmas of motivating mass participation and sustaining democratic controls, and, as interest groups, they lobby state agencies for legal and regulatory conditions favorable to their operations. Successfully resolving the "logic" in one co-operative environment tends to generate additional burdens in the other environments. For example, consumer co-operatives' efforts to satisfy cost efficiencies in the marketplace erode responsiveness to members' needs by turning them from participants into customers or agricultural co-operatives can join employers' associations, which, by being dominated by private firms, can bring the co-operatives into labor relations conflicts over wages and job conditions. These inherent contradictions present persistent dangers that the co-operative form will evolve into a different type of social organization, be it bureaucratic, corporate, based on voluntary association, or a social movement.

Pestoff's explicit policy aim is to identify optimizing instruments that could enable co-operatives to maintain their equilibrium among the contending dilemmas posed by the four logics. He suggests that co-op leaders annually draw up a "social balance sheet" to tally gains and losses in all the relevant environments. In addition to such conventional data as sales, market shares, and investments, the report would also include detailed information "about labor and training, the social bases of the co-operative, its democratic activities and its activities in terms of policy vis-à-vis its own members" (p. 173). Unfortunately, Pestoff falls considerably short in explaining how co-ops might apply such balance sheets in their planning activities so that dangerous tendencies can be caught and corrected. Renewal or transformation is a central conundrum challenging all modern organizations in our era of exploding technology and imploding social relations. Victor Pestoff has done a singular service by drawing social scientists' attention to the unique circumstances of consumer and producer co-operatives. The solution to their many quandaries, however, remains elusive.

Masters to Managers: Historical and Comparative Perspectives on American Employers. Edited by Sanford M. Jacoby. New York: Columbia University Press, 1991. Pp. xii+249. $35.00.

Olivier Zunz
University of Virginia

In *Masters to Managers,* nine authors reassess the social consequences of managerial struggles in order to examine the ways employers remodeled the American workplace from the 1830s to the 1950s. This volume is distinct from previous efforts in that it emphasizes managers' strategies to control the shop floor rather than the workers' collective action.

Employers' behaviors were multifaceted as Walter Licht shows in his essay on Philadelphia firms from 1850 to 1950. Numerous Philadelphia employers, who owned medium-sized firms, never articulated personnel policies but instead trusted their plant superintendents with the hiring, firing, and supervising of laborers. Others found themselves developing a formal employment system as their investment in paternalism was undercut by unionization. Most Philadelphia employers situated themselves in a continuum between these extremes.

With this pattern of multifacetedness established, the authors debunk several long-held assumptions. Jens Christiansen and Peter Philips show that, contrary to common belief, mechanization in the shoe industry (1830–60) did not necessarily entail speedups. Speedups were instead the result of the employers' strategy of tying production closely to inventory and market demand because "just-in-time" production minimized financial risk. In turn, Daniel Nelson proves that, as late as the late 1920s, Taylorism, at least in a technical sense, had affected less than 10% of the American work force. The supposedly massive and rapid deskilling of American workers is therefore more myth than reality. Less convincing is Daniel Raff's assertion that the famous 1914 Ford profit-sharing program was motivated exclusively by economic rationality. That claim is reductionist. Not only does the author fail to provide economic data in its support, it is well known that a complex of reasons motivated the idiosyncratic Henry Ford.

In its second part, the volume tackles the political dimensions of employers' strategies. Howell Harris shows that, although the raison d'être of employers' association in the metal trades of Philadelphia was union busting and strikebreaking, such associations also promoted industrial research in universities. Only by joining resources could medium-size firms absorb the cost of addressing larger issues relevant to the trade as a whole. In turn, Daniel Ernst brings a welcome corrective to the accepted view that turn-of-the-century judges were uniformly hostile to social reform and the labor movement. He uncovers a trend of judicial accommodation, where judges supported unions as long as they resorted to peaceful and orderly tactics. Although not lenient with labor activists, some judges were not subservient to employers, either.

In the concluding comparative chapters, Gerald Friedman studies French paternalism and Sanford Jacoby revisits American labor conflicts. French employers, who competed with workers for the Republic's favors, never had the free rein of their American counterparts. American employers, by contrast, were "considerably less constrained in expressing their hostility" to the labor movement. Jacoby's theory explains why the American labor movement was both less successful and more bloody than its European counterpart. The American labor movement was simply more violently crushed. This work, although at times difficult to read, is rewarding.

Making America Corporate, 1870–1920. By Olivier Zunz. Chicago: University of Chicago Press, 1990. Pp. x+267. $24.95.

Harold Perkin
Northwestern University

The late 19th-century revolution in business organization has been extolled by the Right as the creator of the affluent society and reviled by the Left as the source of unacceptable concentrations of economic and political power. Both sides have focused attention on the "giants of enterprise" or "robber barons," according to taste, who amassed vast fortunes from the phenomenal growth of big business. Little attention, however, has been paid, outside satire and fiction, to the thousands of small men and women who were swept into the cavernous maw of big business, without whom the new ways of life and work would have been impossible. This new middle class of salaried white-collar workers, from the middle managers and supervisers down to the salesmen and typists, has now been rescued from obscurity by Olivier Zunz. He argues that they were not passive observers of their lot but active participants in the development of a new world of work, built around stable careers and suburban living.

Zunz's scholarly book is based on the personnel records of five giant corporations during the years of their formation: the Chicago, Burlington, and Quincy Railroad; the E. I. Du Pont de Nemours Powder Company and its extensions into chemicals, plastics, and other products; the Ford Motor Company; the McCormick Company (later the International Harvester Company) in agricultural machinery; and the Metropolitan Life Insurance Company. These five span the whole gamut of American big business, from the railroads that first pioneered the large corporation through mass manufacturing to finance and investment. Zunz eschews the all too familiar story of the "innovative geniuses" or "malefactors of great wealth" made famous by the muckrakers and free-market triumphalists of the Progressive Era, and chronicles instead the humdrum lives of the white-collar workers who built and inhabited the bureaucracies which, long before government caught up, became the characteristic form of modern large-scale organization throughout the world.

The gains in efficiency, income security, and stable careers were at the cost of a "lost autonomy," the loss of the independence and self-directed enterprise of the old-fashioned merchant and craftsman, who were bought out or crushed by the agents and salesmen of the new corporations, and were forced to join them or send their sons and daughters into their soaring office buildings. Between 1870 and 1900 the proportion of clerical workers grew from 1% to 3% of the U.S. work force and by 1940, it was 10%. Along with managers, salespeople, and other professionals, by midcentury, they made up no less than 37%, the largest single block in the social structure.

Women, too, played an increasing role, rising from almost no women being office workers in 1870 to their being about one-quarter of that group in 1900 and over 60% of it by 1950. Although they rarely rose above the routine jobs in typing, stenography, and the switchboard, and were usually forced to leave upon marriage, the "Miss Remingtons" of the advertisements paved the way for the feminists of the later 20th century. Along with their male colleagues, something like half of whom came from blue-collar families, they also contributed to the "American dream" that was the antidote to socialism and political discontent.

The companies had different approaches to the problems of organizing large-scale business, though they evolved in the same direction in the end. The railroad was forced by its geography to delegate system building to its local managers, spread over 5,000 miles of track in seven states and 25 cities. Pierre Du Pont and his cousins took a more theoretical approach to the management of their many divisions, including, for a time, their rescued giant, General Motors. Henry Ford abominated bureaucracy and preferred intuitive "tinkerers" like himself, but could not avoid the bureaucratic demands of mass production. International Harvester's managers, dispersed among the farming communities of North

America, inevitably became leaders and spokesmen for the farmers against other corporations, banks, and railroads. Metropolitan Life, whose policyholders came by the 1920s to include one in every six American families, found itself drawn into welfare measures like nursing, health education, and public health policy.

Thus the corporations and their employees, often loyal to a fault, transformed American life and society and built the world of work most of us are now embroiled in. That world has, or had, many attractions: the stable career and rising incomes for a growing segment of society, fringe benefits like sick pay, paid holidays, retirement pensions, even stock options and bonuses. For a time America led the way in both "the good life" and the political and military power that came with gigantic productivity. The Depression of the 1930s slowed down this everlasting bonanza, and Zunz's book only hints at the stress and boredom of mass production, as being as much a feature of office work in the skyscraper cathedrals of big business as in the now-outmoded assembly plants.

Zunz, despite his caveats, emulates Candide and plays down the other side of the picture: the overwhelming embrace of the company, its intrusion into the private lives of its employees, its demand for total loyalty, its disruptive transfers of managers' and salesmen's families to distant, friendless places, the massive, sometimes violent strikes in which the white-collar workers were expected to support the employer without question, the use of state force and the manipulation of local and national politicians, the destruction of whole communities by the closure of plants, and all the sins of big business identified by the Progressive movement. To understand all is *not,* pace Madame de Staël, to forgive all. At a time when big business can no longer guarantee stable employment, when, indeed, by exporting capital and jobs it has created the largest trade deficit in history by importing goods made overseas by American (much more than by Japanese) multinational corporations, a study of the making of corporate America that leaves out the darker side of the story is, despite its many virtues, perhaps less than ideal.

Racism and the Incorporation of Foreign Labour: Farm Labour Migration to Canada since 1945. By Vic Satzewich. New York: Routledge, 1991. Pp. x+241. $75.00.

Frank Bonilla
Hunter College

With transnational worker migrations coming to the fore, both as a central feature of the ongoing internationalization of the world economy and as a key concern of state policy in emergent moves toward hemispheric economic integration, this account of Canada's postwar treatment of foreign labor provides an important historical and theoretical grounding for viewing contemporary developments on this front. Vic Satzewich sets out to revalidate a Marxian, political economy approach to the analysis of the forces driving the movement of workers between nations, highlighting the role of the state in recruiting and allocating as well as excluding contingents of foreign labor. The book moves from a broad critique of Canadian and European immigration research to a detailed account of the Canadian government's shifting response to shortfalls in the supply of agricultural labor since 1945. A contrasting of state approaches to the mobilization of internal labor reserves as against European and Caribbean contingents available to fill these gaps constitutes the main body of the work.

Asserting that there is a void in Canadian immigration research with a political economy perspective, the author articulates his own view chiefly via a critique of Stephen Castles and Godula Kosack's seminal work, *Immigrant Workers and Class Structure in Europe* (London: Oxford University Press, 1973). Satzewich faults Castles and his circle of coauthors for underplaying the role of the state and treating state action as narrowly instrumental and functionalist rather than ideologically and politically charged. Castles's work is also said to confound migrants (persons admitted for temporary, generally contractual stays) with immigrants (those admitted as potential permanent settlers). The author's scheme calls for a strict conceptual separation of these categories of foreign entrants. (In U.S. usage, of course, the term "migrant" is generally applied to workers on the move internally.) Beyond this, he shares common ground with Castles both in regarding the processes of capital accumulation and uneven development as the principal shapers of labor demand and in seeing the operations of the labor theory of value and the role of workers themselves as generating their own "redundancy" or marginalization within the labor force. The interplay of the political, ideological, and cultural in historically specific contexts, the author insists, is the bedrock on which successful analysis of particular immigration experiences must build.

Immigration is thus cause and consequence of the process of capital accumulation (p. 8). The challenge and dilemma for state policy is to balance internal and external labor reserves according to the demands of accumulation and the long-term social and cultural unity and harmony of an idealized, "imagined" national community. There are thus, on the one hand, calculations of demand—the magnitude, duration, location, and quality of human capital sought by employers—but, on the other, judgments about the suitability and assimilability of human contingents. While many criteria may validly enter into these calculations, what Satzewich demonstrates as regards farm labor in Canada is the primacy of racial conjectures—stereotypes, prejudices, raw ignorance—in official discourse on this theme. The racialization of immigration policy, candidly conveyed in government documents and debate, thus emerges as the major ideological strand that has generated errors, misfires, conflict, and turbulence that have compromised economic policy and democratic values in the Canadian example. The volume documents the special weight of these dilemmas in the search for external low-wage reserves as well as in the treatment of internal reserves in the lower tiers of the native work force. These too may be subject to "character assassination" (p. 152), especially if they have won wage and benefit claims that can subsequently be depicted as excessive.

This is a compact and condensed treatment of a fairly narrow sector of the Canadian economy. It is, nevertheless, rich in implications and insights that may inform research elsewhere. Though the issue is not pursued in detail, the tension between the economic as against the ideological or racial and where, in this regard, the bottom line lies when a choice is forced, is ever present in this account. Over time the sources tapped have ranged widely, encompassing indigenous peoples, children and youth, prisoners of war, Japanese internes, displaced Eastern Europeans and white U.S. southerners. A comment in the closing pages attributed to a Canadian member of parliament conveys at least one reading of how the cards have fallen to date. "If you put pants on a penguin," he asserts, "it could be admitted to this country" (p. 194). That would require, of course, fresh economic, legal, and racialist inventiveness to locate a profit-generating niche and spot in the social hierarchy for correctly attired penguins.

Between Two Islands: Dominican International Migration. By Sherri Grasmuck and Patricia R. Pessar. Berkeley and Los Angeles: University of California Press, 1991. Pp. xviii + 247. $40.00 (cloth); $14.95 (paper).

Douglas T. Gurak
Cornell University

Dominicans have been migrating to the United States in large numbers since the 1960s, though they have not received the scholarly attention that the magnitude of this migration justifies. Sherri Grasmuck and Patricia Pessar have contributed to the limited set of scholarly research articles on Dominican migration to the United States and they have now significantly augmented our ability to comprehend the dynamics of this migration by adding to the very slender list of books on the topic. *Between Two Islands* skillfully blends the analysis of primary data collected by the authors in the Dominican Republic and New York (the principal region of Dominican settlement in the United States) with several other sources of information. They utilize the research of others systematically, incorporating both empirical work and an impressive body of historical research, to make their book an informed and up-to-date assessment of both the descriptive and analytical dimensions of this migration system. While *Between Two Islands* seeks mainly to provide a coherent interpretation of the extent, composition, and sources of Dominican migration to the United States, its authors have carefully conceptualized the process of migration, and they utilize a useful framework to guide their exposition. On balance, this combination of primary and secondary empirical analysis with historical and conceptual inputs works well.

Other scholars have fallen into traps by providing detailed studies of small groups of migrants from a particular origin (such as a village in the Cibao region of the Dominican Republic) and then generalizing from those few and special cases to all of Dominican migration. A quick reading of chapter 3 on the study's research design might suggest that this was about to happen again since most of the primary data from the Dominican Republic comes from three sites in the province of Santiago in the northern Cibao region (the city of Santiago de los Caballeros, a rural town, and a rural village). Even the richness of the data from these sites (site censuses, ethnographies, and surveys) provides no automatic guard against errors in generalization. Grasmuck and Pessar, however, use their primary data to form only part of the image of Dominican migration. One example of this integrative effort is the extensive reliance on other studies to establish several controversial points: that Dominican migrants come from predominantly urban origins, that female migration is of equal or greater magnitude than male migration, and to provide a reasonable overall estimate of the numerical size of the migration flow. The authors' own data offer both new evidence on key questions addressed by other studies and a rich source of examples for building convincing pictures of the migration and settlement processes. The strength of the primary data is its mixture of census, survey, ethnographic observation, and in-depth quotations and the multiple sites from which the data were collected, including survey and ethnographic data from New York.

Several complex and interrelated themes are addressed in separate chapters. Chapter 2 provides a cogent interpretation of the general forces and the Dominican- and U.S.-specific economic and political processes that have helped build the migration system. The authors link this analysis to a network perspective that helps explain why many migrants are not from among the poorest or unemployed, why structural forces only condition (rather than determine) migration, and why migrations such as this persist even as structural conditions change. Chapter 4 utilizes the Santiago data to provide a rich picture of urban emigration and the characteristics of return migration. Chapter 5 focuses on rural emigration and provides a rich historical perspective on the sweeping changes that have contributed to both rural-urban migration and to migration links to the United States. Chapter 6 examines the roles of household and gender in the Dominican migration. Several important issues are raised here. It is argued that viewing the household as a cohesive unit that maximizes resources is simplistic since generational and gender differences in goals and benefits characterize households. Ethnographic examples document differences by gender quite convincingly and illustrate how migration to the United States increases the autonomy of women—a fact that accounts for the disproportionate role females have played in long-term settlement. Chapter 7 describes the situation of both legal and illegal Dominican immigrants in New York, approximating a picture of a complete migration system and raising some new questions about the relationship of legal and illegal migration.

This review has been positive. There are points that I disagree with and a few substantive and technical issues that require clarification, but these issues are minor aspects of a very strong effort. In sum, this is an excellent book about an important and inadequately studied process—migration systems in general and Dominican migration in particular. It is a case study (actually a set of case studies) whose rich historical and conceptual setting renders it very useful for potential comparisons with other migrations. As such the book has value well beyond the provision of a rich depiction of Dominican-U.S. migration. For those most concerned with Dominican migration, this is the best single source; although as Grasmuck and Pessar make clear, it is never enough to rely on just one source or type of information.

Family Dynamics in China: A Life Table Analysis. By Zeng Yi. Madison: University of Wisconsin Press, 1991. Pp. xiii + 197. $37.50 (cloth); $15.95 (paper).

Yu Xie
University of Michigan

Written in English by a Chinese scholar, *Family Dynamics in China* is one of those rare books from which anyone with an interest in the family, contemporary China, or formal demography will learn something. In the book, Zeng Yi skillfully combines his penchant for multistate life tables with an attempt to understand the Chinese family. As a result, the book is not only a solid contribution to family demography and family sociology but also a well-executed exercise demonstrating the usefulness as well as the limitations of the life-table technique.

The book is divided into three parts. The first part depicts the demographic profile of the Chinese family. The second documents the author's extension of Bongaart's family status life-table technique to studying three-generation families. The third presents and analyzes numerical results. Some readers may find it easier to read the third part before the second part, which is full of technical derivations. This shortcut can be justified by the fact that many key qualitative conclusions of the book are insightfully drawn from observed data and logical reasoning without recourse to simulation analysis.

Zeng's argument is a simple and unmistakable one: demographic forces such as changes in fertility and mortality should have a profound influence on the structure of the family. For example, Zeng argues that, controlling for "the desirability of coresidence," the dramatic fertility decline in China since the 1970s should reduce the proportion of nuclear families because lower fertility means fewer offspring available per elderly couple (pp. 138–50, 166). The book's main findings consist of a series of simulated statistics concerning the characteristics of the family and the family experiences of the individual. By varying the input for family-status life tables, Zeng ascertains the effects of the demographic changes between 1950–70 and 1981 and of the demographic difference between the city and the countryside in 1986.

Zeng defines "family dynamics" as the creation of a family, a change in family size, and the dissolution of a family as well as changes in family characteristics" (p. xix). By this definition, the title *Family Dynamics in China* may be misleading, for the book does not adequately address the processes and the determinants of such vital events in family dynamics as marriage, childbearing, divorce, and household separation. Many issues of sociological import (e.g., mate selection, gender roles, intergenerational relationship, and the significance of education and occupation) are overlooked. Instead, the book concentrates on simulating the impact of demographic rates. The analysis relies heavily on Bongaart's method introduced in *Family Demography: Methods and Their Applications* (Oxford: Clarendon Press, 1987) edited by John Bongaarts, Thomas K. Burch, and Kenneth W. Wachter. Unfortunately, the author does not respond to Nathan Keyfitz's and Norman Ryder's critiques collected in the same book. Hence, the following weaknesses of the book reflect the inherent limitations of the method more than the author's application of the method. First, aside from a very limited number of variables such as age and parity, all individuals within a classification are unrealistically assumed to be homogeneous. Furthermore, events are assumed to be independent of each other. Finally, only *females'* family experiences are considered.

Zeng's macrosimulations are essentially deterministic and concerned only with statistics pertaining to the *central tendencies* of the marginal distributions of relevant variables rather than the variations and covariations of the variables in a multivariate and stochastic context. This feature undermines some of the basic strategies adopted in the research. For example, central to Zeng's simulations is the identification of a senior female "marker" for each family, which enables Zeng to conveniently equate a three-generation family with a "nonmarker" with at least one surviving child. Yet, the procedure dictates a narrower definition of three-generation families in excluding families with a lone grandfather and a narrower definition of nuclear families in excluding families with a lone father (p. 58). Even though Zeng is correct in arguing that *on average* a female "marries earlier and lives longer than her husband" (p. 56), it is untenable to assume that a female *always* outlives her husband. Also problematic is Zeng's measure of "the desirability of coresidence" by the proportion of parents willing to live with married children. A better measurement should take into account the proportion of married children willing to live with their elderly parents. Whereas the parents' desire dominates under a high fertility regime, children's desire is more important under a low fertility regime.

By distributing the overall sex ratio (105 males to 100 females) to all families, Zeng also ignores the sex composition of children within a family, which is further complicated by Chinese parents' general preference for sons over daughters. For a fixed number of children in a family, the number of sons varies. For example, for families with two children, the probability of having zero, one, and two sons is respectively about 25%, 50%, and 25%. Given the patrilineal tradition in China, having no son is associated with an elderly couple's lower likelihood of living with a married child. Having multiple sons does not improve the likelihood much beyond that of having a son because parents normally do not live with more than one married child; and the book does not consider the coresidence of married siblings. Therefore, the net result of the random distribution of the sex of children, as compared with the uniform distribution, leads to a lower proportion of parents living with their married children. This effect will become more pronounced as children born during the one-child-per-couple campaign beginning in 1979 reach their prime ages of marriage and childbearing. For this reason, I wonder whether Zeng's conclusion that "the proportion of nuclear families would be brought down by about 20 percentage points under the 1981 rates compared with what would be found under the 1950–70 rates" (p. 166) is an overstatement.

Fifteen Generations of Bretons: Kinship and Society in Lower Brittany, 1720–1980. By Martine Segalen. Translated by J. A. Underwood. Cambridge: Cambridge University Press, 1991. Pp. xiv+323, $59.50.

Rebecca Jean Emigh
University of Chicago

Fifteen Generations of Bretons is a laudable mixture of demography and anthropology, providing a 250-year history of South Bigouden, with emphasis on one commune, Saint-Jean-Trolimon. Throughout the book, Martine Segalen combines qualitative and quantitative documentary analyses with information from interviews. The first chapters establish the demographic backdrop, showing a population increase over the 19th century, a low age at marriage, and high fertility and mortality rates; the remainder relates other social practices to these demographic ones.

Inheritance was partible and bilateral in South Bigouden—male and female offspring inherited equal shares. The most common form of land tenure was *domaine congéable;* the lessee actually owned the reparative rights. Since the tenant could not divide the property, each heir's share was an equal claim to the parents' reparative rights, movables, cash, and animals. If one child obtained the lease, he or she reimbursed the other siblings for the value of their reparative rights. Segalen analyzes the inventories made for inheritance settlements, to show, for example, that life-styles of rich and poor farmers were similar.

From a computer analysis of reconstructed genealogies, Segalen demonstrates that many marriages (and remarriages among widows or widowers) involved distant affinal or consanguinal relatives. This "relinking," used more extensively by the wealthy and politically powerful, somewhat limited the dispersal of patrimony, but did not effectively reconcentrate it, because marriage partners were only distantly related. For Segalen's subjects, however, choosing partners from the correct social space was more important: affinal relatives were vaguely familiar, yet not "kin."

During the 18th and 19th centuries, the economy centered on the cultivation of this region's fertile soil; the previously important maritime activities regained strength in the 20th century with the development of food-preservation techniques. In the 18th century, some peasants successfully obtained several leases and occasionally purchased property. With the population increases of the 19th century, however, peasants accumulated fewer resources, and these were dispersed among more descendants every generation. Relatives exchanged labor services before mechanization slowly reduced the need for festive labor in the 20th century, but the overall importance of family ties did not decline.

This book provides a valuable reference source for researching peasant populations because of its detailed and integrated demographic, social, and economic analyses that produce a fuller understanding of the phenomena. For example, Segalen compares the region's low age at marriage to that of France but does not reduce marriage to this set of demographic rates. She also analyzes the symbolic content of the marriage ceremony and links it to farming; showing how the parents arranging the marriages searched for the ideal situation of a lease about to expire and a youth of marriageable age. Marriage combined equal amounts of resources from husband and wife, occasionally providing enough to establish a separate residence. Several important theoretical debates are specifically addressed in this book, including the continued importance of kinship, despite the narrowing of its economic importance and the cycle of stem family composition, in a region where parents did not choose a successor during their lifetimes.

Other important theoretical issues are not addressed directly. For example, Segalen discusses two patterns of fertility limitation, one a lengthening of birth intervals throughout the wife's childbearing years and the other an early age of last parturition. The demographic literature (though much of it was published after 1985, when the original French version of this book appeared) discusses this spacing/stopping behavior; Segalen contributes little to this debate, since she presents neither birth-interval data (the text suggests that they are available) nor other measures. Second, she argues that population growth coupled with partible inheritance led to the impoverishment of family lines. Partible inheritance, however, does not necessarily narrow the range of the income distribution or impoverish the entire society, though it may make preservation of wealth within any particular line of descent difficult. By accidents of demography and managerial skill, some peasants can accumulate property lost through others' misfortunes. Measures of income variability, which are perhaps not available, are needed. Segalen's interviewees perceive their ancestors as wealthier, but these impressions are not linked to impoverishment or income distributions. Finally, the issue of peasant rationality is underdeveloped. Tales of inherently conservative peasants consuming hard-earned wealth through conspicuous consumption contradict accounts of clever peasants profiting by speculating on subleases. Is this related to the peasants' confusion of economic and social spheres as suggested by labor parties and market excursions serving both productive and nonproductive purposes? Is the disappearance of festive labor a sign of "fresh economic rationality" or a reflection of smaller farm size? These and other missed opportunities to elucidate points of general theoretical relevance limit the book's usefulness.

Dynamics of Family Development: A Theoretical Perspective. By James M. White. New York: Guilford Press, 1991. Pp. xvii+254. $32.50.

Valarie King
University of Pennsylvania

James M. White's *Dynamics of Family Development* is ambitious. He sets out to refine and extend the theory of family development, which is concerned with the processes of family change over time. Families are viewed as experiencing stages of development. As a family moves through these stages, there are changes in the rules governing roles in the family as well as changes in its structure and membership. The timing and sequencing of family stages are affected by institutional norms. The process of family development is viewed as dependent on both state and duration. That is, the probability of a move to a new family state depends on the current state and the duration of time spent in the current state.

In the first part of his book, White provides a general introduction. He briefly discusses the history of the theory of family development and the problems hindering the advancement of this theory, and he begins to lay the foundation for his reformulation of the theory. White presents his rationale for the formalization of the theory of family development, by which he means the transformation of the theory into a mathematical model. He argues that the formalization of a theory assists in theory construction and refinement. Transforming a theory into a formal model will expose areas of weakness in the theory that will require further consideration.

Part 2 contains the heart of the book and deals extensively with theoretical and methodological issues. In chapter 3 White spends considerable time discussing important concepts in the theory of family development, such as stages and events, norms, and levels of analysis. He criticizes past theoretical formulations that viewed change as static or deterministic.

Instead, he views development as a stochastic process whereby the direction of a transition is probabilistic. Not everyone will follow the transition to the next most probable stage. White also criticizes past conceptualizations of family stages particularly for not being exhaustive or mutually exclusive. He rightly criticizes the lack of relevant stage categories for many family forms such as single-parent or remarried-parent families because of a past focus on one "normative" path of family development. In chapter 11 White again picks up the issue of defining stages of family development and offers a new approach to conceptualizing family stages.

In chapter 4 White begins the formalization of the theory of family development. He begins by discussing previous formulations of the theory and then goes on to advance his own. He lays out some of the important theoretical assumptions and propositions of the theory which he then uses to develop a model for the process of family development. White proposes a continuous time, discrete state, semi-Markov model which is a general model from which more restrictive models can be used for data analysis. It was White's goal to present his mathematical model in terms that would be understandable to the general reader and not just to mathematically sophisticated readers who are familiar with these types of models. He does this quite well, spending much time discussing what this model entails and what lies behind the formulas he presents.

In chapter 5 White discusses methodological issues such as the assumptions made by semi-Markov models and the problematic nature of these assumptions. He also discusses the relative advantages and disadvantages of employing different research designs and the difficulties involved in testing the theory. Throughout the book White is concerned with the connections between theory, methods, and data.

In part 3 of the book, White employs empirical data to illustrate certain elements of the theory. His aim is to illuminate parts of the theory rather than to provide an empirical confirmation of it. His examples show the usefulness of theory formalization but also highlight certain problems such as the constraints that data put on testing the theory.

In part 4 of his book White discusses and summarizes his main arguments and conclusions. He also further clarifies issues brought up in previous chapters and suggests further avenues for research.

White has made an important contribution to the literature on family development. He devotes much time to clarifying and carefully arguing his ideas. He anticipates problems and addresses them head-on. White admits that his version of the theory is not the only possible formulation and does not presume that it is complete. Much more rigorous testing of the theoretical propositions is needed than White has provided. White's book should provoke new areas of research and be a springboard for further progress in the theory of family development. It is a must for those working in the area of family-development theory and will be of interest to family theorists in general.

Lao Peasants under Socialism. By Grant Evans. New Haven, Conn.: Yale University Press, 1990. Pp. xv+268. $30.00.

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Lao Peasants under Socialism explores the attempts of the Laos socialist state to fit its peasantry into cooperative agriculture during the decade from 1979 to 1989. Grant Evans takes on theoretically the major proponents of collective agriculture. He finds that the two major rationales for the organization of peasants into large-scale producers' cooperatives are lacking. Evans rejects the arguments given by planners in socialist states that collective agriculture is needed for economic reasons (for economies of scale and to aid command planning that has no place for the market) and political reasons (to avoid the emergence of capitalist class polarization and resistance by peasants to transferring their surplus to the state). He also rejects the views that the peasant household is a natural cooperating group that can congruently fit into the formal institution of the cooperative.

Apart from his incisive theoretical arguments, Evans also looks at concrete evidence in field studies of several cooperatives and shows that indeed cooperativization was either never fully carried out or did not work as planned.

He shows that in general there are no clear economies of scale in rice agriculture. And (unlike China, e.g.) Laos is labor deficient and land rich. The state needs to provide considerable material benefits to motivate peasants to join collectives, and in this study only the few that lacked land (for particular reasons, such as being immigrants) desired to remain in them. Without considerable material inputs, like fertilizer and machines, which the state could not provide, these benefits were lacking to the majority.

This volume is an immensely important contribution to the study of rural social organization, that reaches far beyond the narrow segment of a decade of experience in the small nation of Laos. The author redefines the socialist state's quest to place its peasants into the collective as its attempt to gain control over the natural economy. He finds that the peasant natural economy does not fit congruently into the collective. Furthermore, as the state pressed down and tightened the collective operations, it made the market activities that absorbed peasant energies difficult. Ironically, in the process of forcing its organizational will on the peasants, the collective made the peasants more insular. There was a resurgence of dependence on kin and neighbors, as in the natural economy. Leadership, authority, and loyalty also followed the lines of peasant values. Ultimately, the natural economy won, and collectives fell apart.

Evans both works within the particular structures of Laotian peasantry and shows how family structure, the role of women, and ecology all combined to limit peasant enthusiasm in the collectives. If he had stopped here, he would have written a fine book. But Evans's work reaches beyond the country of study because he takes on the contemporary debate on the moral economy. However, he redefines its terms away from the either/or propositions often used. He rejects the view that peasants are individualists who reckon with an economically rational calculus. He also maintains that peasants are not primitive socialists. When it is convenient and ecologically suitable, they will work in groups, and labor exchange is favored because it provides reliable labor that is high quality because of its normative controls. Yet, if the market provides alternatives to their labor, they will accept or hire wage labor. By posing the issue in such flexible terms, Evans argues convincingly that agricultural co-operation is best accomplished if it is conceptualized as a "mutating adaption by peasant domestic units to a changing ecological and economic environment" (p. 213). This, then, provides a framework for a line of development that would aid planners of the socialist state, a line of development that was proposed by Chayanov. Evans suggests a timely range of strategies for market-style socialism that do not lock the peasants into a rigid vertical institutional form that makes it hard for them to achieve their goals.

The Poverty of Plenty. By Wang Xiaoqiang and Bai Nanfeng. Translated by Angela Knox. New York: St. Martin's Press, 1991. Pp. xxi+189. $59.95.

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There is no dearth of recent books on contemporary China's economic problems and reform strategies. But research on China's underdeveloped "west" that is written by Chinese nationals is relatively unheard of in the West and not readily available to English readers. This book, written by two leading reformist economists, Wang Xiaoqiang and Bai Nanfeng, from the Economic Structural Reform Institute of China, an influential think tank established by ex-Premier Zhao Ziyang before June 1989, is a valuable addition to the literature.

After presenting the duality of staggering poverty versus staggering wealth that has long existed in the region of China under study, the authors then pose the question of why conditions have not been changed since reforms were implemented in 1978. Put simply, the crucial question is why the wealth of natural resources has not been transformed into productive forces to alleviate the poverty of life in China's northwestern and southwestern regions. While 10 years of reform drastically changed the economic landscape in the coastal areas, the already extreme conditions present in the west have been exacerbated, and the regional imbalance between east and west has intensified.

The authors are correct in pointing out that to transform resource advantages into production and economic growth requires changes in the extrinsic socioeconomic structure as well as in intrinsic human factors. They argue that it is necessary to move from the old "natural economy" to a new "commodity economy" and that such a transformation is not possible without first liberating the minds of peasants.

Wang and Bai use the term "loosening the bonds" to refer to the end of the centralized "one big pot" system and the introduction of a production responsibility system in rural China. To them, once the bonds of the old structure are loosened peasants' enthusiasm to produce will increase tremendously. But, they caution that such enthusiasm could go either way: it could create the desirable commodity economy or it could maintain and intensify the undesirable natural economy. They maintain that the aroused enthusiasm among peasants in China's underdeveloped west has been wrongly directed toward the neotraditional natural economy, and this has destroyed natural resources rather than properly developing them. Wang and Bai compare this with the growth of the commodity economy in coastal regions where individual, village, and town enterprises are booming "like bamboo after the rain."

The theoretical explanation offered by the authors for the dissimilar results lies in the "poor" quality of human resources; what they call the internal sociocultural factors of the region, which they believe held back the great transformation. They cite negative religious ideas such as withdrawing from the world and fatalism, traditional condescension toward trade and commerce, resistance to agricultural innovations, and the lack of entrepreneurship as intrinsic cultural attributes that have heretofore obstructed economic change.

This line of reasoning that attributes underdevelopment to existing internal cultural features has been quite popular in the 1980s among some of China's social scientists. This book is another example. Apparently, the modernization paradigm in the sociology of development that has been severely criticized in the West and the Third World since the 1970s has been revived in China. Unfortunately, the drawbacks and problems inherent in such theorizing are also found in this book's attempt to provide sound explanations for the failure of development among China's ethnic minorities in its western regions.

Such criticisms as Great-Han chauvinism, static reasoning in mistaking the psychological and behavioral consequences of underdevelopment for its causes, and taking the central responsibility system as a given policy without questioning its applicability to such a historically isolated region could easily be employed to challenge the book's arguments. The sections on backward social infrastructure in such areas as education, communication, and transportation, and the undiversified economic structure are indeed counterarguments to those who are blaming the victims in this structurally underdeveloped region. The analytical framework presented toward the end of chapter 3 does not help us much to understand fully the multiple causes of the underdevelopment. Moreover, what they term the "immature social system," in which "poor quality of human resource" serves as a dependent rather than an independent variable can say very little about the historical and structural aspects of the "vicious cycle of poverty" in China's west.

The authors further advocate the development of a commodity economy in the region by reversing the strategy of "encircling the cities from the countryside" and argue for an aggressive urban reform as the leading agent for change. Several concrete reform proposals are delineated with a very clear bureaucratic viewpoint, and in the end the state's policies are once again heavily stressed. Those proposed reforms are not much more than economic measures, and the repeated call for changing human quality remains as rhetoric, at best. The emphases on stimulation from the outside, such as more state inputs and the importing of skills might also run the danger of ignoring the roots of the historical development of underdevelopment in the area. Thus the solutions proposed may not be the best solutions after all.

This book is certainly a welcome addition to the literature on contemporary Chinese economic development. Its updated data and facts about the problems confronting China's western border region and the Chinese government's plans for it are of great value. But its potential contribution to the theory of development and underdevelopment is limited.

Visions of the People: Industrial England and the Question of Class, 1840–1914. By Patrick Joyce. Cambridge: Cambridge University Press, 1991. Pp. 449. $49.50.

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First, it was philosophy and literary studies, then, anthropology and cultural studies, and now it is social history that is being colonized by French postmodernism and poststructuralism. Indeed, the linguistic turn—or, as Bryan Palmer recently referred to it in his book of the same title, "the descent into discourse"—is even in evidence in the most *un*-Gallic of scholarly pursuits, British labor history. Although Patrick Joyce is not the first student with working-class experience to make the descent, his new book is the most deliberate undertaking of this sort.

Joyce, a lecturer in modern history at the University of Manchester, is best known for *Work, Society and Politics: The Culture of the Factory in Late Victorian England* (London: Harvester, 1980), which challenged the "labor aristocracy" thesis and attempted to explain the "*success* of employer hegemony" (p. xvi) by examining the relations of paternalism in the North of England. Here, in *Visions of the People,* he goes after the very concept of class in the Victorian and Edwardian decades. More specifically, Joyce contends that a reading of English working people's culture reveals that however much those workers and their families may have constituted a class (or cluster of classes) in a "Marxist" or sociological sense, their understandings and expressions of English/British history and the social order in which they lived were *not* those of class. To be even more precise, what he says is that while English working people may have shared "the consciousness of *a* class, it was not a consciousness *of* class" (p. 335; my italics). Rather, he claims—and this is the primary thesis of his study—their comprehension of England past and present (and possible future) was "populist."

To write English labor history means confronting E. P. Thompson's *The Making of the English Working Class* (London: Gollancz, 1963) wherein he relates how the varied strands of English radicalism and workers' "class struggles" culminated in the early 1830s in the formation of a class-conscious working class. Even those who admired and appreciated Thompson's work and thesis never fully accepted his story and criticized it. Richard Price has called it the finale of an earlier historical process going back to the 17th century; Eric Hobsbawm and Gareth Stedman Jones have each written that the 1830s would be best understood as one moment in a continuing process of class formation, deformation and reformation; and Craig Calhoun has challenged Thompson's continuing assertions of "class struggle without class" in the 18th century, proposing instead the concepts of populism and community. Joining in the deconstruction of "the making of the English working class," Joyce pushes the commencement of *class* consciousness all the way up to 1914 and the eve of the First World War.

In this lengthy work—which is occasionally a real slog to read—Joyce explores the language of popular politics, the moral discourses of labor, custom, and popular culture and the "imaginary constitution of the social order" (including broadside ballads, dialect literature, and popular theater). Focusing on industrial Lancashire but making steady reference to the larger country and, beyond, to contemporary France, he uncovers not a singular popular discourse of populism but, what he, himself, calls, a "family of populisms" (p. 329) persisting—though subject to adaptation and change—from the 1840s through to 1914. Conceptions of class are also found but only faintly compared to images of "the people."

Joyce's analysis of his topic is persuasive and he handles discourse analysis deftly. Students of English working-class life will have to attend to his arguments and conclusions regarding working-people's populism. However, as accomplished as Joyce appears to be regarding discourse theory and labor historiography he fully ignores the following recent historical and theoretical writings that seriously challenge his approach: John Saville's *1848* (Cambridge: Cambridge University Press, 1987) on class struggle, power, and state formation; James D. Young's (admittedly cranky) *Socialism and the English Working Class* (London: Harvester, 1989) on the "popular, unconscious socialism" of English workers; and the critique of discourse theory to be found in Ellen Wood's *The Retreat from Class* (London: Verso, 1986).

Having smartly abandoned economism and structuralism, historians may be jumping too readily to culturalism and poststructuralism. Finding that discourse is no more determinant of political economy than the reverse, they seem to give up the project of exploring their dialectical and intimate entanglements. As a consequence we lose the dynamic of history which, if I may be so bold here at *post-histoire* or the "end of history," has been a process of struggle, *class struggle* in fact, though not at all that alone. Our scholarly and intellectual task must be to comprehend the connections. Joyce's work is important but he fails to make the essential connections.

America at Century's End. Edited by Alan Wolfe. Berkeley and Los Angeles: University of California Press, 1991. Pp. xv+579. $29.95.

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What sort of a world do Americans live in? How do we relate to each other intimately and in small and large groups? What are the dynamics of the large-scale structures that determine the conditions of our lives, "behind our backs"? How do we adapt to these changes in social institutions and political movements? What are the emerging patterns of American life at century's end? These are the questions raised and addressed in this volume.

According to the authors in this collection, our world is great, it is big, it is complex, and it is not very beautiful. They take us on a journey through the American sociological landscape. The topics covered include: the conditions of gender relations and the family (Judith Stacey, Kathleen Gerson, and Gary Alan Fine and Jay Mechlin); attitudes toward communities (Claude S. Fischer); international politics and culture (Fred Block and Katherine S. Newman); class relations (Ruth Milkman and David Halle and Frank Romo); dilemmas of race (Bart Landry); immigration (Rubén G. Rumbaut); urbanism (Sharon Zukin); media (Michael Schudson); schools (Caroline Hodges Persell); medicine (Jonathan B. Imber); religion and politics (James Davison Hunter and Jonathan Steadman Rice); popular and fine culture (Gaye Tuchman); political culture (Rebecca Klatch and J. William Gibson, and Richard Madsen); criminal culture (Jack Katz); time (Carmen Siranni and Andrea Walsh); and future prospects (Wolfe).

These colleagues bring to their topics a number of common attributes. Most striking is that they are all of the same generation, described here as the third generation of post–World War II sociologists. Hence, they view our fragmented world from a common generational perspective. They all doubt the validity of the political and cultural consensus of the Cold War era, they know that we do not live in the best of all possible worlds, and that social upheavals and breakdowns like those of the sixties, seventies, and eighties are every bit as normal as the "normality" of the fifties, when the dream of the American century prevailed.

A troubled country is portrayed. It faces immense problems because it is a society experiencing rapid change without a clear or happy teleology. The authors understand that our world is highly interdependent and that Americans are groping to understand and act in it, with varying degrees of success. Especially noteworthy is the fact that the authors do not agree about the fundamental causes of American problems; nor are they in agreement about the way these ought to be resolved. There are no clear and definitive policy recommendations that could be drawn from this work. And, in my judgment, that is one of this collection's major strengths.

Indeed, the form of a collection of studies is a distinctly appropriate way to approach the decentered complexity of American society. Important insights are presented, and controversies opened, but closure is not attempted.

An adequate description of the subjects or confrontation of the controversies in this book is beyond my scope here. It would require a book-length manuscript. Noteworthy highlights include Jack Katz's illuminating inquiry into the passions of criminals, in which he shows how both liberal and conservative models of crime and delinquency are based on an overly rationalized material explanation of "deviant" behavior. He proposes instead a straightforward phenomenological account of the lived and pleasurable experience of crime. This, he maintains, is the only way to construct a progressive response to crime.

The search for the construction of appropriate responses to the dilemmas of gender and generational relations is the task of Stacey, Gerson, and Fine and Mechlin. They, too, want to reject the easy conventional wisdoms of conservatives and liberals. They show that, while the old gender order is in disarray, no new one is ascendant or likely to prevail. Plurality in the form of strikingly new models of family structures and child-rearing practices are everywhere to be observed, among both the privileged upper-middle class and the working class. And the socialization of children and their resistance to the adult world creatively proceeds outside the bounds of inherited conventions. Clearly, close empirical studies of zones of intimacy will be a hot sociological topic in coming decades.

Large-scale structures are also undergoing radical change. Block, Newman, and Zukin show that global interdependence, to the relative disadvantage of the American political economy, is what the new world order is all about.

Between global transformation and intimate human interaction lies political and cultural agency; people are trying to make sense and act in their world. Michael Schudson, in his astute analysis of the news and mass dissemination of information, reveals how difficult this will be. Ours is a citizenry overwhelmed by discrete pieces of information, without readily useable frameworks for making sense of them. Hunter and Rice, in their particularly illuminating contribution, show that people still turn to religion for this. But they do so in new ways, not specifically as Protestants, Catholics, or Jews, but together as conservative Protestants, Catholics, and Jews on the one hand, or progressive Protestants, Catholics, and Jews on the other. Hunter and Rice report that recent research seems to demonstrate that our political attitudes and actions are shaped more by our responses to modernity than by our specific religious heritages.

And, not surprisingly, we cannot seem to speak to each other substantively, according to Richard Madsen. Our political discourse is contentless. We want to find a common good, but we cannot. We talk past each other, not to or with each other.

Alan Wolfe, in introducing, editing, and summing up *America at Century's End,* seems to want to overcome this condition. He has highlighted the problems we face and shown how sociologists and their objects of inquiry wish to confront the dilemmas of our world, not to succumb to them, but to address them. He argues that our profession ought to dedicate itself to reconstruction, not to an easy postmodern deconstruction. Institutional analysis, he maintains, is the key to reform.

There is much to recommend in this institutional approach, and these representatives of the "third generation of post-war sociologists" make this approach quite persuasive. Yet, I think there is something missing in this volume—careful but committed normative inquiry. To depict America at century's end adequately, we must ask systematically not only what our society is like in its institutional complexity, but also what we would like our society and its institutions to be. In his book *Whose Keeper?* Wolfe suggests that sociology can provide a moral philosophy, enervated elsewhere in our society and culture. Oddly, the authors of this book do not frontally and critically raise the issues of what institutions should be. They imply or assume political and moral commitments, but these commitments are not examined. If we sociologists are to contribute to critical public debate, which is, in my judgment, a commendable project of this volume, we must pragmatically apply our sociological imaginations to the examination of moral values and political principles.

Perestroika Era Politics: The New Soviet Legislature and Gorbachev's Political Reforms. Edited by Robert T. Huber and Donald R. Kelley. Armonk, N.Y.: M. E. Sharpe, 1991. Pp. 238. $49.95 (cloth); $15.95 (paper).

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As is true of most scholarly work on Soviet politics these days, this collection is rather slow off the mark, rendered somewhat anachronistic and even irrelevant by the fact that the units of analysis—in this case the "new" Soviet legislature and the political reforms of Mikhail Gorbachev—have simply ceased to exist. Nonetheless, it would be unfair to simply dismiss the volume purely on the grounds that its subject matter no longer exists. The era of perestroika represents the most remarkably transformative period of Soviet history, and this period is relevant to all social scientists who are interested in the process of social and political change. The cunning of history may have finally caught up to the Soviet Union, but this does not mean that accounts of what were "current events" not too long ago cannot be interpreted in new and exciting ways to explore important theoretical questions. What, then, does the present volume offer us?

The volume consists of essays on constitutional and electoral reform, the factionalization of politics, an analysis of right-wing challenges to democracy, factionalism in left-wing parties, legislative-executive relations, the process of parliamentary government, and defense and foreign policies. As descriptive cartographies of specific parameters of Soviet political reforms, the essays are capable and coherent and offer the reader a fairly detailed examination of some important dimensions of perestroika era politics. The major problem with the volume is that the essays are primarily descriptive and do not speak specifically to the theoretical question that is offered in the introduction as the centerpiece of the volume: What is the role of ideas in social change? The title of editor Robert T. Huber's introduction to the volume, "The New Soviet Legislature: How Ideas and Institutions Matter," is likely to catch the eye of the sociologist, for the problem of the relation of ideas—or culture more generally—to social change is a central one in sociology. One expects that there might be some specific discussion of the vehement debates on the role of culture versus structure in the process of social change. One is also led to believe that the volume's essays will fit the particulars of the Soviet experience of political reform into some larger theoretical framework on the relationship between culture and social change. Neither expectation is fulfilled. Indeed, sociologists, especially theoretical ones, are likely to be disappointed with the volume. The reader must struggle to understand why the editor touts the book as a theoretical exploration of the role of ideas in the process of political change. One wonders, for instance, why Huber begins the volume with the argument that "ideas have played an important *causal* role in certain political outcomes, or at least helped make possible a range of outcomes" (p. 5) and then ends the volume with an essay (his own) on changes in defense policy and foreign policy that has little to do with this theoretical argument. Similarly, most of the other essays do not sufficiently relate the particulars of the perestroika period to the important theoretical work on the impact of ideas on the policymaking process or on political institutions.

The lack of theoretical and methodological sophistication in this volume is not surprising, given the close relationship that existed between Soviet analysts and Washington policymakers throughout the Cold War era. In studies of Soviet politics, theoretical and methodological rigor were often sacrificed in favor of more general understandings that were immediately accessible and pragmatically useful to policymakers. Although it aspires to offer answers to an important theoretical question, this volume ultimately represents the continuation of a genre of "advice literature" written by political scientists for the "princes" of the American national security state. This is not to say that policy-oriented political science is a misguided intellectual endeavor, but rather that, over the course of the history of thinking about the Soviet Union, such analyses have enjoyed an ontological hegemony over more theoretically informed work. This hegemony might be justified if political scientists had predicted either the occurrence of *glasnost* or the eventual dissolution of the Soviet empire. In fact, however, political scientists (with few notable exceptions) failed to predict either of these phenomena and were generally resistant to the idea that change could even occur within the Soviet Union. Recognizing this, one wonders whether they should have an audience when they embark on the task of theorizing about contemporary changes.

The Closing Door: Conservative Policy and Black Opportunity. By Gary Orfield and Carole Ashkinaze. Chicago: University of Chicago Press, 1991. Pp. xx+254. $22.50.

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Atlanta has been touted in both the scholarly and popular press as the symbol of black progress in the 1970s and the 1980s. Triggered by a favorable business climate, created and orchestrated by the city's black political leadership, the Atlanta region experienced unprecedented economic and black population growth during this period. In *The Closing Door,* Orfield and Ashkinaze argue, however, that even under such favorable economic and political conditions, the quality of life for black Atlantans did not improve significantly during the 1970s and 1980s. More specifically, they contend "not only that full equality of opportunity has eluded Atlanta, but that some of the gains made during the civil rights era are eroding rapidly" (p. 2). In eight chapters, which include 29 tables and 11 figures, Orfield and Ashkinaze document the nature and extent of racial inequality, explain why this state of affairs continues to exist, and offer specific policy recommendations to reopen the door of black opportunity in metropolitan Atlanta.

Following a foreword by Andrew Young, former mayor of Atlanta, chapter 1 sets the context for the analyses that follow. The authors' central thesis is that Atlanta's failure to achieve racial equality is related not to the liberal social policies of the 1960s, as conservative policy analysts like to argue, but rather to the conservative policies implemented by the Republican leadership that has controlled the White House for nearly all of the past two decades (except for the Carter years). As background to the "numbing trends and numbers found elsewhere in the book" (p. 29), in chapter 2, the authors profile the real-life experiences of seven individuals and families who, by virtue of their personal misfortunes and their geographical isolation and economic marginalization from mainstream opportunities, have been bypassed in the post-1970 economic boom in Atlanta.

Chapter 3 describes "the changing metropolitan area and discusses the deepening racial inequalities that have accompanied the region's unprecedented prosperity and the historic breakthroughs of black political leadership" (p. 46). The authors' findings challenge the assumption, widely held by conservative policy analysts, that "what is good for the overall economy is good for blacks" (p. 49). Economic growth is shown to have been unevenly and inequitably distributed in metropolitan Atlanta. Blacks and the neighborhoods they inhabit reaped few, if any, of the benefits.

In chapter 4, Orfield and Ashkinaze demonstrate how residential segregation continues to be a fundamental barrier to racial equality in Atlanta. Poor blacks became increasingly concentrated in deteriorating inner-city neighborhoods, and upwardly-mobile blacks moved primarily to the suburbs south of the city. Moreover, the suburbs to which blacks moved were devoid of any significant economic growth. As opposed to being caused by black preference for predominantly black neighborhoods, the resulting highly segregated housing pattern is attributed to government-supported white resistance to black entry into the predominantly white, economically prosperous northern suburbs of the Atlanta region.

Chapter 5 focuses on Atlanta-area high schools, the "last stage of universal public education in the United States and the key to almost all decent jobs in the American economy" (p. 103). After chronicling the historical sequence of events that resulted in Atlanta city high schools' being more segregated in 1980 than they were in 1970, attention shifts to the impact of school reform policies on black educational attainment in metropolitan Atlanta. The increasing reliance on standardized tests as the ultimate arbiter of educational success, and other conservative education policies, are shown to have actually heightened school failure among black youth, especially black males.

Chapter 6 focuses on the decline of college enrollment among black high school graduates in Atlanta, with particular emphasis on black males. During the 1970s, the authors state, "a bachelor's degree seemed within the reach of every high school graduate in metro Atlanta, but by the 1980s, there were sharply rising numbers of black high school graduates with no realistic hope of obtaining one" (p. 150).

Their data show that fewer black high school graduates were attending college in 1986 than in the late 1970s whereas "white access to higher education increased" (p. 150) during this period. Orfield and Ashkinaze attribute this disparity to, among other conservative policies, massive cuts in federal programs offering full-time financial aid to low-income college students and to state policies that tightened admission standards, eroded minority recruitment efforts, and limited remedial programs for disadvantaged students.

Chapter 7 addresses the question of how well the Job Training and Partnership Act (JTPA) has worked for Atlanta blacks. Two significant findings emerge: that the JTPA program operated through four local service delivery areas (SDAs) in the Atlanta labor market, which separated the concentration of poor people from the concentration of job growth, and that it was easier for local officials to meet the program goals established by the Reagan administration by training mostly minority women and placing them in traditionally black occupations. As a consequence, hard-core disadvantaged black males—those who had dropped out of high school and thus had no marketable skills—made up the smallest proportion of JTPA program participants. Orfield and Ashkinaze conclude this chapter by stating that, "Even in a city run by black men, the JTPA program failed to effectively reach jobless black men" (p. 185).

In Chapter 8, the authors summarize their findings pertaining to the damage imposed on blacks by policies adopted during nearly two decades of conservative political domination. They then outline what must be done to overcome the widening racial divide in metropolitan Atlanta.

The Closing Door is an extremely important and significant book. Numerous scholars have attempted to demonstrate the devastating impact of the Reagan revolution on blacks, but none has done so with the level of clarity and the degree of specificity evident in this book which illustrates, convincingly, how "both civil rights policies aimed at ameliorating ghetto conditions and building bridges of opportunity to mainstream society were reversed suddenly in 1981" and explains masterfully why the conservative policy prescriptions (as well as some of those advocated by progressive policy analysts) have not and will not work for blacks, especially those locked in inner-city ghetto communities. It also advocates the implementation of a set of realistic policies to nurture and facilitate this nation's elusive quest for racial equality. I highly recommend *The Closing Door* to scholars interested in the politics of race, class, and poverty in urban America.

Who Is Black? One Nation's Definition. By F. James Davis. University Park: Pennsylvania State University Press. Pp. xi+204. $25.00 (cloth); $12.50 (paper).

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This is a very well written book that communicates complex ideas with clarity and interest. It is rare, in my experience, for an academic book written by a social scientist to be as interesting and exciting as a piece of fiction. This book is hard to put down because Davis's story of how the

United States as a nation came to define who is black reads like a mystery novel in which every historical event provides one more clue to the final murder of a people.

In chapter 1, "The Nation's Rule," Davis sets the stage through various anecdotes from literature and contemporary events. He underscores the subjective and therefore arbitrary nature of this country's definition of "black." In the popular mind, *phenotype* (a person's physical appearance) determines who is black and should be easy enough to assess. In actuality, however, phenotype variation within the black population makes it difficult to determine who is black. Historically, our nation has resolved this dilemma by relying on the "one-drop" rule—"meaning that a single drop of 'black blood' makes a person black" (p. 5). This rule becomes the focus for much of Davis's discussion and analysis.

In chapter 2, "Miscegenation and Beliefs," Davis reviews the laws and beliefs about the mixing of the "races." He shows that opposition to miscegenation resulted from the fear that the system of Jim Crow, through which the one-drop rule was enforced, would fall and so would all the economic and social advantages whites derived from its enforcement.

In chapter 3, "Conflicting Rules," Davis shows that, in effect, two and one-half centuries of miscegenation and numerous challenges to the one-drop rule preceded its uniform national acceptance during the 1920s. Chapter 4, "The Rule Becomes Firm," reviews how the one-drop rule became law in the United States and the accepted social custom during this century. The one-drop rule has become part of our common sense, so much so that it is hard to conceive of alternative definitions—the topic of chapter 5, "Other Places, Other Definitions." In chapter 6, "Black Acceptance of the Rule," Davis surmises that acceptance of the one-drop rule by blacks is largely the result of the strong sanctions for violating it. Therefore, in the opinion of many blacks, it would be irresponsible not to teach their children how to cope with the nature and consequences of this rule (no matter how much—or how little—"black blood" the children have). Congruently, whites' acceptance of the rule perpetuates the status quo that benefits whites over other ethnic or racial groups. Thus, the rule is strongly embraced from within the black community and strongly enforced from without by the white power structure. The rule creates a hierarchy with whites at the top and blacks at the bottom. But other ethnic and racial groups have this incentive to maintain the hierarchy as well: at least they are not at the bottom.

In chapter 7, "Ambiguities, Strains, Conflicts, and Traumas," Davis uses case studies to show how phenotypical variations have made consistent enforcement of the one-drop rule impossible. This inconsistency has added another layer of psychic pain to the economic and social disadvantages that racism has imposed on blacks. Chapter 8, "Issues and Prospects," briefly explores whether this country's problems with the one-drop rule can be solved or at least ameliorated.

Examination of the one-drop rule is a fascinating way to study intergroup relations in the United States, especially the way in which the rule is used to enforce power disparity between groups. Davis's original analysis and integration of previous work on the subject, lay the foundation for important theoretical developments. However, although Davis clearly states in his beginning chapter that "The analysis in this book . . . has been oriented primarily toward an approach that emphasizes conflict and differences in group power. . . ." (p. 16), he really does not accomplish this goal. This is a great historical and social study, but it lacks a power analysis—especially in the discussion of individual adaptations. Here, Davis seems to suggest that acceptance or rejection of the one-drop rule by individual blacks is based on individual choice rather than on their various experiences with the "power structure" (e.g., discrimination or enhanced opportunity because of their phenotype). Yet, as Richard Wright points out in his introduction to *Native Son,* although there are many different types of Biggers (those who fight back, those who rebel, those who succumb, and so on), how and why each individual takes different adaptations to subordination because of skin color can only be answered through the study of power, not by the outcome of the adaptation.

However important the question of power is, and no matter how little Davis grapples with it, his book does lay the foundation for future researchers to study the function of the one-drop rule in enforcing and maintaining existing power arrangements. I would not only recommend reading *Who Is Black* for this purpose but also to find the answer to the question, Who is white?—and, most important, why we as a society should care about the reply.

The Hollow Hope: Can Courts Bring About Social Change? By Gerald N. Rosenberg. Chicago: University of Chicago Press, 1991. Pp. xii+425. $29.95.

Paul Burstein
University of Washington

Since the 1970s, sociologists have believed that social movements can bring about significant social change and that movement leaders' choices of tactics affect the likelihood of success. If we did not believe that movements cause social change, in fact, it makes little sense to have studied them as intensely as we have in recent years.

Yet our beliefs about movement impact rest on a shaky foundation. Doug McAdam, John McCarthy, and Mayer Zald devote less than a page and a half to movement consequences in their 42-page review of "Social Movements" in the *Handbook of Sociology* (edited by Neil Smelser [Newbury Park, Calif.: Sage, 1988]), and cite William Gamson's *Strategy of Social Protest* (Homewood, Ill.: Dorsey), published in 1975, as still the most systematic attempt to analyze movement outcomes systematically. We know remarkably little—far less than we normally assume—about the impact of social movements on social change.

In this context, Gerald Rosenberg's *The Hollow Hope* should worry sociologists interested in social movements and everyone concerned about the issues he analyzes: civil rights, abortion and women's rights, the environment, criminal law, and legislative reapportionment. Rosenberg claims that a favorite tactic of 20th-century American social movements—going to court—almost never leads to significant social reform. Instead, he argues, the litigation tactics developed by the civil rights movement and adopted by many others are generally ineffectual; furthermore, by absorbing resources that might have been used in other ways, going to court may reduce the likelihood that social movement organizations (SMOs) will attain their goals.

Rosenberg shows, quite convincingly, that many SMO leaders have felt very strongly that legal action could produce social change and so devoted much of their resources to litigation. He points out, though, that there have long been two competing views of how effective courts are likely to be as agents of social change: what he calls the "dynamic court" view, whose proponents argue that American courts are uniquely situated to bring about social change when other branches of government are unable or unwilling to do so and the "constrained court" view, whose proponents argue that courts are powerless.

Both views seem too simple to him, so he refines previous arguments and develops his own. He hypothesizes that courts usually lack the autonomy, organization, and resources needed to produce significant social reform. But, arguably, they may bring about reform if certain conditions are met: if other actors provide benefits or impose costs to induce compliance with court orders; if the market provides incentives to obey judicial decisions; and if judicial decisions provide leverage or legitimation to individuals who have wanted to bring about change but have been unable to do so.

Rosenberg then tests his hypotheses by gauging the impact of Supreme Court decisions on social change. To be fair to those who favor the dynamic court view, he focuses most strongly on civil rights, because that is the area where judicial influence is most widely believed to be substantial. In addition, he considers not only the direct effect of judicial decisions (e.g., the impact of *Brown v. Board of Education* on school desegregation), but also how they might lead to social change indirectly, by affecting media coverage of issues, public opinion, treatment of the relevant subjects in textbooks, elite opinion, social protest activities, growth of SMOs, sponsorship of legislation, and so on.

Drawing on a tremendously wide range of evidence (Rosenberg utilizes data gathered by social movements researchers and collects a good deal of his own), Rosenberg concludes that judicial decisions very seldom had a significant impact on the civil rights of blacks. He reaches similar conclusions regarding the rights of women, and more tentatively (because his treatment of the issues is briefer), decisions in the other areas examined.

Sometimes judicial decisions do appear to have an impact. When they do, Rosenberg claims, it is because the conditions he proposes have been met. Schools were desegregated in the South, for example, not because courts ordered it, but because federal legislation (adopted long after the first court desegregation orders) provided strong financial incentives to do so, and southern business leaders realized that civil peace and good schools were essential for attracting investments from the North.

Rosenberg concludes that "At best [the courts] can second the social reform acts of the other branches of government. . . . A court's contribution . . . is akin to officially recognizing the evolving state of affairs, more like cutting of the ribbon on a new project than its construction" (p. 338). The courts can play a role in social change, but it is a modest one indeed.

Rosenberg's arguments are excellent, the range of his scholarship imposing, and the weight of his evidence impressive. I see two noteworthy problems in the book. First, he is not as clear as he should be about what it is reasonable to expect from the courts or any other potential agent of social change. How much change must be made to consider an attempt to produce social change a success? Second, by presenting a few case studies rather than a larger, random sample of cases (along the lines proposed by Gamson), he gives us little indication of how often the conditions favorable to judicial impact will be present; thus, his conclusion that the courts "*almost never*" (p. 338) effect significant social reform may be an overstatement.

Sociologists have been aware of these problems for a long time; Gamson discusses them in detail. But we avoid confronting them, devoting our efforts to studies of movement organization and participation rather than consequences. By arguing convincingly that what we take for granted—SMOs can succeed, litigation can be an effective tactic—may not be true, Rosenberg may force us to think more carefully about the relationship between political action and social change. I am not certain that Rosenberg's bold claims will stand the test of time; but they deserve to be taken seriously by all sociologists interested in social movements and social change.

Metatheorizing in Sociology. By George Ritzer. Lexington, Mass.: Lexington Books, 1991. Pp. xi+362. $49.95.

Kurt W. Back
Duke University

Metatheorizing in Sociology tries to accomplish several things at once: it tries to explain and justify metatheory as a field of scholarly activity; it presents a classification of metatheoretical approaches and shows examples of each use; it presents the author's own favorite model and assesses the work of others according to it; and finally, it uses these ideas to sketch the work in sociological theory in the last decades and gives projections and advice for the future.

The volume starts with an introductory section that first defines and defends metatheory. Metatheory is defined as the investigation and analysis of theories. George Ritzer considers it as a neglected topic, even more so because many people do metatheory without acknowledging that they are doing it and indeed many of them such as Theda Skocpol and Randall Collins, while attacking it. Given this position on metatheory, Ritzer devotes considerable effort to defending its legitimacy. He seems to be unduly defensive in spots. After all, sociological theory is an important aspect of society and therefore deserves the attention of sociologists, if for no other reason.

The next three chapters of the introductory section describe three uses of metatheoretical research. One is the analysis of theories for the better understanding of sociological theory in general. The second is an overview of theories as a preparatory task to an author's own work; an extreme routinized example is the obligatory "review of the literature" in dissertations. The third is the use of metatheory to develop a new overarching theory. An extreme case, which Ritzer excludes from the limits of metatheory, is the polemical use of metatheory for showing the deficiencies of other theories in order to highlight an author's own achievement. Clearly, the progression here goes from dispassionate analysis to a tendentious polemic. The latter is an ever-present temptation, because few scholars do not have their own axes to grind, and no one can be completely objective. Ritzer himself is not quite able to escape this temptation.

The main section of the book (nine chapters) is an ingenious undertaking wherein Ritzer uses the metatheoretical approach to trace the history of sociological theory. Here, he leans heavily on Kuhn's model, extracting paradigms and tracing their fate, but he does not follow him slavishly. Kuhn's ideas help in the organization of this intricate development: the early history, leading to acceptance of a few dominant paradigms by 1960; the split into many competing paradigms in the 1960s; the dominance of microsociology in the 1970s, and rapprochement between micro- and macrosociology in the 1980s. The tools of metatheory, as described in the introductory chapters, are used to good effect and lead to novel insights into the similarities and differences of various sociological theories. This presentation shows the value of metatheory, skillfully done, and gives a new perspective on the recent arguments between theoreticians.

This section is, however, more than a dispassionate discussion. Ritzer proposes his own paradigm, the integrated sociological paradigm, and he starts to judge other peoples' efforts according to this standard. Although he tries to be fair in his discussion of theories, he comes dangerously close to the approach that he has excluded from metatheory, the use of analysis of theories to highlight his own.

This tendency becomes stronger in the book's concluding section on the current status and future directions of metatheorizing, which tends more and more toward evaluation and prescription. Not to prejudge Ritzer's paradigm, but it could be wished that this work had carried out its original promise, namely, an exposition of metatheory as the science of sociological theories.

In general, the book presents the virtues and limitations of metatheory as a closed system. It takes the theories as given objects of study, classifies, and compares them. It does not go beyond the texts themselves: it neither considers the input into the system, such as influences of social or historical conditions, work in other sciences and in philosophy, or the characteristics of the theorists themselves; nor does it look at the output, namely, such questions as the productivity of the theories for research or practice, or their impact on the society. This selectivity has its advantages: the important features of each theory can be highlighted and the different theories can be discussed on their own merits. This can be an important resource for students or a reference for professionals, especially if it is executed as well as here.

But this choice leads also to limitations: theories are only considered in their own right and, to an outsider, the reasons for interest in these questions are often problematic. The appeal here is from theorists to other theorists, which means that it caters to a select audience, and that the questions posed appear esoteric.

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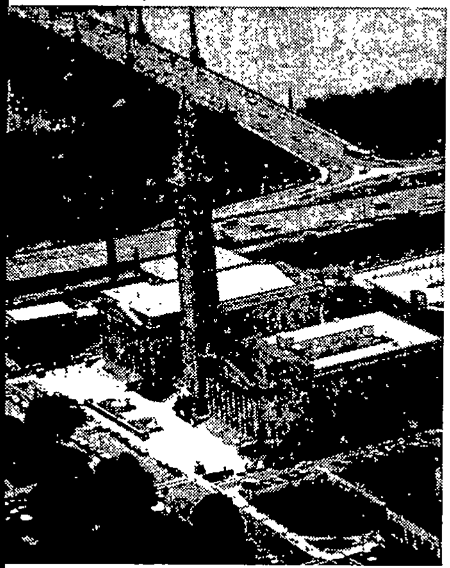
N. J. Demerath III and Rhys H. Williams



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(Rev. 7/92)

Critical Theory and the Pragmatist Challenge[1]

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Habermas's theory breaks with the Continental tradition that has denigrated pragmatism as an Anglo-Saxon philosophy subservient to technocratic capitalism. While Habermas deftly uses pragmatist insights into communicative rationality and democratic ethos, he shows little sensitivity to other facets of pragmatism. This article argues that incorporating the pragmatist perspective on experience and indeterminacy brings a corrective to the emancipatory agenda championed by critical theorists. The pragmatist alternative to the theory of communicative action is presented, with the discussion centering around the following themes: disembodied reason versus embodied reasonableness, determinate being versus indeterminate reality, discursive truth versus pragmatic certainty, rational consensus versus reasonable dissent, transcendental democracy versus democratic transcendence, and rational society versus sane community.

For much of the 20th century, pragmatism was perceived in Europe as a crude expression of Anglo-Saxon utilitarianism. Even thinkers sympathetic to the new American current found it inferior to the Continental philosophical tradition. Less charitable critics, such as the writers close to the critical theory circle, dismissed pragmatism as instrumental reason run amok, a technocratic decisionism severed from substantive-rational moorings. It was not until the 1960s that respectable European thinkers began to pay more favorable attention to pragmatism and its sociological counterpart, symbolic interactionism. A notable example is Jürgen Ha-

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[1] This is a revision of a paper presented at the 85th annual meeting of the American Sociological Association. I wish to thank participants in the Sociology Department Seminar at Boston University for helpful comments on an earlier draft of this article. The critical feedback from Mitchell Aboulafia, Thomas Alexander, Bob Antonio, Thomas Burger, Lewis Coser, Bruce Mazlish, Gene Rochberg-Halton, and Lon Shelby is also gratefully appreciated. Finally, I wish to thank the three *AJS* reviewers for their comments and suggestions. Correspondence may be directed to Dmitri Shalin, Department of Sociology, University of Nevada, 4505 Maryland Parkway, Las Vegas, Nevada 89154.

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bermas, who recently admitted, "I have for a long time identified myself with that radical democratic mentality which is present in the best American traditions and articulated in American pragmatism" (Habermas 1985, p. 198). This statement is noteworthy not only because it holds fresh promise for a transatlantic dialogue, but also because it points to critical thinkers' renewed interest in liberal democracy and its emancipatory potential.

While the search for common ground will be welcomed on this side of the Atlantic, it will also raise some eyebrows. There are many points on which critical theorists and writers steeped in pragmatism appear to part company. The former have a penchant for totalities, are conversant with rationality at large, and have profound reservations about bourgeois democracy, whereas the latter attend to the particular, revel in multiple rationalities, and place much stock in democratic institutions. So, when Habermas (1986, p. 193) describes pragmatism as "a missing branch of Young Hegelianism," he is sure to make some critics wonder if his European biases blinded him to pragmatism's native roots.

I see nothing objectionable in the efforts to trace pragmatism's European lineage. Nor do I agree with those who think Habermas has gotten pragmatism all wrong. A movement as diverse as this lends itself to more than one reading, and Habermas does an important service by illuminating its various facets—most notably its political dimension—which American sociologists claiming the pragmatist legacy tend to ignore. Still, I want to take issue with Habermas because something is amiss in his analysis—the pragmatist sensitivity to indeterminacy, contingency, and chaos. This sensitivity is remarkably in tune with trends in modern science, and it deserves far closer attention from sociologists than it has been granted so far. It is my contention that taking objective indeterminacy seriously would require rethinking central conclusions in Habermas's theory of communicative action. In particular, I would like to show that Habermas elevated verbal intellect at the expense of noncognitive intelligence and thereby truncated the pragmatist notion of experience. I will also argue that incorporating the pragmatist perspective on democracy brings an important corrective to the emancipatory agenda championed by critical theorists.

Critical theory and Habermas have received a fair amount of attention (Jay 1973; Rose 1978; McCarthy 1978; Held 1980; Geuss 1981; Kellner 1985; Thompson and Held 1982; Antonio 1983; Bernstein and Forester 1985; Ferrara 1985; Benhabib 1987; Wolin 1987). Aside from Antonio (1989), however, few authors explored in depth the interfaces between pragmatism, democracy, and Habermas's thought, and none, to my knowledge, incorporated the pragmatist perspective on experience and explored its implications for Habermas's views. My discussion centers on the theory of communicative actions—a segment in Habermas's total corpus in which he joins issues with pragmatism and makes a concerted effort to incorporate its democratic ethos into the quintessentially European project of critical theory. I begin with the sources of critical theorists' ambivalence toward democracy. Next, I examine how Habermas merged the pragmatist and critical theory traditions. And finally, I subject his construction to criticism, using the pragmatist notions of experience, indeterminacy, and democracy as analytical tools.

FROM CRITICAL IDEALISM TO CRITICAL THEORY

It was not until Kant ([1781] 1966, p. xxiv) declared, "Our age is, in every sense of the word, the age of criticism, and everything must submit to it," that the term "critical" entered the philosophical lexicon in its modern sense. Kant chose his nomenclature deliberately to highlight the difference between the age of reason and the age of criticism, between overconfident rationalism of *philosophes* and "my transcendental or, better, *critical* idealism" (Kant [1783] 1950, p. 41). According to Kant, reason could no longer derive its mandate from divine inspiration or natural law but must lay its own standards for judging the true, the good, and the beautiful. For reason is not an outside observer impartially stating the truth and legislating a better future but a participant-observer whose rational activity gives the world its meaning and whose very unreflexivity breeds oppression. The objective structures one finds in the world, physical or social, are grounded in the a priori structures of the mind itself. To change the former, the subject has to grasp the latter. In other words, emancipation starts with self-reflection; only after reason has exposed its own prejudices and learned its own limits can it proceed with its appointed task. Hence, the endless exhortations by Kant's successors to do away with "the dogmatic tendency in man" (Fichte [1794] 1970, p. 161) and "dogmatism as a way of thinking" (Hegel [1807] 1967, p. 99) and get on with "a strenuous reacquisition of everything which has once been acquired" (Schelling [1800] 1978, p. 1).

These utterings sound vaguely subversive, but in the postrevolutionary climate of early 19th-century Europe they had a distinctly conservative ring to them. Anxious to avoid the bloody excesses of the French Revolution, critical idealists hastened to assure the world that the project of modernity they inherited from the Enlightenment would be carried out by peaceful means. The only force they were willing to tolerate was the force of reason itself—reason firmly grounded in principles, conscious of its moral moorings, and committed to the public good. This is what post-Kantian idealists called *Vernunft* and what they juxtaposed to *Verstand,* or everyday understanding, that, unbeknownst to itself, weaves the familiar world from its biases, preconceptions, and particularistic interests. Viewed from this angle, the battle for emancipation is but "the battle of reason . . . to break the rigidity to which understanding has reduced everything" (Hegel [1817] 1975, p. 53).

Vernunft is bound to strike some readers as an oversoul or a superhuman agency, but there is nothing especially mysterious about it. While the telos of reason is humanity as a whole, its locus operandi is the individual who speaks on behalf of reason. Society is fully rational when its members heed the claims of reason they have raised, when they act their conscience and submit to a tribunal within which one is simultaneously a defendant and a judge: "The consciousness of an *inner tribunal* in man . . . is *conscience*. . . . This original intellectual and . . . moral capacity, called *conscience*, has this peculiarity, that although its business is a business of a man with himself, he is obliged by his reason to look upon it as carried on at the command *of another person*. For the transaction is here the conduct of a *law-case* . . . before a judge" (Kant [1803] 1904, p. 289).

The spirit of this statement is remarkably modern and democratic; it implies that every individual, regardless of origin or status, is a rational being and a potential agent of emancipation whose dormant capacity for criticism can be roused by the critical idealists' path-breaking intellections. Emancipation through reason transpires here as a project that humans qua rational beings accomplish by subjecting to critical analysis the a priori grounds for their conduct, freeing themselves from prejudices, and unswervingly following standards they have justified to themselves as universal, equitable, and humane.

The project of emancipation through reason came under attack during the reaction that followed the French Revolution, but its bourgeois democratic ethos continued to nourish the moral imagination well into the 19th century. This ethos was still palpable in the young Marx, who called for *"a ruthless criticism of everything existing"* ([1843] 1972, p. 8) and urged the "reform of consciousness [which] consists solely in letting the world perceive its own consciousness by awakening it from dreaming about itself, in explaining to it its own actions" ([1843] 1971, p. 82). Marx's commitment to emancipation through criticism, however, wore thin in the revolutionary climate of the time. By the mid-1840s, he began to doubt the peaceable route to emancipation and, along with other young Hegelians, set out to investigate what keeps reason from exercising its curative powers. The main impediment, Marx concluded, was class domination and the institutions, such as law, morality, and philosophy, through which the capitalist state obfuscates its oppression and perpetuates false consciousness among the toiling masses. The ruling class has the power to protect its particularistic interests, and it is naive to believe that it would bow to the voice of universal reason and agree to yield its power peacefully. Bourgeois democracy is a sham; its much-touted freedoms stand in the path of emancipation, insofar as they legitimize exploitation and prevent workers from understanding their role as a driving force in history. The real hope for emancipation lies with the concrete historical agent, the proletariat, a universal class for which criticism is not just a theoretical endeavor but a practical revolutionary deed. It is this class that can bring about communism—a society based on genuinely free discourse. Such a society frees consciousness from systematic ideological distortions, brings every rational individual into critical discourse, and thus for the first time makes reason truly universal and society fully rational.

The dilemma Marx bequeathed to his successors—Must reason rely on democratic procedures or class violence to achieve its emancipatory objectives?—informs many debates about critical theory in the 20th century. Few participants in these debates failed to acknowledge that "the critical theory is the heir of . . . German idealism" (Horkheimer [1937] 1976, p. 223). All agreed that critical theory aims at "the transformation of society [that] eliminates the original relationship between substructure and superstructure" (Marcuse [1937] 1968, p. 144; see also Marcuse 1960) and has as its ultimate goal "a society in which the 'people' have become autonomous individuals [freely] choosing their government and determining their life" (Adorno 1965, p. 105). How exactly these goals were to be accomplished, however, remained a contentious issue. The fact that bourgeois democracy had failed to forestall fascism in Europe seriously undermined the trust in liberalism's emancipatory potential. The disillusionment ran especially deep among the writers gathered around Horkheimer and the Frankfurt school, whose members sought to forge a conceptual link between totalitarianism and liberal rationalism. The impotence of bourgeois democracy is transparent in its surrender to totalitarianism, the kind that the Third Reich exemplifies most vividly. Indeed, "we can say that liberalism 'produces' the total authoritarian state out of itself, as its own consummation at a more advanced stage of development" (Marcuse 1968*a*, p. 19). "The pattern of all administration and 'personnel policy,'" according to Adorno ([1951] 1978, p. 131), "tends of its own accord . . . towards Fascism." Horkheimer (1978, p. 219) concurred with this diagnosis, charging that, left to its own devices, "democracy leads to its opposite—tyranny."

Critical theorists were aware that the United States did not fit neatly into this scenario, yet they convinced themselves that America was rapidly moving toward the "administered state," whose more subtle forms of domination bore equally ill tidings. The media-based domination they found in capitalist America looked every bit as pervasive, even if somewhat more benign, as the one achieved in a totalitarian state. The cultural industry of capitalism works over time to produce mass consciousness suitable for the market economy and amenable to social control. Marcuse's (1964) *One-Dimensional Man* is the best-known account of the bondage in which reason finds itself in a capitalist society, although the basic insights articulated in this book had been familiar to critical theorists for decades (Horkheimer and Adorno [1944] 1989, p. 222).

While critical thinkers had few reasons to cheer European liberalism, they could not find much solace in the Marxist scenario either. For one thing, the proletarian masses failed to reveal themselves as the agents of historical emancipation Marx hailed them to be; rather, they displayed unmistakably conservative leanings and then precisely in the countries where "late capitalism" seemed to have reached its final stage. As the century unfolded, critical theorists also became painfully aware that the states claiming Marx's legacy had evolved their own totalitarianism, one that was equally inimical to critical theory's lofty ideals. Already in the 1920s, critical thinkers questioned Marx's thesis about "the universal class" and spurned Lukac's apology for communist party domination. After World War II, their disaffection for Marxist states and Left totalitarianism grew stronger (Neumann 1953, pp. 15–19; Marcuse 1958; Adorno [1966] 1973, p. 367; Horkheimer 1978, p. 230). Horkheimer expressed this indignation with particular force, sparing neither "the tendency toward fascism in capitalist states" nor "a sudden turn of left-radical opposition into terrorist totalitarianism" (1978, pp. 230, 233).

Marcuse was perhaps the only member of the original Frankfurt school willing to sanctify violence, to say that it was "a 'natural right' of resistance for oppressed and overpowered minorities to use extralegal means" (1965, p. 116). But his views did not sit well with the old-generation critical thinkers, who refused to endorse left-wing terrorism and student militancy. This refusal precipitated the split within the New Left, with the younger generation opting for radical action and the older one left wallowing in doubt about critical theory's practical import. "There certainly can be no true criticism without an intellectually grounded hope which derives its legitimacy from realistic possibilities," urged Horkheimer (1978, p. 138). Yet with the liberal path toward emancipation blocked by the market-driven media and with class warfare discarded as a viable alternative, it was precisely "an intellectually grounded hope" that critical thinkers found in short supply. After the Second World War in particular it became obvious to many observers that "Critical Theory was now incapable of suggesting critical *praxis*" (Jay 1973, p. 279). It was in this climate of uncertainty about the prospects for emancipation that critical theorists proclaimed the eclipse of reason and embraced Weber's prophesies about rationalization's crippling effect on democracy.

What attracted Frankfurt school theorists to Weber was his unromantic view of reason as an agency whose power to control the world subverts human longing for meaningful life. This ironic capacity to render the world manageable and meaningless at the same time has been a central theme in emancipatory scholarship from the start. Already in his prize-winning *Discourse on the Sciences and the Arts,* Rousseau articulated the paradox of industry begetting poverty and culture breeding oppression. Critical idealists developed it further in their metaphysics of reason that remains estranged from itself and its products until it realizes its own responsibility for the world out there. Marx's theory, which blends the French Enlightenment and German idealism, offered another variation on this theme: history is the ongoing struggle of humanity to free itself from the dehumanizing consequences of its relentless drive to perfect the production forces—the drive that multiplies goods and miseries alike. These insights, minus the attendant optimism about reason's ultimate triumph, found their way into Weber's theory of global rationalization.

Reason's power to assert control, to increase efficiency, to calculate the future—to achieve any proximate goal—is designated by Weber as "instrumental" or "formal rationality." The capacity to judge value, to realize a higher purpose, to pursue a just cause—to lead a meaningful life—is termed "value" or "substantive rationality" (Weber 1964, pp. 184–86, 211–12). The relationship between the two is antinomian: the greater mastery reason achieves over the world of things and events, the less room is left for the questions of meaning and value; the more organized reason becomes internally, the narrower the scope for personal choice; the farther the state extends its bureaucratic procedures, the heavier its domination over the individual. A telling example is representative democracy, which purports to express the people's will but in fact subverts its professed goal by virtue of its complexity, its pervasive legalism, and its growing dependence on party leaders, who inexorably come to dominate politics. Democracy, Weber (1964, pp. 407–23) concluded, is the most efficient form of domination, all the more pernicious that it conceals its totalitarian proclivities under the veneer of bureaucratic rationality and popular rhetoric. The future of modernity is the "iron cage" that reason has unwittingly forged for itself and where it is destined to dwell—unfree, disenchanted, longing for meaning, unsure of its higher purpose (Weber [1904–5] 1958, p. 182).[2]

It is easy to see how much *Critique of Instrumental Reason, One-Dimensional Man,* or *Negative Dialectics* owes to this dark vision. Weber's unsentimental insights into the rationalization process and its unintended

[2] For further discussion of Weber's views on formal and substantive democracy see Giddens (1972), Mommsen (1974), and Cohen (1985).

systemic consequences have been absorbed into such critical-theoretic concepts as the "administered state," "totalitarian democracy," "expert cultures," "isolation through communication," and "media-distorted discourse." His disdain for formal democracy seems to have been borne out by actual historical developments in the capitalist West and the communist East. And his skepticism about the prospects for substantive democracy neatly rationalized the Frankfurt school's failure to tie its theory to political practice. There was a penalty, of course, that critical theorists had to pay for embracing Weber—surrendering rational hope for emancipation. This was what "melancholy science," as Adorno dubbed critical theory, was coming to. And this was why Jürgen Habermas found the Frankfurt school's confines too narrow and moved beyond its fold. He did it to take a fresh look at the question that the old-generation critical thinkers left unanswered: Is emancipation through reason a rational hope? Habermas's *Theory of Communicative Action* can be seen as an attempt to invigorate critical theory by merging the Continental and Anglo-Saxon traditions and bringing the pragmatist perspective to bear on the project of emancipation through reason.

FROM CRITICAL THEORY TO COMMUNICATIVE ACTION

"It is only in Western nations that the precarious and continually threatened achievements of bourgeois emancipation and the worker's movement are guaranteed to any extent worth mentioning. . . . And we know just how important bourgeois freedoms are. For when things go wrong it is those on the Left who become the first victims" (Habermas 1986, p. 42). This intriguing observation illuminates a paradox: the very fact that the Institute for Social Research, the hotbed of critical thinking, has been thriving in capitalist West Germany seems to suggest that it serves the existing order. This contradiction has not been lost on the right- and left-wing critics, who alternatively charged the Frankfurt school leaders with ingratitude toward the existing order or betraying working-class interests. In their defense, critical theorists pointed to the marginal position they occupy in the academe, the media's power to blunt the critical message, and the false consciousness pervading capitalist society, yet these explanations are rather half-hearted, given the prominent positions that critical theorists acquired in German academia after World War II, and they certainly do not go to the heart of the matter. The real problem is that "the old Frankfurt School never took bourgeois democracy very seriously" (Habermas 1986, p. 98). By contrast, Habermas takes pains to emphasize that academic freedom is for real, that bourgeois democracy is a major historical accomplishment, and that its liberal institutions are indispensable for genuine criticism. All this by no means obviates the fundamental criticism that critical theorists have leveled against the capitalist order, most signally against "the pervasive inequality of freedom [and] unequal opportunity of access to the means of democratic persuasion" (Marcuse [1968] 1976, p. 326). Habermas accepts the premise that the rationalization process has produced systemic consequences highly injurious to the democratic process: modern systems are unmanageably complex; a consumerist economy manufactures false needs; the mass media manipulates public opinion; expert cultures obfuscate the public's stake in technical issues; and relentless bureaucratization robs humans of their autonomy, dignity, and solidarity (Habermas [1962] 1989, pp. 141–222; [1981] 1987*b*, pp. 332–73). These are the familiar ills of late capitalism. Formidable though they are, they do not spell democracy's impending doom. The old-school critical theorists have grown unreasonably pessimistic about the project of modernity, but their pessimism is historically unfounded and theoretically fallacious; the prospects for emancipation through reason "can today no longer be disqualified as simply utopian" (Habermas 1989, p. 235). The agenda for the day is "the reconciliation of a modernity which has fallen apart," the rededication to the idea "that without surrendering the differentiation that modernity has made possible in the cultural and economic spheres, one can find forms of living together in which autonomy and dependency can truly enter into a non-antagonistic relation, that one can walk tall in a collectivity that does not have the dubious quality of backward-looking substantial forms of community" (Habermas 1986, p. 125). To salvage the project of modernity, critical theory must cure the democratic process of distortions it suffers in a capitalist society. How can this be done? Habermas answers with a prescription borrowed from American pragmatism: by mobilizing the public, revitalizing public discourse, and getting personally involved in politics.

Habermas's willingness to join issues with pragmatism is very much at odds with the German tradition, in which the intellectual was "bred in the veneration of theory and history, and contempt for empiricism and pragmatism" (Neumann 1953, p. 19). Frankfurt school thinkers were solidly embedded in this tradition, their writings evincing little appreciation for pragmatism's emancipatory potential (Marcuse 1939/1940; Horkheimer 1937, 1947). They dismissed pragmatism in a wholesale fashion as "the abasement of reason" and "a genuine expression of the positivistic approach," a philosophy which advocates the "reduction of reason to a mere instrument" and serves as a "counterpart of modern industrialism, for which the factory is the prototype of human existence, and which models all branches of culture after production on the conveyor belt, or after the rationalized front office" (Horkheimer 1947, pp. 45–54). Habermas's break with this tradition was not instantaneous. According to his own account (Habermas 1986, pp. 104, 151, 193), his interest in pragmatism goes back to the early 1960s, when Karl-Otto Apel encouraged him to read Peirce and other pragmatists. *Knowledge and Interest,* published in Germany in 1968 (Habermas 1971; see also Habermas 1975), is the first work in which Habermas treats pragmatism systematically. There is no mention of Mead in this volume, Dewey is cited once or twice, but Peirce is treated at length as a representative pragmatist thinker. The treatment is more sympathetic than the one accorded by Marcuse and Horkheimer, but it does not break completely with the thesis, first advanced by Max Scheler ([1926] 1977), that pragmatism exemplifies a formal-rational preoccupation with nature that undermines the normative discourse embedded in substantive rationality. What Habermas (1986, p. 193) finds appealing in Peirce is the "logical socialism" implicit in the latter's exalted view of a community of rational thinkers engaged in critical inquiry and ceaselessly advancing toward the truth through uncoerced discourse, rational argumentation, and consensus building. It was not until Habermas encountered Dewey and Mead, however, that he fully realized the momentous implications that Peirce's ideas had for critical theory.

"The radical-democratic branch of Young Hegelianism" is the term Habermas (1986, p. 151) coined to frame the pragmatism espoused by Dewey and Mead. This apt description highlights the often overlooked debt that pragmatist thinkers owe to German idealism, the ingenuous manner in which Peirce, Dewey, Mead, and to a lesser extent, William James developed a Hegelian concern with language, communication, and intersubjectivity—the social dimension of reason. Dewey's writings were particularly instrumental in sensitizing Habermas to the continuity between scientific inquiry and democratic discourse, to the fact that "freedom of inquiry, toleration of diverse views, freedom of communication, the distribution of what is found out to every individual as the ultimate intellectual consumer, are involved in the democratic as in the scientific method" (Dewey 1939, p. 102). From the same source comes Habermas's appreciation for the public and its role in sustaining inquiry into communal affairs. The prospect for democracy, Dewey contended and Habermas agreed, "rests upon persuasion, upon ability to convince and be convinced," upon "the improvement of the methods and conditions of debate, discussion and persuasion. That is *the* problem of the public" (Dewey 1916*a,* p. 134; 1939, p. 102). Taking the argument one step further, Dewey (1946, p. 132) comes up with an appeal that critical theorists would have appreciated, if not fully endorsed: "Humane liberalism in order to save itself must cease to deal with symptoms and go to the causes of which inequalities and oppressions are but the symptoms. In order to endure under present conditions, liberalism must become radical in the sense that, instead of using social power to ameliorate the evil consequences of the existing system, it shall use social power to change the system."

Mead caught Habermas's attention for some of the same reasons that Dewey did, but in addition to Mead's progressive democratic agenda, Habermas found in his writings a theory that "elevated symbolically mediated interaction to the new paradigm of reason" and that signified a major advance beyond the old "paradigm of the philosophy of consciousness" (Habermas 1984, p. 390). This point, crucial to Habermas's own project, deserves some elaboration. We can recall that critical idealists placed much stock in the historical process that elevates biased, unreflexive, everyday understanding (*Verstand*) to the loftier status of self-conscious reason that spearheads criticism and attends to higher truths (*Vernunft*). Habermas traces this trust in the noble faculties of reason to the tradition that stretches from Descartes, through German idealism, to critical theorists. The problem with this tradition, as Habermas sees it, is that it does not incorporate the sociological perspective on reason as a communicative affair; instead, it treats reason as a unitary phenomenon modeled after instrumental labor activity, as a process bound to the subject, who confronts the world all alone and single-handedly transforms it into a rational objective whole. Hegel's objective idealism did entail some tantalizing insights into the role that language and community play in the genesis of self-consciousness, but much of his work followed the old paradigm, and whatever sociologically relevant ideas he had presaged failed to take root on German soil.[3] By contrast, American pragmatists seized exactly this neglected aspect of German idealism, expanding it into a new paradigm of reason as social through and through. Sidestepping the familiar pair of *Verstand* and *Vernunft,* the new paradigm gives prominence to *Verständigung,* the interactive process of reaching understanding. "The change in perspective from solitary rational purposiveness to social interaction," writes Habermas (1987*a,* p. 149), "does promise to illuminate the very process of mutual understanding [*Verständigung*]—and not merely of understanding [*Verstehen*]." Mead, Habermas continues, resolutely renounced the paradigm of reason as solitary consciousness and went further than other contemporary scholars to lay out the paradigm of reason as communicative action and to spell out its implications for the emancipatory agenda, which is why Mead's ideas must be included in any theory that assigns to self-consciousness and critique a role in social reconstruction.

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[3] Habermas may be underestimating the extent to which the social dimension of reason was elaborated in the German tradition in general and in transcendental idealism in particular. For an alternative view, see Royce (1919, p. 65), Mead (1936, p. 147), and Shalin (1986*a,* 1986*b*).

Indeed, Mead not only joins in the classical discourse on rationality and emancipation through reason—he also pushes it in a new direction. His discussion owes much to German idealism, which Mead studied as a student at Harvard and in Berlin and then taught for many years at the University of Chicago.[4] His views on evolution as a process that brings nature to self-consciousness and assures humans "some degree of control of the process of evolution out of which they arose" (Mead 1938, p. 511) bring to mind Hegel's phenomenology, with its dialectics of reason that objectifies itself in nature, finds itself estranged from its own products, and then gradually rediscovers its authorship over the way things are. But Mead's approach is also thoroughly informed by the evolutionary perspective and the pragmatist determination to tie thinking to conduct. "What I have attempted to do," explains Mead (1934, p. 334), "is to bring rationality back to a certain type of conduct, the type of conduct in which the individual puts himself in the attitude of the whole community to which he belongs. This implies that the whole group is involved in some organized activity and that in this organized activity the action of one calls for the action of the other organisms involved. What we term 'reason' arises when one of the organisms takes into its own response the attitude of the other organisms involved. . . . When it does so, it is what we term 'a rational being.'" Reason is historically embedded in communal existence; once brought into being, it transforms community life itself, for, according to Mead, "when the process of evolution has passed under the control of social reason" (1938, p. 508), it "becomes not only self-conscious but also self-critical" (1934, p. 255). From a central preoccupation with the mastery over things, reason now turns toward the questions of value. To use Weberian terminology, reason becomes substantive; that is, it reevaluates values, rationally resolves social conflicts, and endeavors to revamp the entire social order from which it sprang:

> The rational solution of the conflict, however, calls for the reconstruction of both habits and values, and this involves transcending the order of the community. A hypothetically different order suggests itself and becomes the end in conduct. . . . In logical terms there is established a universe of discourse which transcends the specific order within which the members of the community may, in a specific conflict, place themselves outside of the community order as it exists, and agree upon changed habits of action and a restatement of values. Rational procedure, therefore, sets up an order within which thought operates. . . . Its claims are the claims of reason. It

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[4] See Joas (1985) and Shalin (1984, 1990) on the Mead-idealism connection.

> is a social order that includes any rational being who is or may be in any way implicated in the situation. [Mead (1930) 1964, p. 404]

A cursory look at Habermas's theory reveals the measure of his debt to pragmatism. We find in his work the same mixture of historical optimism that harks back to critical idealism and tough-minded realism found in Progressive Era pragmatism. The belief in "a noncoercively unifying, consensus building force of a discourse in which the participants overcome their at first subjectively biased views in favor of a rationally motivated agreement" (Habermas 1987*a,* p. 315) is combined here with a keen awareness that communications remain "systematically distorted" in a "money-bound," "media-steered" society that keeps public discourse from realizing its full critical potential (Habermas 1987*b,* pp. 256–82). In spite of these instructive continuities, there are several issues on which Habermas and pragmatists part company. I shall come back to the pragmatist critique of Habermas in the next section; here I consider the points for which Habermas takes pragmatists to task.

From the sociological standpoint, pragmatism's central contribution is to an "action-theoretic" framework. Symbolic interactionists have explored at length the linguistically mediated interactions in which human identities are formed, and thereby they have expanded our understanding of the communicative foundations of life worlds. At the same time, pragmatism-inspired social theory has little to offer to "system-theoretic" approaches, Habermas insists. It ignores the normative underpinnings of society, its functional needs as a system, and it cannot satisfactorily explain how communicative distortions and social oppression are generated and reproduced. Thus, Mead assumes the normative status for his notion of the "generalized other" without explaining where its power to control behavior comes from. Similarly, Dewey is too sanguine about democratization's by-products—expert cultures and administrative procedures, which are as endemic to modern democracy as they are subversive of its substance. "We want democratization," Habermas (1986, p. 67) intones, "not so much in order to improve the efficiency of the economy as to change the *structures* of power: and in the second place to set in motion ways of defining collective goals that merely administrative procedures or power-oriented decisions would lead astray or cripple."

It is arguable whether pragmatism lacks normative dimension, let alone whether it is inherently incapable of dealing with structural phenomena, but it is fair to say that system-theoretic issues have not been central to pragmatist analysis in the past. To offset this limitation of classical pragmatism, Habermas seeks to complement it with ideas from several other sources. From Weber, he borrows his insight into the differentiation of value spheres; from Durkheim, the notion of normative constraint; from Parsons and Luhmann, a version of systems theory. From Austin, Wittgenstein, and Searle, he appropriates the theory of speech acts, and from Kohlberg and Piaget, the genetic theory of moral growth. Combining these ideas with the Meadian theory of symbolic interaction, Habermas formulates his theory of communicative action (TCA), with its conceptual core—"universal pragmatics." Habermas uses the term "pragmatics" in a different sense than Peirce, who saw in it a branch of cosmology dealing with signs in their natural settings, or Charles Morris, who used the term to designate a part of semiotics that explicates the relations between symbols and their users. The pragmatics that Habermas has in mind is "universal"—it purports to unveil most general standards that govern rational communications in human discourse. Drawing on Weber's theory, Habermas isolates three basic forms of discourse or value spheres that become progressively autonomous in the course of historical rationalization: theoretic/scientific, moral/practical, and expressive/aesthetic. Communications within each of these domains revolve around a peculiar validity claim: theoretic discourse concerns the truth of our propositions, practical discourse bears on the justice of our actions, and aesthetic discourse highlights the sincerity of our feelings. Although these validity claims are intertwined with scientific, moral, and artistic discourses, they are not bound exclusively to these specialized value spheres. In our everyday life, we routinely assert facts, appeal to norms, and claim to be sincere; that is, we raise and settle validity claims concerning truth, justice, and authenticity, and, by doing so, we continuously reproduce our normative, cultural, and private worlds. In the language of speech act theory, we "do things with words" via "performative actions," which are linguistic facts just as they are social facts. Now, the crucial point Habermas makes is that the validity claims remain largely unthematized in everyday transactions, during which they are redeemed not so much by recourse to reasons and arguments as through strategic action and appeal to custom. It is the task of universal pragmatics to render these unreflexive validity claims problematic, to help settle them by rational means. Universal pragmatics articulates "a procedural concept of rationality," "a pragmatic logic of argumentation" (Habermas 1987*a*, p. 314), and promises to certify "the rationality of process of reaching understanding" (Habermas 1985, p. 196). Its main premise is that any communicative act aimed at reaching understanding contains implicit, context-free, and imminently social standards that must be met if its outcome is to be judged rational. The situation where such standards are fully met is called "ideal speech situation."

Habermas does not provide a glossary of rules underlying the ideal speech situation. Nor does he offer any final formulation, as he continues to revise the theory he first outlined in the late 1960s (see, e.g., Habermas

1970, 1979). Still, I think these procedural rules or guidelines for achieving communicative rationality can be codified as follows:

1. An ideal speech situation provides every interested individual a chance to participate in discourse and argue one's viewpoint.
2. It is free from coercion, domination, and power play—all purely instrumental and strategic motifs.
3. It differentiates cognitive, normative, and expressive validity claims implicit in our assertions and redeems them through arguments alone.
4. It makes a freely reached consensus the sole foundation for democratic will formation and policy articulation.
5. It leaves a rationally motivated agreement open to revision in light of further deliberations.

The thing that strikes one immediately is how well these stipulations jibe with the critical idealists' belief in *Vernunft* as "the true tribunal for all disputes of reason [which] secures to us the peace of a legal status, in which disputes are not to be carried on except in the proper form of a *lawsuit*" (Kant 1966, p. 486). The continuity does not escape Habermas, who grounds his theory on the "principle, that—expressed in the Kantian manner—only reason should have force" (Habermas 1970, p. 7). At the same time, Habermas is quick to point out that his theory is not to be confused with transcendental idealism. Universal pragmatics presupposes certain standards for rationality and serves as a measuring rod for judging concrete communicative practices, yet its validity is not entirely a priori. Procedural standards for rationality spelled out in universal pragmatics are counterfactual: "One should not imagine the ideal speech situation as a utopian model of an emancipated society" (Habermas 1986, p. 90). Nor should an ideal speech situation be confused with an ideal type, for the latter professes ethical neutrality whereas the former is self-consciously normative and prescriptive. Universal pragmatics is the case of "reconstructive theory" (Habermas 1979, pp. 8–9, 178–79); that is, theory whose normative thrust does not preclude empirical validation, even if it can be achieved only indirectly. Taking his clues from Durkheim's writings on the sacred and Piaget-Kohlberg's research on moral growth, Habermas infers that the movement toward communicative rationality is both an evolutionary trend, evident in the shift from sacred to discursive practices, and an ontogenetic current, manifest in the gradual increase in the individual's capacity for moral reasoning. At the heart of modernity is the empirically observable drive toward rationalization, which gradually replaces "the weight of tradition with the weight of arguments, . . . an attitude of faith based on the authority of a doctrine with a theoretical attitude" (Habermas 1979, p. 113), and it is this relentless drive that pulls society away "from the sacred foundations of legitimation to foundation on a common will, communicatively shaped and discursively clarified in the political public sphere" (Habermas 1987*b,* p. 81). This shift, most apparent in the history of the Occidental world, can be gleaned from the gradual gain in human rights, the emergence of the independent judiciary, the separation of cognitive and power claims, the strengthening of voluntary associations, and similar developments that mark the movement, however contradictory, toward communicatively rational forms of legitimation.

Bureaucratization, juridification, mediatization, and such like systemic dysfunctions point to another, less benign facet of rationalization. Subjected to the capitalist market imperatives, these developments produce distortions that undermine communicative rationality and weaken public discourse. As each value sphere evolves according to its own logic, it becomes insular, impregnable to considerations from other value spheres: technical issues are separated from moral concerns; ethical demands are severed from expressive needs; personal agendas come into conflict with public ones. The mass media further exacerbates these trends by making a spectacle out of public discourse, turning it into an entertainment: "Discussion, now a 'business,' becomes formalized; the presentation of positions and counterpositions is bound to certain prearranged rules of the game; consensus about the public matter is made largely superfluous. . . . Critical debate arranged in this manner fulfills important social-psychological functions, especially that of a tranquilizing substitute for action" (Habermas 1989, p. 164). Now, it needs to be stressed that the problem for Habermas is not modernity and rationalization as such, but fractured modernity and one-sided rationalization, and the cure is breaking the walls that separate value spheres without destroying an insight peculiar to each. The question, in other words, is how to "bring viewpoints of moral and aesthetic critique to bear—without threatening the primacy of questions of truth" (Habermas 1987*b,* p. 398). If this can be done at all, it is through public discourse, by painstakingly redeeming the validity claims implicit in our communications and following the rules of procedural rationality elucidated in the universal pragmatics. One hundred percent procedurally rational communications may be impossible to achieve, but by opening up the legitimation process to all members of society and verbalizing norms previously immune to rational adjudication, we, at the very least, assure movement in the right direction. Take the ideal speech situation seriously, Habermas advises, rid yourself of hidden agendas and avail yourself of procedurally rational discourse, and you help bring about an emancipated society, a democracy that is substantively rational. "The false alternative set up by Max Weber, with his opposition between substantive and formal rationality, is overcome" (Habermas 1987*a,* p. 315).

Most commentators agree that TCA constitutes an important advance, or certainly a new beginning, for critical theory. This advance has not come cheaply. In some respects, Habermas's theory was a step backward (e.g., Habermas does not show Adorno's sensitivity to the indeterminate and the irrational). In settling old issues, Habermas has raised new, sometimes even more vexing ones. Is reason genderless and classless? Does it have to shed its ethnic, racial, religious, cultural, and personal colors before it can do its critical job? How can theoretical, practical, and aesthetic discourses inform each other without losing their vital autonomy? What about contingency and indeterminacy thwarting our best plans and good faith efforts? Is the lack of rational consensus a sign that communicative action has failed? These are just some of the questions that TCA has stirred up and that have generated a voluminous literature.[5] I shall try not to repeat the more obvious criticisms voiced in the past, and, in keeping with my objectives, confine my comments to the issues on the interfaces of pragmatism, democracy, and critical theory.

FROM COMMUNICATIVE ACTION TO PRAGMATIC POLITICS

The ideal speech situation outlined in Habermas's universal pragmatics is more than a prescription for successful communication. Enciphered in its principles is a blueprint for a rational society—a society whose members make good sense, offer rationales for their action, mean what they say, and practice what they preach. This lofty image, which brings to mind the ancient quest for a way of life combining truth, justice, and happiness, has undeniable appeal. It is also flawed in several respects. My critique is sympathetic, for I share Habermas's humanistic agenda, yet it is principled because I question some of his fundamental premises. My discussion draws on the pragmatist ideas left out in Habermas's analysis, and it is organized around the following themes: disembodied reason versus embodied reasonableness, determinate being versus indeterminate reality, discursive validity versus pragmatic certainty, rational consensus versus reasonable dissent, transcendental democracy versus democratic transcendence, and rational society versus sane community. It should be noted that I do not try to picture pragmatism as a monolithic movement free from internal contradictions and inconsistencies. However, the present reconstruction centers on pragmatists' shared concerns, and especially on their common stance against rationalism, the vestiges of which can be found in Habermas's thought.[6]

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[5] A wide range of critical comments on Habermas's corpus can be found in two representative collections: *Habermas: Critical Debates*, edited by Thompson and Held (1982), and *Habermas and Modernity*, edited by Bernstein (1985). For a more detailed discussion of Habermas's theory of communicative action see McCarthy (1978), Ferrara (1985), Benhabib (1987), Antonio (1989), and a special issue of *Symbolic Interaction* (Shalin 1992).

Disembodied Reason versus Embodied Reasonableness

My first objection concerns the place Habermas assigns to reason in relation to nature and human body. Reason appears in TCA primarily as thinking (consciousness, understanding, cognition). It has no obvious relation to the human body and noncognitive processes (emotions, feelings, sentiments). What pragmatists call "experience" has shriveled into verbal intellect, which assumes in TCA a privileged position as a locus of rationality. Communicative competence is predicated on reason's capacity to be "relieved of the pressure of action and experience" (Habermas 1984, p. 25), to "transcend all limitations of space and time, all the provincial limitations of the given context" (Habermas 1987*b*, p. 399). Rational discourse, correlatively, deals in ideas, concepts, and reasons rather than in sentiments. The latter represent an inferior species of intelligence, in that they have limited generalizability, cannot be readily communicated, are inherently uncritical, and need to be edified by intellect. To the extent that noncognitive elements enter discourse, they have to be grasped conceptually and measured by theoretically grounded standards, a process that certifies our emotive life as authentic and sincere. Affects that do not pass the test set up by reason are deemed "irrational" and subjected to "therapeutic critique," which helps the individual "free himself from illusions, and indeed from illusions that are based not on errors (about facts) but on self-deceptions (about one's own subjective experiences)" (Habermas 1984, p. 21).

By contrast, pragmatists caution against the "hypostatization of cognitive behavior" (Rorty 1982, p. 201) and warn that consciousness "is only a very small and shifting portion of experience" (Dewey 1916*b*, p. 6). "Reason, anyway, is a faculty of secondary rank," Peirce (1976, p. xxi) remarks, "cognition is but the superficial film of the soul, while sentiment penetrates its substance." What is important for the pragmatist is that cognitive behavior belongs to a larger context of material practice, which philogenetically and ontogenetically antedates mind's conceptual faculty. Communication is contingent on minding something together, carrying out a larger act in which participants are engaged bodily as well as mentally. "Mental processes imply not only mind but that somebody is minding" (Mead 1938, p. 69). "The mother minds her baby; she cares for it with affection. Mind is care in the sense of solicitude, anxiety, as well as of active looking after things that need to be tended" (Dewey [1934] 1958*b*, p. 263). Pragmatists refuse to isolate communicative actions from this larger context, from "the universe of nonreflectional experience of our doings, sufferings, enjoyments of the world and of one another" (Dewey 1916*b*, p. 9). Notice that the pragmatist maxim—knowing is doing—brooks no anti-intellectualism. Pragmatists do not deny the key role abstraction and generalization play in theoretical discourse, nor do they dispute that private interests and crude emotions can distort reasoning. Nevertheless, pragmatists argue, the "conclusion is not that the emotional, passionate phase of action can be or should be eliminated in behalf of a bloodless reason. More 'passions,' not fewer, is the answer. . . . Rationality, once more is not a force to evoke against impulse and habit. It is the attainment of a working harmony among diverse desires" (Dewey [1922] 1950*b*, pp. 195–96). Pragmatists are quick to point out that "reasoning has no monopoly of the process of generalization," that "sentiment also generalizes itself" (Peirce 1976, p. xxi). Feelings can be universalized and communicated even more readily than ideas. We share attitudes before we share thoughts (Mead 1934), we sympathize before we understand (Benhabib 1987), we feel other people's pain before we know its source (Rorty 1989). Habermas shows little appreciation for such nondiscursive communication. He elevates the cognitive form of universality above all others and in the process inadvertently devalues human experience as merely private and intellectually mute. This indifference to the nondiscursive element in culture, to the fact that just "as the body becomes 'encultured,' . . . so culture becomes 'embodied'" (Alexander 1987, p. xix), is a vestige of rationalism and its notorious tendency to think in dichotomies, such as subject and object, reason and nature, sentiment and intellect, and so on. Pragmatists, on the other hand, are convinced that noncognitive prehensions have an intelligence all their own, which a radically theoretical attitude tends to ignore or, worse, suppress. Contrary to the rationalist view, reason has a lot to learn from noncognitive functions: feelings point to a crisis in experience, sentiments signal when general principles take a beating from obdurate reality, emotions provide a running commentary on the success of our plans. To divest reason from living experience is to disembody it, to leave it helpless in the face of the perennial indeterminacy and contingency with which humans have to struggle in their everyday existence. When thinking leaves experience far behind and escapes into *theoria,* it is likely to lead practical action astray.

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[6] Among contemporary works on pragmatism, I found particularly useful the following: Alexander (1987), Bernstein (1983), Coughlan (1975), Joas (1985), Kloppenberg (1986), Rockberg-Halton (1986), Rorty (1979, 1982), and Rosenthal (1986).

I am not trying to ascribe to Habermas an untenable view that feelings and emotions are inherently irrational and need to be suppressed in favor of pure reason. He is also right when he says that some of our sentiments are systematically distorted and have to be subjected to therapeutic critique. My point is rather that TCA leaves out from its purview the noncognitive forms of intelligence irreducible to verbal intellect, what pragmatists call "embodied" or "concrete reasonableness" (Rochberg-Halton 1986; Alexander 1987). Reasonableness is minding embedded in practical activity and embodied in emotionally charged situations. It does not scoff at common sense or *Verstand,* and it resists *Vernunft's* imperious tendency to subordinate other faculties to its dictate. Knowledge uninformed by feelings and stripped of emotive elements can be rational without being reasonable; it achieves certainty by discarding insight from the senses in favor of the rationales laid out by the intellect. Yet, even though both noncognitive experience and speculative thought partake in the world, the former is embedded in nature more immediately, yielding instant information about the changing situation through its affective states vital to the organism: "Experience is *of* as well as *in* nature. [It] reaches down into nature; it has depth. It also has breadth and to an indefinitely elastic extent. It stretches. That stretch constitutes inference" (Dewey [1929] 1958*a,* p. 4a). "The continuum which [sentiment] forms instead of being like that of reason merely cognitive, superficial, or subjective . . . penetrates through the whole being of the soul, and is objective or to use a better word extant, and more than that is existent" (Peirce 1976, p. xxi). Reason's access to the world, by contrast, is mediated by a feeling body, whose testimony certifies our validity claims. It is in the Platonic domain that reason reigns supreme, the domain where objects are not contaminated by impurities besetting the mundane realm and obey laws prescribed by pure reason. As long as reason stays within this rarefied chamber, it can abstract from concrete situation and take a profitable leave of one's emotional investments, but as soon as knowers step into the world of uncertainty, they inexorably fall back on an auxiliary intelligence about things themselves that only noncognitive faculties can gather. Human intelligence is emotional just as emotions are intelligent, and this is so because we live in the world of indeterminacy that no rational faculty and theoretical rigor can expunge.

Determinate Being versus Indeterminate Reality

The residual place Habermas assigns to body and noncognitive experience is consistent with a rationalist ontology. This ontology paints an overdeterminate picture of the universe as factual, internally structured, determined prior to the knower's engagement in it, and marked by "the categorical distinctions between the objective, social, and subjective worlds" (Habermas 1987*b,* p. 159). The early and relatively undifferentiated worldviews (those centered on mythology or religion) tend to blur the distinctions between these three worlds and the validity claims peculiar to each. Knowledge appears in such worldviews as a mixture of the objective and the subjective, the particular and the general, the cognitive and the emotional, with no sharp line drawn between verifiable facts, culturally sanctioned dogma, and patently idiosyncratic claims. The modern—scientific—outlook allows one to separate verifiable facts from commonly held illusions and makes possible, at least in principle, the rational adjudication of conflicts in the normative sphere. Purified by proper method from unwarranted preconceptions, ideological obfuscations, and personal biases, public discourse yields a rationally motivated consensus about facts in our shared universe and points to the most rational course of action.

Twentieth-century pragmatist ontology has come to us bearing different labels—"pluralism," "perspectivism," "objective relativism," and more recently "new fuzziness" (Rorty 1989, p. 51). Yet its basic insight is essentially the same: "Uncertainty does not belong simply to the values, it belongs to the facts as well" (Mead, n.d., *b*8, *f*1).[7] "Any view which holds that man is a part of nature, not outside it," according to Dewey (1946, p. 351), "will certainly hold that indeterminacy in human experience, once experience is taken in the objective sense of interacting behavior and not as a private conceit added on to something totally alien to it, is evidence of some corresponding indeterminateness in the process of nature within which man exists (acts) and out of which he arose." The pragmatist ontology pictures a universe vastly different from the one envisioned by classical rationalism, "a universe which is not all closed and settled, which is still in some respects indeterminate and in the making, . . . an open universe in which uncertainty, choice, hypotheses, novelties and possibilities are naturalized" (Dewey [1927] 1950*c*, p. 52). Such a universe is full of uncertain outcomes that defy our best efforts to reduce chaotic processes to a theoretical scheme in which all effects have identifiable causes and our destiny submits to rational manipulation. Pragmatist philosophy "gives us a pluralistic, restless universe, in which no single point of view can ever take in the whole scene" (James [1897] 1956, p. 177). "Man lives in a world of surmise, of mystery, of uncertainties," admonishes Dewey (1958*b*, p. 198), "'Reasoning' must fail man." Again, it is easy to misconstrue these musings as a sign of anti-intellectualism, but the point is not that rational knowledge is impossible or useless, but that rationalists underestimate the contingency endemic to the world, and they vastly exaggerate reason's capacity to marshal it as an orderly flow of objective being.

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[7] Mead (n.d.) refers to the George H. Mead papers gathered in the Special Collections Department of the Joseph Regenstein Library at the University of Chicago. The letters *b* and *f* followed by a number indicate the box and folder, respectively, in which a particular document is located.

What is to be stressed here is that we are dealing not with residual indeterminacy reflecting the limits of our current knowledge but with "objective uncertainty" and "indeterminate reality," the emergent universe in which "deliberation and choice are determining factors" (Dewey 1950*b,* p. 310). It is up to concrete reasoning—always an interest-bound, socially anchored, situationally specific undertaking—to lift the world from its natural state of indeterminacy and turn it into a meaningful, manageable, semiorderly whole. This objective whole maintains its predictable properties insofar as we sustain our interest in it, as long as our determined collective efforts last. Each time we pass judgment on the situation at hand—literally terminate indeterminacy—we bring out some of its potentialities and render obscure its other possible determinations. An act of doing justice, which a theoretical, normative, or aesthetic judgment aspires to be, is thus inevitably an act of doing violence. Just as it opens one horizon of meaning it closes an indefinite number of alternative determinations (fittingly, "to terminate" means to extinguish, to put an end to, as well as to bring into focus, to frame in definite terms). Whatever determinacy we encounter in the world is, consequently, of our own making. We terminate indeterminacy in deed and in situ, using terms supplied by a community, and we do so as participant-observers who are part and parcel of the situation we seek to comprehend: "If there were no human beings (or comparable sentient creatures) there would be no situations in nature" (Gouinlock 1972, p. 8). The knower's embeddedness in the world as a participant-observer has far-reaching epistemological implications, none more important than this: validity claims about the world of uncertainty cannot be settled through argument in the propositional discourse; the redemption of validity claims is a pragmatic endeavor accomplished via social intercourse.

Discursive Validity versus Pragmatic Certainty

One of the pillars on which Habermas founded TCA is his "consensus theory of truth" (Habermas 1973, p. 19; 1984, pp. 8–42). We can speak more broadly about the discursive theory of validation, for all validity claims, including rightness and sincerity, are at issue. This theory stipulates that validity claims must be redeemed through arguments and that "communicative actors can achieve an understanding only by way of taking yes/no positions on criticizable validity claims" (Habermas 1984, p. 70). If "reasons that force us to take a rationally motivated position of yes or no" (Habermas 1985, pp. 194–95) failed to produce a consensus, a communicative action has missed its stated end; such failure signifies that the participants lacked candor to carry out communicative action to its rational conclusion. One notable exception allowed by discursive theory of validation involves aesthetic discourse: "The claims to sincerity connected with expressive utterance is [*sic*] not such that it could be directly redeemed through argument as can truth or rightness claims. . . . The sincerity of expressions cannot be *grounded* but only *shown;* insincerity can be *revealed* by the lack of consistency between an utterance and the past or future actions internally connected with it" (Habermas 1984, p. 41). No extradiscursive means are allowed into theoretical and normative discourses, where participants are compelled toward a rational consensus by methodically advancing well-formed propositions, by clarifying the internal logic of the argument, and by adjudicating conflicts through theoretical means.

While TCA attends to discursive validity vouchsafed through propositional formal logic, pragmatist theory focuses on "pragmatic certainty" (Rosenthal 1986, p. 59), which requires joined action and logistical reasoning as much as argumentative skills. Discursive validation is part of a larger human practice where all ideational objects have their roots. There are no objects to perceive, to value, to abstract from, according to pragmatists, until there has been the "full completion of the act" or "consummation" (Mead 1938, p. 23). The world that lends itself to objective judgment is already an objectified world, reality transformed by our perception, cognition, and collective action, and to say that our thought is true to this world makes as much sense as to say that this world is true to our preconceptions about it. Either way, to be certain about our claims, we have to engage in collective transformative action. Pragmatists are at one with Marx on this: "The question whether objective truth is an attribute of human thought—is not a theoretical but a *practical* question" (Marx [1846] 1963, p. 197). That is to say, there is more to redeeming truth claims than finding good reasons and building consensus about them. We need to be certain that the predicated identity between knowledge and reality can be actually redeemed, and that means immersing oneself in the situation, joining in a collective act, and carrying it to a completion.

A word of caution against setting up a false dichotomy between practically reached certainty and communicatively established consensus is called for: one is meaningless without the other. The pragmatist critique presented here aims at redressing the balance. Rational arguments have been advanced in favor of releasing mentally ill patients who pose no immediate threat to themselves and to the public, and a solid public consensus was built around this issue in the 1960s. Yet this perfectly rational policy turned out to be a failure, as the logistics of providing for the ex-patients' needs via neighborhood communities proved to be much too formidable for the effective delivery of noninstitutional care. The moral here is that we cannot always redeem substantive claims—we cannot be certain about their pragmatic merit—in advance of staking our action on their truthfulness or, for that matter, justness and sincerity (the premise equally applies to theoretical, normative, and aesthetic discourses). The pertinent question for pragmatists is not whether a proposition is true according to some intrinsic rationality, but whether the real situation can be shaped according to a stated rationale. To find out how consensually validated terms mesh with reality, we have to move beyond symbolic and performative action; we must try to terminate extant indeterminacy (or reterminate established determinacy) through practical social intercourse. Truth is no longer grasped here in the rationalist manner as *adequatio intellectus et rei* but instead is pragmatically conceived as a practically accomplished unity of knowledge and reality. In this reckoning, it makes as much sense to inquire whether an action-transformed situation is true to our concepts as to ask whether our propositions correspond to objective reality. To accomplish pragmatically the predicated unity of knowledge and reality, the knower has to engage in what interactionists call "joint action" (Blumer 1969, p. 17). This region, situated on the intersection between labor and discourse, has no conceptual footing in TCA. And yet, this is a domain of the utmost importance, the realm of everyday living and minding together where theoretical, normative, and aesthetic discourses merge into one, where humans feel, think, and transact at the same time, and where a different logic is called upon to help us master everyday contingencies.[8] The pragmatist logic is the logic in use; it stipulates that reality does not always lend itself squarely to yes/no judgments and allows practical knowers to say "perhaps," "it depends," "who knows," and to use other indeterminate truth values that help us handle situational indeterminacy. The dilemma that so struck William James ([1909] 1967, pp. 208, 207)—"either [I] give up my intellectualistic logic, the logic of identity . . . or, finally, face the fact that life is logically irrational"—is familiar to every practical knower who had to contend with everyday indeterminacy in the pluralistic universe and who could conclude, with James, that "logic being the lesser thing, the static incomplete abstraction, must succumb to reality, not reality to logic" (1967, p. 207). Binary logic favored by rationalist thought is replaced in pragmatism by the logic in use, also known as the logic of inquiry or the logic of situation. This pragmatist logic signals a break with "the rationalism's disdain for the particular, the personal, and the unwholesome" (James 1967, p. 309). It stipulates that judgment is "in" this world as much as it is "about" it, that "the proposition is itself a factor in the completion of the situation" (Dewey 1916*b*, p. 338), and it attends to "the transformation of an indeterminate unsettled situation into a determinate unified existential situation" (Dewey 1938, p. 296). The quest for rational truth and moral rightness is supplanted here by the quest for warranted assertability and practical certainty. Whereas the former depends on discursive validation and "comparing ready-made ideas with ready-made facts," the latter requires pragmatic inquiry and presumes that "both idea and 'facts' are flexible, and verification is the process of mutual adjustment, of organic interaction" (Dewey [1890] 1969, p. 87). The quest for pragmatic certainty sensitizes the knower to fuzzy things, multiple realities, semichaotic systems, and it favors participant-observation as a practical way to fathom objective uncertainty. The radically pragmatic epistemological stance also entails clear ethical and political implications: it counsels tolerance to ambiguity, calls for personal responsibility, and encourages rationally motivated dissent.

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[8] Various attempts have been made to conceptualize this intermediate space. Durkheim searched for it in the intraprofessional type interactions, as did Parsons, and Peter Burger detected it in the realm of communal, familial interactions. See David Sciulli (1988, 1992) for an overview of the issues involved.

Rational Consensus versus Reasonable Dissent

My next criticism concerns TCA's emphasis on consensus and its disregard for the constructive properties of dissent. On several occasions, Habermas (1979, pp. 1, 92) qualifies as "normal" situations and communications that are "largely conflict free." Consensus appears to be communication's raison d'etre. This is certainly true in an ideal speech situation where "all participants pursue illocutionary aims without reservation in order to arrive at an agreement that will provide the basis for a consensual coordination of individually pursued plans of action" (Habermas 1984, pp. 295–96). The broader the consensus, the greater the rationality. Anything that falls short of universal consensus is, on this premise, less than fully rational. Discourse ethics demands that those partaking in rational discourse should be motivated by the "intention of convincing a *universal audience* and gaining general assent for an utterance" (Habermas 1984, p. 26). Anybody who refuses to join in an emerging rational consensus, spurns communicatively certified reasons, or stops short of trying to convince all other participants of the merit of one's proposition violates the norms of discourse ethics. The spirit of communicative action militates against accepting conflict as a normal part of the communication process and dissensus as a rational product of action aimed at reaching understanding. "To be sure," Habermas (1985, p. 194) admits, "it is also a characteristic of modernity that we have grown accustomed to living with dissent in the realm of questions that admit of 'truth'; we simply put controversial validity claims to one side 'for the time being.'" Dissent about truth claims, in other words, is more an expediency than a principled stance in a communicatively shaped situation; in the long enough run, dissent must yield to rational consensus.

Pragmatist ontology and epistemology suggest a different approach, one that accentuates the limits of theoretically grounded consensus and highlights the productive properties of dissent. "Real possibilities, real indeterminations, real beginnings, real ends, real evil, real crises, catastrophes, and escapes, a real God and a real moral life, just as common sense conceives these things, may remain in [a radically pragmatic] empiricism which that philosophy gives up an attempt either to 'overcome' or to reinterpret in monistic form" (James 1956, p. ix). The universe so conceived belies TCA's consensual bias. The inexhaustible possibilities of being hidden in its depths can hardly be fathomed through a thin-gruel theoretical consensus. It is as if we choose to understand Bach by reading the music sheets rather than by listening to his fugues. Communication at the level of formal notation is what Habermas seems to propose in order to ferret out the communication's rational content. Yet the score is not the only thing that counts in music; each recital offers a dissenting yet valid interpretation or rather improvisation (just think about the vastly different communication that takes place when Vladimir Horowitz, Glenn Gould, Bella Davidovich, or the Modern Jazz Quartet interprets Bach). The pluralistic universe envisioned by pragmatists encourages dissent, warrants a wide margin of uncertainty, and invites caution toward policies based on purely theoretical calculations.

Habermas's universal pragmatics leaves hardly any room for the honest difference of opinion. A disagreement that refuses to go away is taken here as a sign of a failure, and a moral one at that: it hints at a strategic motif at work and/or betrays a weak communicative resolve. By grounding a rational consensus in sound reasons, Habermas also finds himself saddled by an awkward implication that whosoever refuses to abide by the communicatively established consensus is, by definition, less than fully rational. In the pragmatist reckoning, there is no necessary relationship between procedural rationality and substantive consensus. Public discourse is as much about consensus building as it is about fostering dissent. In pragmatist theory, by contrast, dissensus and consensus are accorded an equally prominent theoretical role. The proper function of communicatively achieved consensus is to designate reasons sound enough to merit pragmatic validation. A freely achieved consensus is usually partial, imperfect, provisional; it does not obviate the need for conflict, it legitimizes conflict as an inalienable part of rational discourse.

Nor does communicative action merely tolerate dissent, it encourages dissent as vital to the community's well-being. Dissent is the first sign that communication was uncoerced and that participants expressed themselves freely. It is in the countries where the speech situation is far from ideal that consensus is commonly forged and dissent becomes exceptional. What makes the dissenting attitude rational is the realization that various lines of argumentation can be meritorious, that the situation lends itself to more than one adjudication, that the attendant risks and uncertainties are great, and that the widest possible consensus is bound to break down the moment we set out to implement it.

All this should not be taken to mean that discursive consensus has no practical value. I am not trying to attribute to Habermas an untenable position that dissent does not matter. It is also true that some basic ground rules must be agreed upon before we can dissent in a meaningful and productive manner. Still, I feel that Habermas does not make nearly enough of rationally motivated dissensus, nor does he explore the practical consequences of dissent with which we must square off once we have agreed to disagree. Rational consensus, like a generally accepted moral rule, is but "a tool for analyzing a specific situation, the right or wrong being determined by the situation in its entirety, and not by the rule as such" (Dewey and Tufts [1908] 1976, p. 302). We cannot play chess without agreeing on rules, nor drive a car without knowing traffic signs, nor live in a community without following social conventions, yet we routinely disagree about the best chess move, the safest response to an emergency, or a just solution to a social problem, and the more complicated the situation, the more room there is for the honest difference of opinion. This goes not only for common folks unschooled in hermeneutics but also for well-seasoned experts (think about split decisions handed down by the U.S. Supreme Court). Being at odds with oneself, being of two (or more) minds on a given issue, is a distinctly human and imminently rational sentiment. We call it "ambivalence," and we find it especially handy in dealing with the muddled situations that surround us on all fronts, most signally moral situations, which rarely submit to general principles. "Every moral situation is a unique situation," pragmatists contend (Dewey [1920] 1950*a*, pp. 132–33): "The primary significance of the unique and morally ultimate character of the concrete situation is to transfer the weight and burden of morality to intelligence." The key word here is "intelligence"—the pragmatist name for reason firmly embedded in a concrete situation, fully in touch with its feelings, and mindful of the uncertainties and risks involved. Such embodied reason has a modern temper that befits democracy, and the "gospel of uncertainty" (Kloppenberg 1986, p. 413) it brings into the project of modernity offers an important corrective to the emancipatory agenda championed by classical and contemporary critical theory.

Transcendental Democracy versus Democratic Transcendence

Although the index for Habermas's monumental study does not mention the term, TCA is very much a treatise on democracy or rather a critical examination of its perversion in modern society. According to Habermas, whose views on the subject go back to 1962 and his *Habilitationsschrift* (1989), the capitalist welfare state subverts the substance of the democratic process through bureaucratic procedures and mediatized communications which, on the one hand, bring more people into the public sphere than any other political system, but on the other, emasculate that sphere by whittling down its participatory substance. Late capitalist society stifles "the possibilities for spontaneous opinion formation and discursive will-formation through a segmentation of the voter's role, through the competition of leadership elites, through vertical opinion formation in bureaucratically encrusted party apparatuses, through autonomized parliamentary bodies, through powerful communication networks, and the like" (Habermas 1987*b*, p. 365). Communicative action is bound to be distorted under these quasi-democratic conditions, and a manufactured consensus is likely to be false as long as capitalist market imperatives constrict various social strata's access to and participation in public affairs. Characteristically, Habermas does not cite the incurably formalistic logic of rational administration that critical theorists have singled out as the culprit. The gist of the problem, for Habermas, is the disuse, misuse, and abuse that the public sphere has fallen into under certain historical conditions. He believes that these conditions can be rectified and ameliorated through critical inquiry into our communicative practices. Such inquiry falls within the domain of "transcendental hermeneutics" or "transcendental pragmatics" (Habermas 1979, p. 23), which offer "a reconstructive analysis oriented to general and unavoidable presuppositions" and reveal "structures of mutual understanding that are found in the intuitive knowledge of competent members of modern societies" (Habermas 1987*b*, p. 383). Now, the crucial point in the whole argument is that the a priori conditions for reaching understanding explicated by transcendental (or universal, as Habermas now prefers to call it) pragmatics are fundamentally the same as the conditions for achieving a democratic society. This is already evident in the rational procedures guiding communicative action. These procedures, encoded in the ideal speech situation, can be read as prescriptions for substantive democracy or "*democratic form* of decision-making, namely: rationalizing decisions in such a way that they can be made dependent on a consensus arrived at through discussions free from domination" (Habermas 1970).[9] Notice that substantively rational decisions cannot depend on opinion polling, electioneering, and vote counting because such mechanisms of formal representation transfer individual's discursive rights to others and succeed chiefly in "a cleansing of political participation from any participatory content" (Habermas 1987*b*, p. 350). Formally democratic decisions and collective actions based on them are marred by nondiscursiveness and thus are communicatively flawed, which is why "majority decisions are held [in TCA] to be only a substitute for the uncompelled consensus that would finally result if discussion did not always have to be broken off owing to the need of a decision" (Habermas 1970, p. 7). A communicatively sound social order must be based on a rationally motivated, freely achieved universal consensus, which simultaneously satisfies the transcendental conditions for successful communicative action and for a genuinely democratic society.

There is much in the above argument that dovetails with, if not derives from, pragmatism. Pragmatists acknowledge that modern societies produce systemic consequences and have functional implications detrimental to the participatory ideal. Long before Habermas they thought that "discontent with democracy as it operates under conditions of exploitation by special interests has justification" and warned that "the functional aspect is contradictory to the ends of democracy" as long as "there is the opportunity for exploitation of the individual" (Dewey 1946, p. 133; Mead 1934, pp. 288–89). Since the onset of the Progressive Era, pragmatists stressed that "no government by experts in which the masses do not have the chance to inform the experts as to their needs can be anything but an oligarchy managed in the interests of the few" (Dewey [1927] 1954, p. 208). Habermas's vision of a communicatively sound social order is also adumbrated in pragmatism, which treats democracy as a form of communication, "a name for a free and enriching communion" (Dewey 1954, p. 184), and trusts humans to resolve their differences by discovering common goods. "Reason is then a medium within which values may be brought into comparisons with each other, in abstraction from the situations," reads a particularly Habermasian passage in Mead ([1930] 1964, p. 406), "and within this impartial medium it becomes possible to reconstruct values and our conduct growing out of them." In spite of these instructive continuities, there are several points on which Habermas and pragmatists diverge.

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[9] The term *substantive democracy* is used here in a broad sense that is consistent with Habermas's vision of a communicatively sound democratic system that is rich in participatory content and, as such, contrasts with the notion of formal democracy, in which individuals are deprived of a meaningful opportunity to engage in communicative action.

Pragmatists are not satisifed with discursive elucidation of common values and universal principles; "the universe of discourse," they are likely to stress (Mead, n.d., *b*8, *f*8), "is rather the universe of intercourse"—the latter brings to a test the precepts furnished by the former. "The ethical problem is always a specific one," says Mead (1964, p. 405), "the problem itself defines the values." "Especially does the pragmatist deny that the solution of our problems can be found in any vision given on the mount or prearranged order of society" (Mead, n.d., Addenda, *b*3, *f*7). Universal pragmatics does not exactly envision a prearranged order, but its procedural strictures are too general, too far removed from messy things out there to help us navigate in the world of uncertainty. The theory of communicative action goads us toward consensus and demands the unequivocal commitment to certain policies even before we have a chance to find out whether they pass muster in life. Only immersion into the practical world, with all its hazards, confusions, and unforeseen developments, can clarify what a given consensus means, which reality it engenders. Habermas (1973, p. 19) misses this point when he disparages the pragmatist rush to action and praises the virtues of discursive situations "which transcend the compulsions of action." Pragmatists do not deny reason its proper rights—they want to subject its pronouncements to pragmatic test. Truth is a practically accomplished unity of knowledge and reality, and that means tinkering with things and events to make sure they match our theoretical calculations. In the process, we are certain to run into unforeseen circumstances, unanticipated consequences, and unyielding particulars, which need to be transcended in situ and not just *in theoria*. As chaos theory tells us, minor changes in one variable can have vast ramifications for a system as a whole. This applies to human societies as well, which evolve patterned ways of handling indeterminacy.

If society is a semiordered chaos routinely generating unanticipated consequences, as pragmatism implies, then democracy is a historically specific mode of managing uncertainty. "Democracy expects the unexpected" (Betz 1974, p. 216) and "recognizes that uncertainty is inevitable and then turns it to positive account" (Dewey and Childs 1933, p. 309; see also Przeworski 1986). Democratic systems thrive on uncertainty; they rely on market, competition, ad hocing, and muddling through as necessary, even if distortion-prone, mechanisms for handling a large number of incalculable variables. By the same token, democratic polities promote conflicting life-forms, open up public discourse for an ever-widening range of participants, and maximize the public's role in defining the terms in which indeterminacy can be legitimately terminated. Contrariwise, nondemocratic polities seek to expunge uncertainty through exhaustive planning, centralized control over terminological practices, and punitive actions against dissenters who favor alternative terminologies. The less democratic the system, the more it fears discord and values consensus and the more likely it is to favor a monopoly on the terminological means of production of social reality as objective and meaningful. This is why pragmatists do not accept consensus, however discursively achieved, as the highest democratic value. The pluralistic universe presupposed by pragmatist thought precludes any one standpoint from being anointed as unassailably true, good, and authentic. If any claim merits such honorific title, it is the agreement to disagree, without which a democratic process is indeed unthinkable. Democratic institutions give extra room for the honest difference of opinion, maximize opportunities for dissent, not just for consensus, and protect minorities from aspersions the majority is apt to cast on their rationality. Dissenting insights may be rejected by the community, and for good reasons, yet they are to be safeguarded because they hint at the unrealized potentialities of being.

To be sure, democratic societies fall short of their professed ideal of the unlimited access to public discourse and allow assorted elites an undue influence over public affairs, but this is a poor reason to discount nondiscursive means in politics. When we vote, select representatives, delegate authority to experts, and vest power in the executive branch, we admittedly move away from democratic discursiveness and thereby open the door for the kind of distortions Habermas so eloquently decries. But without these formal means we could not break the discursive impasse or react efficiently to situations that require prompt action. If we take discourse ethics seriously, we should keep on arguing until a universal consensus has emerged, lest our good faith efforts are put into question. Yet such demands are unrealistic. They are certain to run afoul the hung jury predicament and founder on the kind of problems Rousseau faced when he tried to reconcile *volonte general* and *volonte de tous* in his proposal for direct democracy (see Schumpeter 1950, pp. 235–68; Van den Berg 1990, p. 163). From the pragmatist standpoint, the fact that participants resort to a show of hands and settle for a less than universal consensus is no affront to reason. Majority decision serves as a democratic, if formal, device for reaching a working consensus about conflicting rationalities vying for practical validation. To test a rationality means not only going beyond communication but also assuming responsibility for our action—not just discursive action but also a joint act, which has practical consequences and which presupposes a different type of ethics than the ethics of discourse.

The pragmatic ethics (I shall call it the "ethics of uncertainty") urge close attention to the "correlation between the means used and the consequences that follow" (Dewey 1946, p. 138). Attention to consequences produced by our conduct is mandatory because different lines of action incur varied risks and beget unpredictable, irreversible outcomes. Rational people disagreed as to whether we should have relied on economic sanctions to force Saddam Hussein from Kuwait, but once the decision was made to forge ahead with the military option, consequences befell the innocent and the guilty alike. The Lithuanian government's bid for independence was well grounded in "good reasons," yet it produced a bloody backlash few people were able to foresee. No matter how discursively validated a policy is, responsibility for the consequences should be borne by the individual. The ethics of democracy are the ethics of responsibility, and, as such, they contrast with the ethics of good faith and ultimate ends, which seek to suppress uncertainty and narrow the scope for individual judgment.

It would be unfair to say that Habermas somehow endorses the ethics of ultimate ends—his writings on German politics belie any such accusation. Nor does he subscribe to the utopian vision of Jacobin democracy that imposes its will on the unwilling subjects. And yet in its implications, Habermas's ethics are not free from some of the difficulties faced by the moral systems based on good faith and end-rational grounding. Discursive ethics plead for a domination-free life, forswear force other than the force of reason, and aim at substantive democracy, yet the results are likely to be ironic. Reason cannot escape domination as long as it seeks to impose on the world an overarching rationality in the face of the ample evidence that things themselves do not suffer theory gladly and are sure to spoil our best-faith efforts. A consensus compelled by no other force than that of good reasons is still a forced consensus if it chains the individual to a predetermined rationale, situational contingencies notwithstanding. And transcendental democracy is likely to remain a utopian trap if it does not make room for personal responsibility. By digging the communicative foundations of a rational social order, Habermas gave the critical-theoretic program a much-needed lift, yet his communicatively grounded reason still needs to be enlightened to fulfil its emancipatory promise. It needs to be guarded against its own intolerance and maximalism. It needs ambivalence, common sense, compassion—the virtues of intelligence that pragmatists consider central to democratic transcendence and sane existence in the world of uncertainty.

Rational Society versus Sane Community

Habermas shares with classical critical theory its predilection for "democratization, decentralization and socialist positions," yet his agenda is different from the one implicit in the Frankfurt school, for he respects liberalism, appeals directly to the public, and "demands a *remoralization* of politics" (Habermas 1986, p. 71). All systemic distortions, according to TCA, are prefigured in the communicative domain. If social organization has turned oppressive and politics have collapsed into administration, it is because our transactions have grown communicatively irrational. With the community as a whole losing control over society's steering mechanisms, special interests seize the opportunity to assert their particularistic rationalities under various ideological covers. Repackaged for mass consumption, these (ir)rationalities are translated back onto the individual plane. Here, through the mechanisms of consumption and socialization, they are bred into actors' bones, producing distorted needs and mentalities that, in turn, help reproduce impersonal bureaucracies and oppressive institutions. The system has uncoupled itself from the private sphere; it has stripped humans of their dignity, usurped their autonomy, perverted their needs—it has colonized the life world. To reclaim control over the system, the community must cut bureaucracy down to size, symbolically as well as literally, and recreate the conditions somewhat akin to the intellectual salons of the Enlightenment, where men and women gathered to make sense together and to furnish intellectual insights that would later be felt throughout society. The task for our time is to open the political forum to the public at large, refocus attention on communicative action, and radically upgrade the quality of the processes aimed at reaching understanding: "The reevaluation of the particular, the natural, the provincial, of social spaces that are small enough to be familiar, of decentralized forms of commerce and differentiated public sphere—all this is meant to foster the revitalization of possibilities for expression and communication that have been buried alive" (Habermas 1987*b*, p. 395). The communicative sphere must be freed from distortions, and that means taking seriously our assertions about facts, becoming reflexive about the normative bonds that we forge through our performative actions, making a personal commitment to be sincere. We have to learn to speak to ourselves and others in the voice of reason. Herein lies hope for "the possibility of settling our disagreements by adducing reasons," of releasing the "emancipatory potential built into communication structures themselves" and achieving a communicatively "rational society" (Habermas 1987*b*, pp. 74, 390).

Once again, we can see how well Habermas's "communicative socialism" (O'Neill 1985, p. 59) fits in with the pragmatist agenda and how much his specific program veers away from it. Pragmatists agree with Habermas that bureaucratized social systems should be scaled down and made accountable to the public. "Democracy must begin at home, and its home is the neighborly community" (Dewey 1954, p. 213). Like Habermas, pragmatists believe in the "passing of functions which are supposed to inhere in the government into activities that belong to the community" (Mead 1899, p. 369). "The most concrete and fully realized society is not that which is presented in institutions as such," contends Mead ([1915] 1964, pp. 166–67), "but [in] the readjustments of personal interests that have come into conflict and which take place outside of court, in the change of social attitude that is not dependent upon an act of legislature." But look at the values pragmatists praise in the communicatively shaped order: "The community values of friendship, of passion, of parenthood, of amusement, of beauty, of social solidarity in its unnumbered forms" (Mead 1964, p. 311). This is not exactly the list you find in TCA. The two perspectives share broad objectives but differ in significant details.

Habermas wants to clear communications from inarticulate sentiments, private interests, logical inconsistencies, and similar distortions as inimical to reason. Pragmatists find these essential to keeping one's sanity amidst the semichaotic order that surrounds us in everyday life. Pure reason has always looked with suspicion at passion and sentiment, but it has never succeeded in purging itself from their invidious touch. Reason has shown itself to be intolerant of ambiguity, contemptuous of common sense, disdainful of compromise, proud of its intellectual machismo in dealing with particulars, and arrogantly dismissive of its own blunderings in the practical domain. History is filled with records of human enterprises bearing reason's seal of approval and stoking nothing but bitter ironies: revolutions that abuse human rights in the name of humanity; laissez-faire liberalism that spawns monopolies under the banner of free trade; centralized economies that excel in producing shortages under the aegis of the plan; welfare programs that create a permanent underclass under the pretext of giving the underprivileged a fair chance—the list goes on and on. Habermas (1987*a*, p. 310) has a point when he sees the problem "not as an excess but as a deficit of rationality," but then he may be too kind to pure reason. He is certainly a bit disingenuous in his critique of "Western 'logocentrism'" given his own failure to acknowledge intelligence native to instinct and common sense. Deracinated affect is a dangerous thing, but reason that plugs its ears to elude the siren voices of sentiment runs equally great risks. Cultures that have mindlessly entrusted themselves to the guidance of pure reason and "undercut instinct, common sense, and the reasonableness of sentiment" have insured their own "imminent extinction at the hands of unhinged reason" (Rockberg-Halton 1986, p. 144). "Motivation through 'good reasons'" (Habermas 1979, p. 200) does not forestall the emergence of the bureaucratic "megamachine" that devours its creators (Mumford 1967). Pushing body, instinct, and sentiment to the life world's periphery does not make culture more humane (Alexander 1987). And as Dewey acutely sensed, abstract thought that shuns the senses and ignores the ordinary betrays its insensitivity in practical affairs.

> Men who devote themselves to thinking are likely to be unusually unthinking in some respects, as for example in immediate personal relationships. A man to whom exact scholarship is an absorbing pursuit may be more than ordinarily vague in ordinary matters. Humility and impartiality may be shown in a specialized field, and pettiness and arrogance in dealing with other persons. . . . "Reason" as a noun signifies a happy cooperation of a multitude of dispositions, such as sympathy, curiosity, cooperation, exploration, experimentation, frankness, pursuit—to follow things through—circumspection, to look about at the context, etc., etc. [Dewey 1950*b*, pp. 198, 196]

To guard against its own excesses, reason must be enlightened by feeling, edified by emotions, ennobled by desire. Above all, it must be sensitized to objective uncertainty. Coming to grips with the nondiscursive element in our experience not only helps safeguard reason from overindulging in abstractions but also opens the door to creativity and social reconstruction. Habermas appears to overlook this point when he commits himself to "the cognitivist position" that social problems can be solved within the domain of *Verständigung,* strictly "by way of argumentation" (Habermas 1984, p. 19). For their part, pragmatists accentuate the role of intuitive impulses and appeal to artistic imagination as powerful tools for breaking the routines of experience, smashing barriers that separate groups, and generating fresh insights into troublesome social issues (Dewey 1958*b*). "To the degree that we make the community in which we live different we all have what is essential to genius," explains Mead (1934, p. 218). Social creativity involves "those values which are found in the immediate attitude of the artist, the inventor, the scientist in his discovery, in general in the action of the 'I' which cannot be calculated and which involves a reconstruction of society, and so of the 'me' which belongs to this society" (Mead 1934, p. 214). Social change is predicated here not just on the linguistically mediated "me" as it makes an appearance in discursive communication but on the instinctive, aesthetic, unpremeditated "I" that bursts forth on the social scene and makes individual experience valuable to the community as a whole.

Seen from this pragmatist angle, values appear inseparable from habit, instinct, and behavior; they are "valuations, habitualized acts of judgment rather than simply inert norms" (Rochberg-Halton 1986, p. 16). Social norms have to find their way into mind's noncognitive recesses and become suffused with emotions, transformed into habits, translated into routine judgments: "No social modification, slight or revolutionary can endure except as it enters into the action of a people through their desires and purposes" (Dewey and Childs 1933, p. 318). Humane community is first and foremost an attitude shared by its members, a feeling like empathy or solidarity generalized to a point where it can inform social routines. Such nondiscursive communications endemic to communal being are not to be equated with diminished rationality. "Another meaning of 'rational' is, in fact, available. In this sense, the word means something like 'sane' or 'reasonable' rather than 'methodical.' It names a set of moral virtues: tolerance, respect for the opinion of those around one, willingness to listen, reliance on persuasion rather than force" (Rorty 1987, p. 40).[10] The last point hints at broadening communicative action to include rhetoric and suasion. Communicative, or rather communal, actions need not be a zero-sum game, in which my being in the right means you are in the wrong. Communal living requires tolerance to contradictions, a state that TCA proscribes as "a sign of a more irrational conduct of life" (Habermas 1984, p. 61). Inconstancy and paradoxicality are endemic to the pluralistic universe, to the "big, buzzing confusion" that James discovered at the core of our being. This universe is composed of many verses and is shot through with competing perspectives. It allows reason to be scattered across disparate social niches; it makes it appear under jarring sexual, racial, ethnic, religious, cultural, and social guises; it does not demand that various life-forms be brought to a common denominator other than their proponents' commitment to coexist peacefully, respect each other's uniqueness, and, where possible, draw on experience accumulated by others. As such, the pluralistic universe serves as the epitome of modernity pragmatically understood.

The pragmatist outlook on modernity is closer to Simmel than to Weber in that pragmatists find modernity distinguished by the expansion of the meaningful domain rather than its contraction, the unfettering of reason rather than its encagement, the revitalization of the life world rather than its disenchantment. The pluralistic universe does present the modern individual with the mind-boggling question of how to wade through jangling possibilities and keep one's sanity intact, yet pragmatists see this situation less as a threat than a promise, insofar as it makes for a more meaningful life—as in *life full of meaning* (James 1956, pp. 184–215). Today's pragmatists feel no compulsion to transform this semirational/semiabsurd world into a unified, logical, communicatively purified, perfectly transparent block universe. To deal with modern life's chaotic cross-currents, they cultivate "irony," aim for "a de-theoreticized sense of community," and "take seriously Dewey's suggestion that the way to reenchant the world, to bring back what religion gave our forefathers, is to stick to the concrete" (Rorty 1985, p. 173).

This modest program has several practical implications. For one thing, it suggests that not every evil and irrationality can be communicatively exorcised—some are endemic to human conditions and are best dealt with through joint narrative, communal grieving, shared muteness, bracketing, and glossing over. The pragmatist stance implies that universalizing the feeling of empathy and compassion is at least as important for sane existence as staking and redeeming validity claims. It calls for irony, humor, and ambivalence in handling many an absurdity of everyday life (Rorty [1989, p. 61] has a point when he calls Habermas a liberal without irony, for just as any other virtue, earnestness can be carried too far). Pragmatism also has a clear political dimension. It has been historically aligned with progressive reforms aimed at systemic distortions that limit access to public discourse, the most insidious among these distortions being economic deprivation (Faris 1970; Deegan and Burger 1978; Diner 1980; Shalin 1988). At the same time, politics in the pragmatist key is rather ideologically atonal. Laissez-faire market, nationalized economy, industrial growth, entitlement programs—these are but means to make our communal being more reasonable and sane, and if the results prove to be other than expected, pragmatists do not hesitate to acknowledge as much and to try other means. Critics have variously spurned this stance as conservative, radical, or opportunistic, but it defies any partisan label.

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[10] The pragmatist argument presented here overlaps with Gadamer's (1977) critique of Habermas for his indifference to the constructive, creative role that tradition plays in furthering communal values and living.

Above all, pragmatists call for personal efforts in one's immediate community. In this respect, they follow Chekhov's counsel to avoid grandstanding and take up small deeds. That is to say, pragmatism challenges us to start with ourselves, become reasonable with those closest to us, get out to a town meeting, PTA gathering, neighborhood association, and try to body forth a better community by talking, humoring, and cajoling its members into more reasonable ways. Once our efforts are met with success in our own abode, they are likely to be noticed and to fire up action elsewhere. As the Progressive Era pragmatist reformers had learned, social reconstruction starts in one community, envelops the city, moves to the state level, and then comes to the national legislature. The scheme does not fit each case and every country alike, but it suggests the kind of pragmatic, grass-root politics essential to democratic reconstruction. As long as we are willing to exert ourselves on behalf of our own community, pragmatists urge, we make the burden of living more bearable for all and keep alive the hope for emancipation through reason that critical theory has clung to since Rousseau.

CONCLUSION

In fairness to Habermas, it needs to be said that he foresees some of the objections raised in this essay. On several occasions, most copiously in his interviews, he intimated that he personally feels no urge to "bring a satisfying order to chaos," that "there is nothing at all to which I have an unambivalent attitude," that rational society must be "as fallibilist and as open to self-correction as possible," that "every intervention in complex social structures has such unforeseeable consequences that processes of reform can only be defended as scrupulous processes of trial and error, under the careful control of those who have to bear their consequences" (Habermas 1986, pp. 126, 144, 187). Such statements qualify only as disclaimers, however, unless they are translated into theoretical terms. In its present form, TCA provides no conceptual room for indeterminacy, has little use for nondiscursiveness, fails to appreciate the critical potential of sentiment, and does not square off discursive ethics with the need for personal responsibility. This is exactly why it is so important for Habermas and pragmatists to continue the search for common ground. The cross-fertilization is likely to be beneficial for both sides.

Pragmatists can learn from Habermas how to grasp in communication-theoretic terms systemic distortions that delimit access to public discourse. The theory of communicative action offers valuable insights into the discursive bottlenecks that are created in an overloaded market system and that are prone to be exploited by particularistic interests. Habermas has put his finger on an issue that is (or should be) central to the pragmatism-inspired social inquiry: the need to bridge the gap between action-theoretic and system-theoretic languages, between micro- and macrolevel analyses of social processes. Thus TCA provides fresh food for thought on how the movement toward formal representation in democratic systems affects discursive will formation and what can be done to safeguard substantive democracy from the distortions it suffers when responsibility for public affairs is delegated to experts. Finally, Habermas's analysis should help contemporary interactionists to reclaim the critical dimension of early pragmatism and refocus their inquiry on the structural conditions that hamper access to public discourse and undermine the conditions for fruitful dissent.

Habermas and Continental critical theorists have still more to learn from pragmatists. They have yet to acknowledge that reason uncaged is reason enlightened by sentiment, sensitized to uncertainty, steeped in ambivalence, willing to come to terms with common sense, humbled by the consciousness of the limits that nature sets to its ambitions. Emancipated from its bias against indeterminacy, reason is likely to concede that a consensus based on good reasons alone is a poor guide for action and that the ideal speech situation must include among its provisions an agreement to disagree. The question that critical thinkers might want to ponder is how to make our emotions intelligent and our intellect emotionally sane. In sum, critical theory could greatly benefit from the pragmatist insights into embodied experience and objective uncertainty.

These concerns are consistent with the broadly based trends in modern science, which is increasingly turning its attention toward chaos, uncertainty, fuzzy logic, emergent processes, dissipative structures, and other patently nonclassical subjects. The time has come to reconsider pragmatist philosophy and interactionist sociology (which derived its inspiration from, though has not always been faithful to, pragmatism) with an eye to determining how both have presaged the developments in nonclassical science.[11] The future agenda includes a sociology in the pragmatist key, the one that eschews both the irrationalism of *Lebensphilosophie* and the conservative proclivities of postmodernism—a critical social science of uncertainty that combines the commitment to emancipation with the readiness to meet head-on objective uncertainty.

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[11] See Shalin (1991) on pragmatism and symbolic interactionism as humanistic counterparts of nonclassical science.

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Bank Control, Owner Control, or Organizational Dynamics: Who Controls the Large Modern Corporation?[1]

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There has been a long-standing debate over who controls the large American corporation. The results presented here indicate that existing power relations within the firm, the conception of control that dominates the firm's actions, and the actions of competitors account for the economic actions undertaken by large firms, while the differences in manager or owner or bank control and the presence of bank interlocks do not. A sociology of markets is therefore more likely to account for the courses of actions of large firms by considering these relations, rather than focusing on the construction of interests by bankers, owners, or board interlocks.

INTRODUCTION

The question of who controls the large modern corporation is one of the most enduring of modern social science. Sociologists who have studied the issue have gone in two directions: they have been interested in detecting a capitalist class and discovering the nature of its cohesion (Zeitlin 1974; Useem 1984) or they have tried to use organizational theory to account for the structure of the relations between large-scale business organizations (Burt 1983; Mintz and Schwartz 1985; White 1981; Fligstein 1990; Baker 1984, 1990). Economists have approached the question by being concerned about the possible negative effects of the separa-

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tion of ownership from control in maximizing profits (Williamson 1964, 1985; Marris 1964; Jensen and Meckling 1976; Fama and Jensen 1983*a*, 1983*b*; Holmstrom 1988; MacNeil 1985). What is at stake in this debate is our understanding of how large corporations operate and the nature of the worlds that are constructed around them.

Much of the evidence for the effects of owner versus management versus bank control and the composition of boards of directors on the financial performance of corporations is mixed or negative (Burt 1983; Herman 1981; for evidence that supports some effect see Pennings [1980]; for a review, see Scherer [1980]). These analyses have a number of problems. They use different measures of control and generally use only one measure of performance (profits) with a cross-sectional research design. Even more important, there are almost no attempts to model the effects of the various forms of control on important strategic decisions, such as product mix, mergers, and which subunit dominates firms (for an exception, see Palmer et al. [1987]).

The major empirical contribution of this article is to expand these tests in a number of ways. First, we test a number of measures of forms of control simultaneously in order to assess if any has particular advantages. Second, in addition to a cross-sectional design, we utilize a panel design to assess whether control forms affect outcomes over time. Third, we model a larger number of important organizational outcomes, thereby extending attempts to test versions of control theory to other variables. Finally, we contrast control theory with a political/cultural view that focuses on interorganizational factors as predictors of outcomes.

Our major substantive conclusion is that differential ownership and the presence of bank interlocks are not important determinants of the strategic and financial outcomes of large firms. A far more plausible model is that the actors who control corporations use what occurs in their organizations and organizational fields as cues to guide their behavior. Family owners, bank owners, managers, and bank directors behave according to the dictates of the organizational field in which the firm is embedded and the internal dynamics of their organizations.

Economic sociologists have been debating the nature of the sociological embeddedness of markets for over 10 years. Much of the work has focused on network analyses (Burt 1983; Granovetter 1985; Baker 1984), and a fair amount of that work has concentrated on using boards of directors and bank interlocks as the basis of the network data (Burt 1983; Mintz and Schwartz 1985). Our results here suggest that American firms are neither constrained nor enabled by this form of network embeddedness. Therefore, the social structure of American firms may be studied more adequately by targeting the existing distribution of power in the firm, the behavior of competitors in the organizational field (White 1981), and the relations of firms to the government (Fligstein 1990). Further, it is the substantive content of those relations (i.e., the background of chief executive officers [CEOs] and the existing conceptions of action that dominate competitors) that are the strongest predictors of any given firm's actions—not the mere existence of social relations between them.

REVIEW OF THE LITERATURE

In this section, we consider the relevant literature from three perspectives, (1) management control versus family or individual control, (2) the potential for bank control, and (3) the findings of the recent organizational literature. Much of the sociological literature has tried to establish the existence of an economic elite or capitalist class and has therefore been uninterested in differences in the economic behavior of corporations (Zeitlin 1974; Allen 1974; Useem 1983). We do not address the concern of that literature here, nor do we focus on the translation of elite interests into political action (Mizruchi 1989). Our purpose is to examine the determinants of the strategic behavior of large firms and their relation to the control debate.

Corporate control will be defined as the power to determine the broad policies guiding the corporation, including decisions regarding capital structure (such as equity, debt, profits, and cash flow), geographic expansion, product diversification, corporate structure, and mergers (see Herman [1981] for a similar definition). Our question here is whether differences in ownership patterns and bank linkages actually affect the actions that firms undertake. The review suggests why groups might have varying interests that would cause them to make strategic decisions differently.

Management versus Owner Control

Economists have been interested in the control debate ever since Berle and Means (1932) asserted that there was an increasing separation between the ownership and control of the largest firms (theoretical arguments for the relation are presented in Marris [1964] and Williamson [1964]). Their concern is the degree to which managerial discretion causes firms to fail to pursue profit maximization. The theory suggests that managers try to protect their jobs by avoiding courses of action that could potentially threaten them, whereas owners would act in a more entrepreneurial fashion. The alternative tactics pursued by managers could include settling for lower but more stable profits by aiming for growth in sales and assets over profits, acting for the public interest instead of shareholder interests, increasing their salaries and perquisites, and implementing strategies that involve low risk-taking behavior. Managers would thus be more likely than owners to diversify product lines in their efforts to spread risk. They would also use mergers for corporate growth because buying an already existing company is less risky than entering a new business.

Several theoretical advances in economic theory have altered this debate. Williamson (1985, pp. 311–22), extending a transaction-cost perspective, has argued that the governance structure of joint-stock corporations has been set up to minimize managers' opportunism. This opportunism arises from information asymmetries between boards of directors and top managers. Boards of directors act to control this by writing contracts for top managers that bond their interests to those of the shareholders. The multidivisional form is also a control mechanism that makes managerial opportunism more difficult (Williamson 1985, pp. 320–22). While Williamson thinks these devices align interests, he also argues that managerial discretion and the possibility for opportunism always exist.

Agency theorists make a similar case.[2] The basis of their analysis is the principal-agent model that emphasizes that one common feature of economic life is that individuals (principals) engage others (agents) to perform some service. The problem is that the interests of the two will not always coincide, and so the principal must establish appropriate incentives and monitor the behavior of the agent in order to ensure that the agent acts in the principal's interests (Jensen and Meckling 1976, p. 308). In the context of the large corporation, agency theory seeks to explain why the joint-stock corporation exists, how the principal-agent relation between owners and managers is structured, and how it can go astray.

Agency theorists consider the firm to be a fictitious entity. They see the firm as a governance structure made up of contracts and claims on the assets and cash flows of the organization (Jensen and Meckling 1976). The form of that governance structure will reflect the organization's special agency problems and will be selected as an efficient solution to them. Fama and Jensen (1983*b*) argue that the joint-stock corporation comes into existence in situations where technology requires a large amount of capital, there is a demand for specialized agents to utilize economies of scale, and a large pool of capital is needed to bond contracts and specific assets of the organization. As they put it, "The benefits of unrestricted common stock residual claims in activities where optimal organizations are large and complex offset the agency costs resulting from separation of decision functions and residual risk-bearing" (Fama and Jensen 1983*b*, p. 346).

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[2] It should be noted that there are a number of versions of agency theory (i.e., Ross 1978; MacNeil 1985; Holmstrom 1988). Here, we develop only enough of the general points so as to acquaint the reader with the perspective.

The principal-agent relation between managers and owners is mediated through boards of directors and financial markets. Boards function to monitor managers' decisions and are backed up by contracts that reward managers for performance. Financial markets monitor management teams by providing owners with current information about the firm's prospects (i.e., the price of the stock) and the ability to sell the stock at low cost. The market for corporate control provides the final backup to ensure that managers act in the interests of principals. If the fortune of the firm declines substantially, competing groups of managers can bid to take control (Fama and Jensen 1983*a*; Jensen and Ruback 1984).

The principal-agent relation between boards of directors and managers can go awry when boards lack the will or information to enforce discipline on managers. This will occur most frequently when stock ownership is diffused or institutional investors dominate (Jensen 1989; for an analysis of other dependent variables that use a similar argument, see Graves [1988] and Fombrun and Stanley [1990]). An oft-used measure of managerial discretion is free cash flow, which indicates whether managers are holding capital instead of distributing it to shareholders. Other firm strategies that reflect managerial control include diversification and growth in sales and assets with little change in profits. Diversification can represent a tactic to hold onto free cash flow and enter less profitable businesses (for a dissenting view, see Amit and Wernerfelt [1990]). Managerial salaries tend to be tied to firm growth and examining the determinants of variables like the ratio of profits to sales and assets would suggest whether or not managers were sacrificing profits for growth.

Bank Control

The issue of financial control over the largest firms has lurked in the social science literature since the turn of the century. The role of financiers in the merger movement of 1895–1905 is well known (Lamoreaux 1985; Mizruchi 1982). The literature on bank control has recently focused on the extent of that control and its effects on the behavior of large firms. Banks, insurance companies, mutual funds, and pension funds have all increased their holdings in large industrial firms (Herman 1981). The interesting question is whether they extended their involvement to active control over strategic decisions and if so, in what direction? Those who argue for bank control can be divided into three camps, in which they espouse bank control either through ownership, through bank hegemony achieved by their key position in the financial system, or through the potential for bank influence because of financial dependence.

The strongest advocate of direct bank control is Kotz (1978). He argues that finance-controlled firms are more likely to take on external debt than nonfinance-controlled firms and thus their debt/equity ratio should be higher. This propensity to assume debt also makes them more likely to engage in mergers than nonfinance-controlled firms. The presence of finance control throughout an entire industry which can be indexed by the degree of bank interlocks should lower competition and raise profits. Hence, finance control would lead to higher profitability for firms.

Mintz and Schwartz (1985) argue that banks exert hegemony in the industrial system by virtue of their key control of the capital markets. This structural position has two effects. First, bankers intervene in corporations under conditions of crisis and replace managements who are not providing profitable returns. Second, bankers are at the center of corporate interlocks, and can use this influence to help firms collude or cooperate.

Unfortunately, it is usually difficult to ascertain the direction of influence between bankers and internal corporate representatives. Indeed, there is evidence that bankers are invited onto the board to co-opt them into making loans to the firm (Burt 1983; for a debate see the exchange by Herman [1979, 1980] and Kotz [1980]). Bankers may also be brought onto boards of directors when firms are in financial trouble (Mizruchi and Stearns 1988). The evidence above suggests that bankers, themselves, are being used as much as they are influencing the course of the organization.

These problems have led to a third position that posits a more complex relation between banks and firms. Because bankers often play many roles, one would expect that their influence would depend on the situation and their relative power. Bankers brought onto boards because of the financial dependence of a firm would be the most likely to have influence over the firm's financial structure and choice of debt instruments (Hirsch 1989). But bankers could be brought on the board for numerous reasons, including co-optation, prestige, or social and political connections (Herman 1981). This explains why, historically, banks have been at the center of firm interlocks, but different banks have occupied these positions over time (Mizruchi 1982), and why board of director interlocks are notoriously unstable and renewed less than 15% of the time (Palmer 1983).

Organizational Dynamics and Management Control

The organizational literature has increasingly focused on organizational fields (DiMaggio 1985). It is believed that organizations' actions are shaped by the links between any given organization and its surrounding organizations. Two literatures are relevant: the external control of organizations approach (Pfeffer and Salancik 1978; Burt 1983) and what can be called a political/cultural approach (Fligstein 1985, 1987, 1990). Each has a different perspective on important mechanisms governing organizational embeddedness.

Organizations exist in organizational fields, within which they are dependent on other organizations for inputs and the consumption of outputs. The view that organizations are externally controlled suggests that actors who can make claims on delivering inputs or outputs are likely to have a disproportionate say in the internal allocation of resources (Pfeffer and Salancik 1978). Burt (1983) has used this perspective to argue that boards of directors are used by firms as mechanisms to co-opt organizations in the environment. His analysis shows that outside directors are often representatives of firms who are important customers or suppliers. The presence of bankers on boards of directors can be explained by the need to generate capital. Unfortunately, Burt was unable to show that these ties actually produce additional profits for the firm, which would seem to undermine the claim that this co-optation has positive effects.

The political/cultural approach expands on the insights suggested by institutional theory (Meyer and Rowan 1978; DiMaggio and Powell 1983) and the work of White (1981) on the structuring of markets by focusing on how the intraorganizational power struggle is structured in large organizations and how it is affected by and affects organizational fields (Perrow 1970; Fligstein 1987, 1990). This sociological view offers explicit hypotheses about the links between firms, their organizational fields, and the structuring of their behavior.

The issue for any given organization is what courses of action to pursue. Since a priori the actual effect of any given strategy may be difficult to assess, there arises the possibility that there will be conflict over the goals of the organization. There are two bases for control: ownership and authority. To the degree that owners control large-scale organizations, they will be able to direct the course of those organizations; but even ownership groups will have to formulate a view of the world in order to act.

The basic problem for organizational actors is to create a stable world so the organization will continue to exist. This necessitates the construction of an organizational field where actors take each other's actions into account in the framing of their own actions. The two problems involved in creating a stable organizational field are the necessity for understandings that are general and enforceable and the problem of legitimation of those understandings by the government. Fligstein (1990, chap. 1) calls such a set of understandings a conception of control.

One interesting question is: Where do conceptions of control come from? Since conceptions of control require a perspective on the world, these perspectives can have two sources: they may be borrowed from other organizations or organizational fields, or they can be embedded in how the different subunits of an organization conceptualize its problems and solutions. Subunit background provides actors with a way of seeing the world. A conception of control implies a filter that actors use in viewing their internal and external problems and in defining their actions to solve those problems. Effective conceptions of control will diffuse and eventually dominate and define organizational behavior in organizational fields. In this way they operate both as a cultural template for structuring new actions (i.e., what behaviors make sense) and a set of structures limiting the possibilities for action (i.e., what others are doing, thereby structuring what reactions are possible).

Intraorganizational groups gain and maintain power as a function of their ability to promote organizational survival and create stable organizational fields. From this perspective, organizational fields are more than socially constructed meanings, they are systems of power. The largest organizations generally control the fields and dictate their rules. Smaller organizations benefit from this system as long as they fall in line with the organizations setting the rules. All participants in the field come to see the world similarly and react to each other's moves from this perspective. This theoretical approach can be termed political/cultural because it focuses on the intra- and interorganizational power struggles and suggests that the goal of such struggles is to create shared meanings, that is, local cultures that operate to produce stable interactions. The approach is also political because the state can enter into and regulate actions in any given organizational field.

Today, most large American corporations are dominated by the finance conception of control that views the firm as a bundle of assets that are deployed in order to maximize short-term earnings (Fligstein 1990, chap. 6). The finance conception of control originated with executives trained in finance methods who had been primarily financial officers in their companies. Their balance-sheet approach implies that the firm is no longer in the business of producing commodities, but instead operates as a set of assets, whereby divisions of the firm that do not perform up to expectations are sold off and new ones purchased. A central tactic is the use of mergers (and divestitures), often for diversification, to achieve growth.

The finance conception of control displaced the sales and marketing conception during the 1960s and early 1970s. Sales and marketing executives favor policies that boost sales by increasing marketing, differentiating products, and diversifying product lines. The finance conception emerged in the postwar era as a function of the increasingly diversified nature of the firm and the aggressive antitrust policies of the Truman and Eisenhower administrations (Fligstein 1990, chap. 6). Subsequently, many of the largest firms have become dominated by finance personnel espousing a finance conception of control (Fligstein 1987). Their claim to expertise is based on their ability to evaluate the profitability of different product lines and hence manage the large firm as an investment portfolio. Their claim to power is derived from the relative success of such a point of view in generating growth in the firm. One of the key hypotheses that this implies is that those acting with a finance conception are able to produce growth in sales, assets, or profits, mainly through mergers and diversification.

During the merger movement of the 1960s, two of every five firms that had assets of more than $10 million were merger targets (Fligstein 1990, chap. 7). If the field of a firm was invaded by firms using the finance conception of control, then that firm had to either adopt a similar view or risk becoming a target for merger. In this way, the organizational fields of many of the largest corporations were reorganized by the finance conception of the corporation.

HYPOTHESES

The critical issue in these literatures is how the agents of control affect the actions of the largest corporations. The political/cultural approach stresses the current structure of the situation, both internal and external to the organization, as the wellspring of action. The owner/management/bank debate suggests that the goals of specific interest groups, as owners and creditors, will differ and cause the organization to act differently, net of its internal configuration and its links to its field.

The dependent variables studied here are of two types: financial and strategic. Financial outcomes are to some extent strategic choices. If firms have cash surpluses, then they must decide how much to retain or disburse to shareholders. Firms might retain cash flow because they want to invest in new projects, or use it as a cushion against unforeseen events, or managers might hold excessive cash in reserve for opportunistic reasons. Three other types of strategic decisions are modeled: whether firms undertake mergers, whether firms have diversified their products, and who gets to be the corporate CEO. These actions reflect strategic policies of the organization. The use of mergers is an important decision regarding the allocation of capital. The decision to diversify the product mix reflects the actual definition of the organization in terms of its goals. The background of the incumbent CEO is an indication of which organizational subunit is responsible for determining firm strategy (Pfeffer 1981).

The management-control literature, as opposed to the owner-control literature, predicts that manager-controlled firms are more likely to increase equity over debt, decrease the ratios of returns to assets and sales, have relatively low returns on equity, relatively high price/earnings ratios (because of low payouts), and relatively high levels of free cash flow. The return on assets and return on sales variables reflect how much managers are trading off earnings for assets or sales. Manager-controlled firms also favor mergers and diversification. All of these actions reflect management's propensity to retain or reinvest earnings rather than disburse them to shareholders.

There are a number of indicators of management control, including a low percentage of stock held by a single family, individual, or bank, or a high percentage of stock held by institutional investors, a gauge of shareholder passivity (Jensen 1989). Product diversification is another indicator of management control as managers tend to diversify rather than disburse earnings. From the transaction-costs perspective, the presence of a multidivisional form suggests that managers are more constrained in their actions.

Bank control is indexed by the percentage of voting shares held by the largest bank and the number of bank interlocks on the board of directors. The bank-control literature predicts that firms controlled by banks are more likely to pursue merger strategies in order to increase the banks' power. They should also be more likely to have higher returns on equity and higher debt/equity ratios. The predictions of the financial hegemony perspective are indeterminate, although interlocks could be associated with higher returns on equity and higher debt/equity ratios. The financial dependence perspective suggests that the presence of bank control would make firms more likely to engage in mergers and to have high debt/equity ratios. Network views that see boards of directors as co-optive devices to lessen resource dependence would predict that bankers on the board and bank interlocks should enhance financial performance.

The political/cultural perspective stresses three types of factors that govern the financial and strategic choices of firms: the behavior of competitors who form an organizational field, defined here in terms of the other large firms that occupy the same industry (specified at the two-digit Standard Industrial Classification [SIC] level), the already existing strategies and structures in place, and which subunit controls the organization. The major hypotheses of the political/cultural perspective focus on how much an organization and its organizational field are dominated by the finance conception of control. If other firms in the same field are diversifying or merging, indicating the dominance of the finance conception of control, a firm is more likely to engage in those activities also.

The existing strategy of the firm is a powerful determinant of many outcomes, including growth and mergers, and reflects entrenched systems of power. Finance-controlled firms will be diversified and should be more likely to engage in mergers and show higher growth rates and returns than nondiversified firms. This prediction runs counter to that of agency theory, which views diversification as lowering returns on assets and sales. The multidivisional form will be the favorite structural vehicle for finance-oriented diversification and mergers, and its presence should positively affect growth in sales, assets, and profits.

The subunit that controls the CEO of the firm also operates as an indicator of the conception of control that dominates the firm (Fligstein 1987). Sales and marketing and finance CEOs will be more likely to lead firms that are diversified than will manufacturing CEOs; they will also be more likely to engage in mergers. It is probable that sales and marketing CEOs will lead firms with lower returns on assets and sales, reflecting their preference for sales growth over asset and profit growth. They will also be less likely to engage in product-unrelated diversification and more likely to engage in product-related diversification. Finance CEOs are expected to be adept at producing profits for all measures, including returns on assets, sales, and equity.

The growth of large firms will be determined, to some degree, by the rise or fall of the firm's specific industry. If the industry is expanding, then we expect sales, assets, and profits of a given firm to increase. This is not, of course, an effect of the social organization of a given field, but a purely economic variable.

It should be noted that a number of our variables index items for multiple theories but that the theories predict different effects (e.g., the measures of diversification). Additionally, agency theory and transaction-cost theory are probably impossible to falsify. If it turns out that management-controlled firms do not use managerial discretion to lower profits, that implies that the mechanisms to control managerial opportunism work. On the other hand, if evidence exists that managers do not favor profit maximization, then the mechanisms have failed. Therefore, the theory cannot be proved incorrect and can only be used to suggest if opportunism exists.

DATA AND METHODS

The selection of a data set and a time frame is hampered by a number of problems, largely stemming from the character of the existing measures of control. First, many of the available data that attempt to measure control were collected at different time points ranging from the mid-1960s to the early 1970s. Second, almost all of the control measures have focused on the largest industrial corporations. This is understandable, given their important role in the U.S. economy. However it means that there are no data on control forms for a large number of firms in any given industry.

Given these limitations, we focus here on the 100 largest industrial corporations in the United States in 1969 and the firms that entered the list of the 100 largest industrial corporations between 1969 and 1979. These data were obtained from *Fortune* magazine (1970, 1980). This design was chosen to create a panel study of large firms (in essence, the *population* of the largest). The panel design allows us to test how the initial conditions that include the control types and the organizational factors affect the behavior of the firm over a subsequent period of time. The decade-long time frame allows for the slow accumulation of strategic changes and effects of control in the largest corporations. By modeling such change over long periods, we are more likely to observe such effects. For the financial measures, 10 years stabilizes the effects of year-to-year fluctuations. We chose 1969 as the initial year because many of the control and organizational variables had available measures for this year. One problem created by this choice is that the financial data refer to 1969 and 1970, years when the economy and stock market were depressed.

The measures of forms of control are not straightforward to operationalize. Indeed, proponents have not been able to agree on how to measure them, and there is substantial disagreement on which firms fall into bank, owner, or management control. Our approach is to operationalize measures of control that might be general to all theories.[3] We measured the percentage of stock held by the largest family or individual (Herman 1981), the largest percentage of stock with voting rights held by a bank (U.S. House of Representatives 1971; U.S. Senate 1973), and the percentage of the stock held by institutional investors in 1969 (U.S. House of Representatives 1971). The number of bank interlocks through boards of directors (U.S. House of Representatives 1969) was operationalized as well. There were substantial missing data for the bank data (25%) and the interlock data (37%).

In order to maximize the number of cases in the analysis, we created two dummy variables, coding them "0" if data were missing for the case and "1" if data were available. The observations with missing data for the bank and interlock variables were then recoded "0." If there is some systematic bias in how the data on control are related to the outcomes, the dummy variables will be statistically significant. We estimated 32 separate coefficients for these dummy variables and only two are statistically significant. By chance, one would expect one or two of the coefficients to have statistical significance related to the outcomes and, therefore, we conclude that selectivity of missing data on these two variables is not a serious problem.

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[3] The various measures of control proposed by Herman (1981), Larner (1971), Burch (1972), and Kotz (1978) were operationalized as dummy variables in unreported analyses. These analyses provide results that are very similar to the results presented here: i.e., a lack of effect for the control measures. Since each author bases these measures on very different criteria, we thought that using the actual percentages of the stock controlled by various groups was a more effective measure of the general control concept.

The measures of forms of control suffer from two kinds of problems. First, they may or may not tap into control. Since these are our only measures of stock-ownership patterns, we must accept them as face valid. Second, we only measure control at one point in time. Because we use the control variable to predict change over a substantial period (10 years), control may have shifted over the period. We know little about the stability of the control variables because they have not been systematically measured over time. In order to maximize the possibility of discovering control effects, we have run the models for each of the dependent variables at the first time point (i.e., the cross section) as well as looked for effects of control over time. It should be noted that most of the organizational variables have also been measured only at the first time point, and they may have similar problems. One advantage in this is that there is no confusion of the causal order of important relationships. Since the exogenous variables are measured at the first time point, they are clearly causes of outcomes during the decade.

We used a large number of dependent variables in this analysis. First, we analyzed free cash flow, market value, price/earnings ratio, return on assets, after-tax profit margin (return on sales), return on equity, and the debt/equity ratio in 1970 (Compustat 1989).[4] These variables were taken from corporate reports and 10-K forms and coded by Compustat. Free cash flow could not be adequately constructed by its standard definition for the first time period (1970) because of the unavailability of requisite data. Consequently, we approximated it by subtracting current liabilities from current assets for both time periods. The calculations for all variables appear in the Appendix. We also analyzed the 1980 levels of these variables plus the 1980 value of assets and sales. Dollar amounts were adjusted to 1967 levels. In the panel analysis we included the level of each of the dependent variables at the first point in time as a regressor.[5]

[4] The years 1970 and 1980 were chosen because many firms begin and end their fiscal years at different times. Therefore, the data may include part of the years 1969 and 1979 as well as those of 1970 and 1980. If 1969 had been selected as the year to predict results, then some of the data would refer to 1968, and this might have created problems of time ordering.

[5] Financial reporting can be uneven. Different firms can calculate these measures in different ways and so we must be cautious in accepting the definitions of the accounting terms across all organizations and time. Of course, we hope that such variations are random.

Corporate strategy was coded into three categories: product dominant, product related, and product unrelated. We used Rumelt (1974) and *Moody's Manuals* (1970, 1980) to code these categories. Product dominant implies that firms produce more than 70% of their revenues in a single product line. Product related implies that firms produce multiple products that were related or market extensions (e.g., chemical companies that produced paint and explosives), with no single line accounting for more than 70% of output. Product-unrelated strategies imply that firms depend on unrelated businesses for a substantial portion of their revenue (no product could account for more than 70% of revenue). Two dummy variables were coded indicating whether a firm had a product-related strategy (coded "1" on one of the dummy variables) or a product-unrelated strategy (coded "1" on the other dummy variable). They refer to 1969.

Corporate structure refers to the existence of a multidivisional form. A dummy variable was coded "1" if a firm had a multidivisional form as its structure in 1969. These data were coded from Rumelt (1974) and *Moody's Manuals* (1970).

Mergers were coded using the Federal Trade Commission's *Report on Mergers and Acquisitions* (1981). The total number of mergers from 1969 to 1979 was summed for each firm. On examination of the variable, we found that a distinction between firms who engaged in mergers and those who did not best captured the skewed distribution. Therefore, this variable is coded "0" if firms did not engage in mergers and "1" if they did. The variable indicating firms' previous merger behavior is the total number of mergers engaged in during 1959–69. This variable did not require dichotomization.

The subunit background of the corporate chief executive officer was coded for 1969 and 1979. Once the name of the CEO was collected from *Moody's Manuals,* we pursued it in *Who's Who in America, Who's Who in Finance and Industry,* and other sources. From the description of each CEO's career, a decision was made, on the basis of previous job titles, as to how the person came up through the firm. If the person switched firms, the subunit background was decided on the basis of job titles across the career. The following categories were coded: manufacturing, sales and marketing, finance, entrepreneur, lawyer, general management, other, and "unable to ascertain." The general management category reflects whether a person held different titles in different parts of the firm (e.g., plant manager and vice-president in charge of finance). In the data analysis presented here, we have collapsed the measure into three dummy variables indicating whether the CEO was a manufacturing person, a finance person, or a sales and marketing person. The omitted category is all others.

The organizational field variables all refer to the year 1969 and were based on the characteristics of the major industry of the firm (two-digit SIC). The change in the growth in assets from 1969 to 1979 in the industry was used as a measure of industry growth, with the change in assets for each firm in the analysis subtracted from the industry's change in order to remove any spurious relation between the measures. This variable serves as a rough measure of increase in demand. The percentages of other large firms who had product-related and product-unrelated strategies in the industry were coded to predict whether a given firm also had a product-related or unrelated strategy. Similarly the percentages of the other large firms in the industry whose CEOs had been in finance, sales and marketing, and manufacturing in 1969 was used to predict the CEO in 1979.

These measures have a number of potential problems. They assume that the relevant organizational field is composed of direct competitors, which are also large firms. If these measures do predict outcomes, then we can assert they have predictive validity. It could be argued that these measures tap into what is economically efficient for firms of a given type and that any association is merely due to the correlation between what is efficient and what others are doing. This criticism is less easy to answer and implies that there is a need for caution in accepting the organizational field interpretation of these variables.

We used two techniques to analyze the data: a least squares regression was used to analyze the level and change in the financial measures; logit analysis was used to determine whether or not a firm engaged in a merger, to analyze whether a firm had a product-dominant, product-related, or product-unrelated strategy (product dominant was the left-out category), and to predict the background of the CEO in 1979 ("other" was the left-out category). This procedure is appropriate because these are qualitative dependent variables. All calculations were performed using SAS.

RESULTS

We will discuss the hypotheses by first examining the effects of the control, industry, and organizational variables on the financial performance measures in 1970. Then we will discuss the results for the decade 1970–80. Finally, we will consider the determinants of the organizational variables. The means and standard deviations for the variables used in the analyses are presented in table 1.

Table 2 contains the results for the regression analyses of the various measures of financial performance in 1970. Our models do not explain a lot of the variation in financial performance in 1970, and the measures

TABLE 1

MEANS AND STANDARD DEVIATIONS FOR VARIABLES USED IN THE ANALYSIS (N = 120)

Variable Label	Description	Mean	SD
MERGER1	Merger activity, 1959–69	1.69	2.17
MERGER2	Merger activity, 1969–79	.56	.76
INDGROW	Growth of industry	4.94	4.98
RELN	Product related strategy, 1969	.55	.49
UNRELN	Product unrelated strategy, 1969	.20	.40
MDF1	Multidivisional form, 1969	.73	.44
SALEPR	Sales president, 1969	.19	.39
MANPRES	Manufacturing president, 1969	.24	.42
FINPR	Finance president, 1969	.22	.41
PERREL	% of firms with related strategy	47.43	26.58
PERUNREL	% of firms with unrelated strategy	18.16	16.12
SINDPER	% of firms with sales president	17.08	8.34
MINDPER	% of firms with manufacturing president	19.26	16.90
FINDPER	% of firms with finance president	25.13	20.51
FCF80	Free cash flow, 1980	437.79	568.19
MV80	Market value, 1980	.21	.31
PE80	Price/earnings ratio, 1980	3.76	2.94
ROA80	After tax return on gross assets, 1980	2.02	1.79
PM80	After tax profit margin, 1980	2.12	1.93
ROE80	After-tax return on common equity, 1980	12.75	70.95
DE80	Debt/equity ratio, 1980	.18	1.18
ASSETS80	Assets, 1980	5,137.83	7,961.98
SALES80	Sales, 1980	4,926.26	6,699.99
FCF70	Free cash flow, 1970	403.85	454.63
MV70	Market value, 1970	2,615.13	4,701.90
PE70	Price/earnings ratio, 1970	13.57	24.60
ROA70	After tax return on gross assets, 1970	3.52	2.63
PM70	After tax profit margin, 1970	4.97	3.34
ROE70	After tax return on common equity, 1970	8.35	7.24
DE70	Debt/equity ratio, 1970	.54	.80
ASSETS70	Assets, 1970	3,987.13	6,301.19
SALES70	Sales, 1970	2,573.08	2,928.85
ILOCK	Bank interlocks, 1969	2.71	2.81
BANK	Largest bank shareholder, 1969	3.05	3.78
FAMILY	Largest family shareholder, 1970	4.95	10.46
ILOCK-D	Missing data-interlocks	.25	.43
BANK-D	Missing data-bank shareholder	.36	.48
INSTIPER	% shares held by institutions	.11	.06

NOTE.—Data sources and coding are discussed in the text.

TABLE 2

Regression Analysis of the Determinants of Various Measures of Financial Performance, 1970 (N = 109)

Independent Variables	Dependent Variables													
	FCF70		MV70		PE70		ROA70		PM70		ROE70		DE70	
	b	SE(*b*)	*b*	SE(*b*)	*b*	SE(*b*)	*b*	SE(*b*)	*b*	SE(*b*)	*b*	SE(*b*)	*b*	SE(*b*)
MERGER1	24.35	23.12	6.24	233.21	−.01	1.27	.02	.11	.12	.15	.38	.36	.01	.04
MDF1	101.85	110.49	1,602.48	1,114.43	−.03	6.07	−.14	.57	.38	.74	−1.49	1.74	.13	.19
RELN	−34.93	115.22	−681.45	1,162.16	16.14**	6.33	.92	.59	1.21	.77	3.68*	1.82	−.06	.20
UNRELN	−101.62	161.86	−2,673.03	1,632.52	17.11*	8.89	−.54	.83	−1.83*	1.08	−1.08	2.56	.70**	.29
MANPRES	−72.73	124.40	933.30	1,254.75	2.50	6.83	−.35	.64	1.79*	.83	−.58	1.97	−.10	.22
SALEPR	167.88	130.58	2,830.78*	1,317.01	−.36	7.17	.52	.67	.28	.87	1.28	2.06	.10	.23
FINPR	−16.68	128.00	200.75*	91.03	−9.06	7.03	.91*	.36	.49*	.25	−1.24	2.02	−.12	.23
INSTIPER	−756.54	872.64	−22,549.00**	8,801.20	−23.53	47.95	−10.45*	4.52	−10.15*	5.85	−6.18*	3.81	−.54	1.57
ILOCK	31.99	20.30	61.79	204.78	−2.43*	1.11	−.27*	.10	−.40**	.13	−.53*	.32	−.00	.03
BANK	−20.56	15.13	−205.41	152.66	−.41	.83	.05	.07	.04	.10	.25	.23	−.04	.02
FAMILY	8.62	5.88	−20.98	59.31	−.06	.32	.01	.03	−.00	.03	.12	.09	.00	.01
ILOCK-D	−173.02	139.13	−1,666.51	1,403.30	−13.87	7.64	−.65	.72	−1.33	.93	−.09	2.20	.03	.25
BANK-D	−316.70*	122.97	−3,493.67**	1,240.27	1.66	6.75	−.30	.63	−.57	.82	.17	1.94	−.29	.22
INTERCEPT	499.77*	217.33	6,059.23**	2,191.98	15.79	11.94	4.86**	1.12	5.96**	1.45	7.71*	3.44	.66*	.39
R^2	.08		.14		.16		.05		.16		.05		.07	

Note.—See table 1 for description of variables.
* $P > .05$.
** $P > .01$.

of forms of control fare poorly. It is useful to consider the specific hypotheses more closely. There is no evidence that bank or family shareholding affects any of the measures of financial performance. The lack of these effects suggests that, in situations where bank or family shareholding is not large, managers do not generate excesses of free cash flow, reduce the firm's market value, increase equity over debt, or favor assets and sales to profits. Further, bank ownership did not make firms more profitable (by any measure of profits) nor did it make firms more likely to favor debt over equity. Highly interlocked firms were actually less profitable in terms of returns on assets, sales, and equity, contradicting the bank-control hypothesis that firms with interlocks would be more profitable. One plausible explanation for this result is that firms in troublesome environments (those with low profitability) reduce their dependence by interlocking (Burt 1983). That there is also no effect of the multidivisional form on the financial performance of firms undermines transaction-costs arguments. This result could be because the multidivisional form has already spread to those firms who would benefit from it and, hence, it has little effect. Armour and Teece (1978) demonstrate that early movers to the multidivisional form benefit, relative to late adopters, but that, once diffused, the form has no impact on profits. Agency or transaction-cost theorists could conclude that managers were sufficiently constrained during 1970 so as not to act against the interests of shareholders. For bank- or family-control theorists, there is no evidence to support the view that either group affected the financial performance of firms.

What effects did predict financial performance? Generally, the more shares held by institutional investors, the more poorly the firm performed in terms of returns on assets, sales, equity, and market value. We can interpret this result in two ways. First, this is evidence that there was a problem of separation of ownership from control. Alternatively, 1970 was a bad year for the stock market, and many stocks lost a lot of value. It is possible that firms held by institutional investors fared more poorly during the downturn because these investors had overbought certain stocks during the boom from 1967 to 1969. If these results hold up in the panel analysis, then this will be evidence for the thesis that managers control firms with a large percentage of stock held by institutional investors and act to increase variables other than profits.

Several of the political/cultural variables also predicted financial performance. There is evidence that diversified firms had a higher return on equity and a higher price/earnings ratio. This suggests that the agency-theory view that diversification reflects management control and an unwise use of a firm's resources is flawed. Instead, diversification proved profitable. Firms with unrelated-product mixes also held higher debt, reflecting the acquisitive conglomerates' strategy of using debt to diversify. This also undermines the view that management-controlled firms are likely to favor the issuance of equity over debt.

Finally, there is evidence that the background of the CEO affected financial performance. Finance CEOs were able to improve firms' balance sheets; their firms had higher returns on assets and sales and higher market value than firms headed by other types of CEOs. Sales and marketing CEOs also were able to increase the market value of their firms. We interpret these effects as the operation of the special expertise of these types of CEOs who aimed at improving their firms' performance on exactly these measures. It also could be that finance CEOs have greater legitimacy in the institutional environment.

The evidence for the effects of forms of control on changes in financial performance are in table 3. The panel results parallel the cross-sectional results. Because the previous level of performance is included as a regressor in these equations, the rest of the parameters can be interpreted as predictors of change in the dependent variables (Kessler and Greenberg 1981, chap. 2). Firms that were linked by bank interlocks generally underperformed other firms, showing lower returns on assets, sales, and equity and a lower price/earnings ratio. Thus, being networked into the financial system did not prove to be a benefit. Further, firms that were interlocked had lower debt/equity ratios, suggesting that these firms favored equity over debt. None of these predictions supports a view that, during the decade, bank control directed profits toward banks. There was no direct effect of banks being significant shareholders on the financial performance of firms. Firms that had large numbers of bank shareholders did no better or worse than anyone else.

There are several effects of family holdings on performance. Family-controlled firms had lower returns on assets and sales and a lower price/earnings ratio. They also experienced less growth in sales over the decade. The higher the share of family holdings, the less well firms performed. This contradicts all of the theories of managerial control and suggests that during the turbulent economic times of the 1970s family control was not a good way to increase shareholder value.

The percentage of shares held by the institutional investors did not affect the change in the financial performance of firms, indicating that the results for 1970 were probably an artifact of the poor performance of firms whose stock was disproportionately held by institutional investors during the 1970s and not an effect of managerial control.

The most systematic predictor of change in financial outcomes during the 1970s was the asset growth of the industry. This variable positively affects growth in return on assets, growth in assets, sales, and market

TABLE 3

Regression Analysis of the Determinants of Various Measures of Financial Performance, 1970–1980 ($N = 106$)

Independent Variable	Dependent Variable																	
	FCF		MV		PE		DE		ASSETS		SALES		ROA		PM		ROE	
	b	SE(*b*)	*b*	SE(*b*)	*b*	SE(*b*)	*b*	SE(*b*)	*b*	SE(*b*)	*b*	SE(*b*)	*b*	SE(*b*)	*b*	SE(*b*)	*b*	SE(*b*)
LAG[a]	.97**	.10	.52**	.03	−.00	.01	.42**	.15	1.20**	.04	1.86**	.13	.30**	.06	.31**	.05	.00	1.11
MERGER1	−6.62	22.30	−129.11	83.35	−.01	.14	.01	.06	−85.40	102.40	−190.02	165.01	.00	.07	.03	.07	−1.68	4.06
MDF1	−22.29	107.04	63.74	402.23	−.31	.69	−.33	.30	−68.51	487.28	29.56	86.99	−.15	.34	−.22	.36	26.48	19.36
INDGROW	15.07	9.64	252.34**	35.53	−.03	.06	.00	.02	171.12**	44.87	531.80**	72.02	.07**	.03	−.01	.03	−.08	1.72
RELN	18.91	106.78	−175.68	398.92	−.19	.71	.49*	.20	290.15*	144.28	−112.46	202.63	.17	.34	.40	.36	35.78*	19.79
UNRELN	4.91	156.10	109.38	586.58	−.27	1.03	.68*	.35	−71.23	112.41	−54.56	155.22	.68	.50	.70	.53	−38.80	28.36
MANPRES	188.54	121.95	10.44	456.85	.50	.79	.25	.34	−48.53	62.23	94.04	92.90	−.03	.39	−.13	.42	−7.39	22.12
SALEPR	34.83	126.35	−111.05	478.13	−1.15	.80	.35	.35	569.53*	274.53	502.82*	234.28	−.39	.39	−.62	.42	−8.47	22.54
FINPR	23.54	118.99	129.34**	44.38	.17	.78	−.49	.34	413.74**	172.63	476.66**	187.26	.72*	.38	.72*	.40	42.27*	21.63
INSTIPER	276.32	816.04	63.18	3,145.90	3.67	5.31	1.32	2.32	−5,103.50	3,842.90	1,074.97	2,331.62	1.60	2.70	1.68	2.79	22.52	148.04
ILOCK	−33.32	19.72	−3.82	73.25	−.26*	.13	−.15**	.05	−71.43	88.84	−266.92	147.62	−.23*	.06	−.19*	.06	−10.80*	3.63
BANK	−20.11	14.27	−2.88	53.40	−.05	.09	.01	.04	−53.43	65.10	−61.61	106.28	−.00	.04	.01	.04	−.24	2.58
FAMILY	−9.28	5.52	−31.62	20.36	−.06*	.03	−.00	.01	−7.22	25.35	−73.98*	41.51	−.05*	.01	−.04*	.01	.38	.99
ILOCK-D	−65.88	132.98	−397.35	494.75	.06	.86	−.19	.37	−431.11	605.71	−296.48	278.34	−.60	.42	−.13	.45	35.85	23.77
BANK-D	−97.49	121.05	−115.83	454.77	.36	.75	.18	.33	−372.44	546.71	−519.57	620.54	.16	.37	.23	.39	2.44	21.14
INTERCEPT	164.09	210.44	115.60	798.03	4.85**	1.35	−.08	.60	634.81	558.58	−566.42	1,602.26	1.56*	.72	1.23	.75	−21.63	38.38
R^2	.52		.78		.03		.14		.94		.80		.41		.43		.15	

[a] Each dependent variable was lagged to its 1970 value.

* $P > .05$.

** $P > .01$.

value of the firm. From an investor's point of view, the best type of firm to own stock in was one that was located in a growing industry.

The political/cultural variables also predict changes in outcomes in the 1970s. Sales and marketing CEOs produced growth in assets and sales for their firms. Finance CEOs produced growth in assets, and returns on assets, sales, and equity, as well as in market value. Firms with product-related strategies were likely to increase their return on equity while firms with product-unrelated strategies did no worse or no better than firms with product-dominant strategies. Firms with product-related and unrelated strategies were also likely to carry higher debts than product-dominant firms, reflecting their merger growth strategies of the 1960s and 1970s.

Taken together, the results suggest that forms of control were generally not important predictors of the financial performance of large corporations, either in the cross section or over time. Instead, the growth of the industry, the product strategy, and the background of the CEO were the most important predictors of performance. Managerial firms not only did not underperform either bank- or family-controlled firms; they outperformed them.

One interesting question is: What determined the strategies of firms? The results in table 4 explore the evidence that control types affected strategic decisions beyond financial performance. None of the control measures predict whether a firm engaged in a merger in the 1970s. Similarly, bank interlocks did not make firms more likely to engage in mergers. The only measures that did predict mergers are the political/cultural ones. If a firm had engaged in mergers in the 1960s, it was more likely to engage in mergers in the 1970s. Firms with product-related and product-unrelated strategies were more likely than firms with a product-dominant strategy to participate in mergers. Finally, firms with a finance CEO were more likely to have a merger in the 1970s. These results suggest that mergers as a strategy were entirely the product of organizational factors and unaffected by the measures of forms of control.

The equation predicting firm product mix in 1980 is also shown in table 4. None of the control measures predict whether firms are diversified; thus, there is no evidence that diversification is undertaken by managers who act on their own behalf and against the interests of owners. Instead the evidence suggests that diversified firms performed better than nondiversified firms (see tables 2 and 3) and that these firms undertook diversification for organizational, not control, reasons. Three sorts of political/cultural variables affected whether a firm was diversified in 1980: if it had engaged in a merger during the 1960s, if it had a finance CEO in 1969, and if a firm was in an industry with high percentages of firms with product-related and product-unrelated strategies.

TABLE 4

LOGISTIC REGRESSION ANALYSIS OF THE DETERMINANTS OF VARIOUS ORGANIZATIONAL CHARACTERISTICS (N = 110)

	DEPENDENT VARIABLES											
	MERGER2, 1969–79		Strategy, 1980				President's Origins, 1980					
			Related		Unrelated		MANPRES		SALEPR		FINPR	
INDEPENDENT VARIABLES	*b*	SE(*b*)	*b*	SE(*b*)	*b*	SE(*b*)	*b*	SE(*b*)	*b*	SE(*b*)	*b*	SE(*b*)
MERGER1	.23*	.12	1.59**	.48	.27*	.13	.27	.17	.42*	.23	.05	.18
RELN	1.38*	.65					.04	.39	.24*	.12	.05	.39
UNRELN	1.97*	.86					.53	.53	1.65*	.87	.60*	.24
MANPRES	.72	.61	.20	.54	.20	.43						
SALEPR	.73	.64	.02	.57	−.02	.37						
FINPR	1.21*	.65	.18	.53	.31*	.14						
MDF1	−.79	.57					−.04	.36	1.57**	.61	.68*	.33
INSTIPER	−.66	4.29	3.81	7.25	1.43	5.61	.63	.37	.50	.75	.75	.79
BANK	.05	.08	.03	.13	.04	.12	.02	.08	−.27	.17	−.28*	.13
FAMILY	−.03	.03	.09	.06	.04	.06	.03	.03	−.04	.06	−.03	.03
ILOCK	.04	.10	.03	.16	−.03	.13	−.14	.15	.08	.17	−.08	.13
BANK-D	.80	.61	−.05	.53	−.09	.40	.14	.40	.42	.60	.94	.43
ILOCK-D	.73	.70	.34	.61	.30	.43	.03	.47	.60	.66	.51	.48
PERREL			.03**	.01	.00	.01						
PERRUNREL			−.07**	.02	.06**	.02						
FINDPER							.01	.05	.02	.05	.04*	.01
SINDPER							−.01	.03	.06*	.02	.01	.03
MINDPER							.05*	.02	−.04	.03	−.01	.02
INTERCEPT	−2.29**	1.18	3.60**	1.67	2.03*	1.14	−.34	1.52	−3.27	2.47	−.82	1.57
χ^2 (*df*)	22.65 (13)		56.72 (29)				147.70 (54)					

* $P < .05$.
** $P < .01$.

Finally, we consider the determinants of what types of CEOs were in place in 1980. Some of the control variables have interesting effects. For instance, family-controlled firms were less likely to have finance CEOs. Given that finance-CEO-led firms outperformed other firms in the 1970s, this proved to be a mistake. Second, firms with a high percentage of stock held by institutional investors tended to have sales and marketing or finance CEOs. This can be interpreted as an effect of manager control; that is, managers tended to control these types of firms, and they selected CEOs from power bases that were in ascendance.

There are clear organizational effects on who is made CEO. Firms that had mergers or product-diversified strategies were more likely to have sales and marketing or finance CEOs, consistent with the argument made earlier. Furthermore, industries with high percentages of finance or sales and marketing CEOs tended to affect the firm's propensity to have finance or sales and marketing CEOs. This clearly reflects an isomorphic process (DiMaggio and Powell 1983).

DISCUSSION AND CONCLUSIONS

Overall, the agency theorists and the sociological theorists who have focused on forms of control, either family, management, bank, or resource dependence through bank interlocks do not do a good job of explaining how large corporations performed or how they behaved strategically. Bankers, owners, and managers live in organizations that are systems of power located in organizational fields that help define successful behavior. If they attempt to construct their actions outside that context, then they threaten to undermine the organization that is producing wealth for them. The controlling force that determines courses of successful action is thus emergent in organizational interaction. The theory that accounts for these patterns most effectively is the political/cultural approach.

We might be tempted to argue that the political/cultural approach is generally consistent with a standard economic view that the market signals to managers about what works, and they follow the signals. One problem with the economic view is that it can only specify strategies after the fact. The political/cultural approach considers how competing conceptions of control can be compared in terms of their efficacy in producing outcomes. Put another way, economic theory could not theorize these variables precisely because they are not concerned with capturing firm or organizational field differences in conceptions of control. It is also true that these effects exist net of the growth of the industry (a rough indicator of the demand for the product), and therefore, they cannot have a purely economic interpretation.

Agency or transaction-costs theorists could interpret these results as evidence that managers were sufficiently constrained by boards of directors, contracts, and financial markets. Indeed, there is evidence that professional managers actually outperformed their counterparts in family-owned firms. But, to the degree that these perspectives are testable, there is little evidence in their favor. First, it is clear that managers do not favor free cash flow. Diversification, which was supposed to be bad, generally had positive effects. The data analyses show that the conventional wisdom about the 1970s was wrong as well: financially oriented executives who ran diversified firms and engaged in mergers outperformed their brethren, on the average.

It is useful to consider these results and what they imply about the causes of the 1980s merger movement. Jensen (1989), an agency theorist, has argued that inefficient managers held onto cash flow and made poor investment decisions by favoring diversification, provoking others to engage in mergers in the 1980s. The evidence presented here is that managers, particularly finance CEOs, did pretty well in the 1970s, considering the turbulent economic times. Two other hypotheses about the merger movement of the 1980s are more consistent with this evidence.

Friedman (1985), a finance economist, has argued that the primary cause of the 1980s merger movement was the high inflation of the 1970s that kept interest rates high, stock prices low, and inflated the value of corporate assets. Fligstein and Markowitz (1992) have argued that the lax Reagan-era antitrust policy and the 1981 tax cuts gave firms incentive and money to engage in mergers. They also have shown that firms that were diversified and led by finance CEOs were more likely to engage in all forms of financial reorganization (mergers, divestitures, and the repurchase of stock) and were less likely to be the targets of mergers.

This suggests an alternative interpretation of the 1980s merger movement that focuses on how managers were able to deal with the inflated value of their assets. Finance-oriented managers maintained control by recognizing their assets were undervalued and taking actions to prevent their being deposed, while non-finance-oriented managers were targets of mergers. It was not that the managers were systematically working against the interests of shareholders but, instead, that a select number of them were able to analyze the cause of their problem (undervalued assets) and act.

There has been a lot of interest in the recent literature in creating a sociology of markets, and much of that discussion has focused, and we believe rightly so, on the problem of social embeddedness (Granovetter 1985). The theoretical difficulty is deciding what that embeddedness consists of. The results presented here suggest two things. First, organizational and not financial or ownership embeddedness is likely to be a more important cause of actions of firms than anything else. This means we should concentrate efforts on specifying models of the relations between firms that focus on intra- and interorganizational processes, such as the construction of strategic action and the cultural frames by which such a construction makes sense.

Second, the type of embeddedness we choose should be relevant to the problem. The connections between firms, banks, owners, and managers just do not significantly predict much that is interesting in the strategic choices of firms, and, therefore, our theoretical focus should be on what does. In our particular case, most of the financial interlocks appear to reflect co-optation by managers and not control by bankers. This result is consistent with the general view proposed here that the relevant focus of attention should be the firm. We should abandon our concentration on board of directors as a source of network data, in general, and financial linkages across boards of directors, in particular, unless their possible relevance can be specified theoretically (for an example that explicitly concerns investment banks, see Baker [1990]). More relevant network data would focus on how firms obtain knowledge of competitors (trade publications, associations, meetings, and market research) and how they filter their use of that information.

The above does not imply that elites do not exist, nor that they do not organize to achieve collective political ends. Instead, it implies that the substantive content of economic actions taken by elites who control the large corporation will not be determined by a narrow view of what their interests are. Those interests will be determined by the context of their actions; that is, the organizational situations in which they find themselves. Organizational theory provides an alternative sociological view that forces us to take seriously the nature of the relations between large-scale corporations, their internal structuring, and the problem of constructing courses of action. In short, we think that a real sociology of markets will need to focus on some of the elements that have proved to be important here: inter- and intraorganizational politics and the specification of cultural frames that define the social relations between actors.

So short an article is not the place to develop a full-blown theory of the organization of markets, and the results we present are only suggestive of such an approach. However, we have tried here to push this debate by showing the usefulness of such notions as conceptions of control, organizational fields, and firms as structures of power, in order to understand how actors who control firms structure the behavior of those firms.

APPENDIX

TABLE A1

CONSTRUCTION OF FINANCIAL MEASURES

Financial Measure	Component Parts
Market value	Stock price (at close) × common shares outstanding
Price/earnings ratio	Stock price (at close)/earnings per share excluding extraordinary items
Return on assets	100 × income before extraordinary items/(total assets + total gross property, plant, and equipment − total net property, plant, and equipment)
Profit margin	100 × income before extraordinary items/net sales
Return on equity	100 × income before extraordinary items adjusted for common stock equivalents/total common equity
Debt/equity ratio	Total long-term debt/total common equity
Assets	Total assets + (total gross property, plant, and equipment − total net property, plant and equipment)
Sales	Net sales
Free cash flow	Current assets − current liabilities

NOTE.—Measures were run using data from Compustat (1989).

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A Hero for the Aged? The Townsend Movement, the Political Mediation Model, and U.S. Old-Age Policy, 1934–1950[1]

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During the Depression, the Townsend movement enjoyed varied success in seeking pensions for the aged. Social-movement models predict that success depends on the mobilization of resources or on collective action. Other theories predict that economic or political conditions cause the emergence of movements *and* changes in public spending. The political mediation model used here holds that, to succeed, a movement must reinforce political action with strong organization of members under favorable political conditions. This article defines "success" and employs various analytical and empirical strategies, including qualitative comparative analysis on state-level data, to appraise the models. Although each perspective has some support, the political mediation model offers the best explanation of the patterns of successes. The state and the political party system determine whether mobilization and action benefit a constituency and win acceptance for a movement organization.

Perhaps more than any other social protest movement of the Great Depression, the insurgency of the aged led by Francis E. Townsend has been credited with transforming public social provision in the United

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States. Its opponents such as Frances Perkins (1946) and Arthur Altmeyer (1966) have attested to its importance, as have social scientists ranging from Piven and Cloward (1971), to Quadagno (1988), to Weir, Orloff, and Skocpol (1988, p. 23). Yet such claims are paradoxical. The "Townsend plan"—$200 per month for those 60 years old or older who agreed not to work and to spend the money—failed, and President Franklin D. Roosevelt refused to meet Townsend. Holtzman's (1963) study dismisses the movement of the Townsendites (as its partisans often called themselves) as a failure. The question remains: Was Townsend a hero for the aged?

This dispute is paralleled by social science debates about the outcomes of mobilizations and what constitutes "success" for them (e.g., Gamson 1990, app. A; McAdam 1982; Jasper, Nelkin, and Poulsen 1990). Some researchers (e.g., Gamson 1990) discount the macrosociological context, assuming that resource mobilization or collective action will achieve results; others (e.g., Kitschelt 1986) argue that the political context will decide both the movement's form and the response to it. Yet they ignore an important question: Under what conditions do movements win benefits for constituents and recognition for the organization?

Our goal is to uncover the determinants of successes and failures. To do this we will first explore the Townsend movement's influence on the historical trajectories of old-age policy at the national level, examining the Social Security Act, the amendments to it in 1939 and 1950, and the wartime hiatus on social spending. Next we will use multiple regression techniques to analyze old-age assistance or "pensions" at the state level and we will analyze state "memorials" asking the national government to enact the Townsend plan—a form of acceptance. To examine the *conditions* under which the movement succeeded or failed we will employ Boolean, qualitative comparative analysis (QCA), which locates combinations of conditions leading to qualitative outcomes (see Ragin 1987)—winning recognition, new advantages, and combinations of such victories and failures.

We argue that the political context mediates the impact of movement organization and action on its goals and sets the range of possible outcomes. Structural characteristics such as undemocratic political systems and patronage-based, traditional parties can deflect movement efforts. Middle-range influences of state bureaucracies and short-term political coalitions can intensify the impact of movements. To take advantage of openings, movements must back political action with organizational strength. We find that the movement helped the aged, but mainly through old-age assistance, on which the views of the Roosevelt administration and the Townsendites were not greatly in conflict. In some states the movement achieved high pensions and recognition, becoming in effect a "member" of the polity, to use Tilly's (1978) terminology. Strength in organization and standard political action—in favorable political party and state climates—led to success. By contrast, the movement had only a minor effect at the national level in improving old-age assistance. Even this gain would have been unlikely without the existence of a moderately strong pro-spending coalition.

TOWARD A POLITICAL MEDIATION MODEL OF PROTEST MOVEMENT OUTCOMES

Analyses of movement outcomes are hampered by two key problems. First, what is meant by success is rarely well defined and varies from analysis to analysis. Movement organizations usually attempt to influence the policies of the state to aid their members, but there is no standard way to assess benefits gained through these policies. Second, research on movement outcomes often does not take into account the causes of mobilization that might also influence outcomes (cf. Snyder and Kelly 1979; Isaac and Kelly 1981; Burstein 1981). This is a problem because movements often hope to effect changes in public spending policies, and the determinants of such policies are often similar to those of social-movement formation (see Skocpol and Amenta 1986; McAdam, McCarthy, and Zald 1988). Thus, analyses may exaggerate the effect of the movement on policy outcomes. To sustain a claim that a movement's actions are effective, a researcher must show that their influences are in addition to other causes of changes in policies.

Success and the Social Movement

There are three levels of success for movements. The lowest is for the organization to achieve recognition from opponents or the state. Following Gamson (1990), we call recognition that does not win benefits "cooptation." At the highest level the challenger transforms itself into a member of the polity; that is, processes of government or interactions with opponents routinely favor the group (see fig. 1). A challenger's interests may be achieved through laws, state bureaus to enforce them, and the removal of the issue, favorably resolved, from the political agenda.[2]

At the middle level are gains in policies that aid the group. At this level, the movement organization can, at minimum, influence the thinking of the public or policymakers (Gusfield 1981). In the best case, the organization may have its own program enshrined in legislation. Many

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[2] A revised version of Gamson's (1990, p. 29) typology, this figure is a static representation of outcomes; protest movements may move into different categories over time.

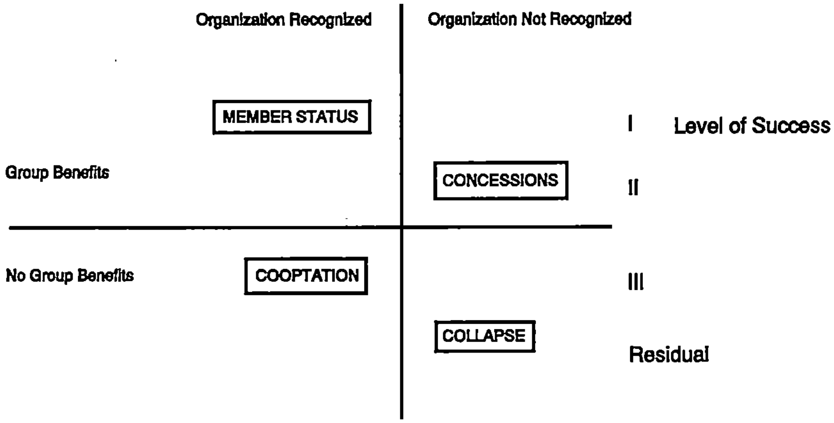

FIG. 1.—Levels of success for social movements

cases fall in the middle: the movement's activity may keep its issue on the political agenda, but without its opponents recognizing the movement organization. Sometimes new spending legislation benefiting the group results from such activity—what Gamson calls "preemption." We refer to these gains as "concessions," because the organization has aided the group and will seek to gain credit for it. Laws establishing group-based rights with bureaucratic and fiscal reinforcement move a group closer to polity membership than do short-term and discretionary spending laws.

Theories of Movement Outcomes and Theories of Public Policy

Typically, theorists of movement outcomes focus on the structure and activity of movement organizations (e.g., Gamson 1990). Disputes center on the relative effectiveness of noninstitutional (Piven and Cloward 1971) and "assimilationist" (Lipsky 1968) means and on the optimal characteristics of the movement organization (Gamson 1990). Most agree that the movement's features matter most and that large movements engaged in coherent programs of collective action without displacement goals will typically succeed.

Others have argued that movements arise from individual responses to strains in the social fabric and are mainly ineffective (e.g., Smelser 1962). In this view, strains breed grievances, which lead to aggression, expressed partly in collective behavior. The goals of such movements are often labeled utopian. This view sees movements as correlating with policy change, but not causing it; instead, political institutions confront the strains that drive collective behavior. By resolving these strains—

perhaps by enacting public spending to address social needs—these institutions subdue the grievances uniting the movement.

Some economic theories of public spending have much in common with this grievance view of movements (see Skocpol and Amenta 1986, pp. 133–36; Quadagno 1987, pp. 111–17). One economic theory holds that industrialization causes social dislocations—such as destruction of the extended families who then no longer care for the aging population—that force political regimes to engage in public spending—such as old-age pensions. Some theories of capitalism argue that economic declines such as the Depression require public spending to legitimate the system. In both cases, economic conditions are the fundamental causes of both movements and public spending, and so movements have no independent effect.

Theories focusing on "political opportunity structures" (e.g., Kitschelt 1986) have similar implications for the outcomes of protest movements. In the strongest form of this argument, opportunity structures determine both movement formation and what may be perceived as gains won by the movement. A member of the polity may aid a movement in order to add to its coalition—perhaps by introducing a spending program favoring the challenger; the movement is an epiphenomenon—a sign that policies are changing, but not the cause of changes.

Some perspectives, focusing on party partisanship, the strength of organized labor, or on the state and political institutions, suggest that political conditions that influence spending policies may also spur movements. One view is that public spending programs are boosted when left or center political parties come into power (Shalev 1983; Myles 1984) or when labor movements are strong. These parties or the labor movement may also aid the mobilization of new groups. Others argue that state actors have distinct interests (Orloff and Skocpol 1984; Carruthers 1991) and may find it advantageous to encourage like-minded movements. Similarly, the policy framework of a polity may influence further innovations—and also movements (McCarthy, Britt, and Wolfson 1991).

The Political Mediation Model of Movement Outcomes

We take theories of public spending policies into account by arguing that the outcomes of movement organization and action are mediated by the political context. Figure 2 highlights the differences between our political mediation perspective and other points of view. Unlike those using the standard social-movements models (arrows labeled "II"), we argue that mobilization and collective action are usually insufficient for policy changes; under some circumstances, however, mobilization and collective action may bring about changes. We avoid the strong view that the

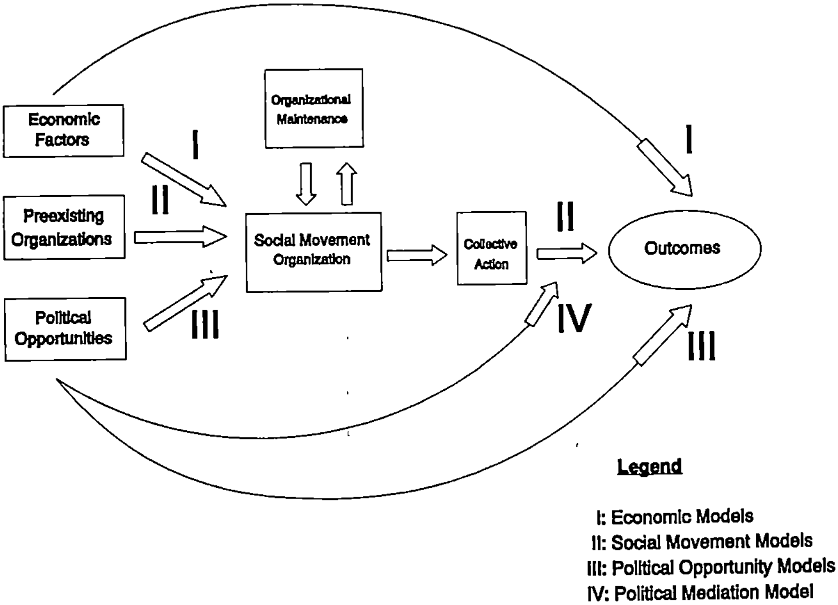
Economic Factors
Preexisting Organizations
Political Opportunities
Organizational Maintenance
Social Movement Organization
Collective Action
Outcomes
I
II
III
I
II
IV
III
Legend
I: Economic Models
II: Social Movement Models
III: Political Opportunity Models
IV: Political Mediation Model

FIG. 2.—Models of social movement formation and outcomes. Legend: "I" represents economic models; "II" represents social movement models; "III" represents political opportunity models; "IV" represents political mediation models.

economy (arrows marked "I") or political opportunity structures (arrows marked "III") determine both movement formation and policy outcomes; we argue that movements can also influence outcomes, though no specific type of collective action is deemed the most efficacious. The political context mediates the relationship between action and outcomes (arrow IV).

Two key long-term aspects of the political context are the political system and the party systems. First, an undemocratic political system—indicated by restricted voting rights and a lack of choice between parties—discourages both social movements and public spending policies. Even where movements coalesce in such polities, they are unlikely to achieve their goals, symbolic or substantive.[3] Second, traditional or patronage-oriented parties regard movements as menaces and programmatic spending policies as threats to the individualistic rewards on which such parties thrive. If movements contend in such polities, traditional patronage parties are more likely to give *symbolic* benefits than to pass programmatic spending policies because discretionary and individualistic policies are the lifeblood of these parties. We expect that these structural blocks will also thwart the efforts of state actors and insurgents in the party system to enhance programmatic public spending.[4]

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[3] Referees argued that the model does not seem to fit the civil rights movement, which was based in the nondemocratic South. We disagree because the model of movement emergence is multicausal and does not rule out the possibility of movements arising in nondemocratic polities. Our model also implies that full success would be unlikely in nondemocratic polities. We believe this fits the case, as the civil rights movement

We also anticipate more than one political path to changes in public spending policies. Favorable middle-range and short-term party and state conditions can produce social-spending gains for many disadvantaged groups. Left-center parties and state-building officials can be forces behind public spending. However, movements can fill voids where such actors are insufficiently powerful. If the structure of a political system is conducive—a democratic polity with an open party system—and a movement is strong in organization and in standard politics, it can be successful even if it lacks state or party allies.

Moreover, for issue-oriented movements such as the Townsend movement, the existing policy framework is also important in accounting for the possibilities of success and failure. A movement devoted to a policy at odds with the established framework is unlikely to be recognized (see fig. 1). When the policy orientation of a movement conflicts with established policy frameworks and the plans of both the regime in power and state bureaucrats, a movement is unlikely to achieve full success (right side of fig. 1). The best possible outcome is to win concessions, the worst is to "collapse," because the movement is expected to go unrecognized. Polity members may attempt to woo away the organization's supporters by amending current programs benefiting the group, but without transforming the policy framework. When the plans of the movement and the policy framework of polity members do not conflict, the movement can achieve recognition, but must avoid co-optation, a level of success lower in our scheme than concessions (left side of fig. 1). The organization has a chance to win polity membership by possibly securing policies to aid its constituents. These chances depend not only on its collective action

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won its victories mainly through the actions of the federal government, whose officials were concerned with the voting behavior of African-Americans in the North, to bring recalcitrant Southern state legislatures in line (see McAdam 1982).

[4] Both conditions have mattered in American history (Amenta and Skocpol 1989). The democratization of the polity was early—there was almost universal white male suffrage by the 1830s—but uneven—the South constricted voting rights from the turn of the century until the 1960s. Traditional parties took hold in the 19th century and only gradually lost their grip on politics.

and organizational strength, but also on conducive state or party circumstances.

THE FAILURE OF THE TOWNSEND PLAN AND THE RISE OF OLD-AGE SPENDING PROGRAMS

The Townsend movement was inspired by Townsend's September 1933 proposal: that all Americans over 60 years old be given $200 at the beginning of every month; everyone accepting this pension had to be retired and had to spend the sum by the end of the month. What was soon labeled the Townsend plan led in January 1934 to the not-for-profit "Old Age Revolving Pensions, Ltd." (OARP). The plan would end the economic woes of old age and solve unemployment, as the elderly would not compete with the young for jobs, and new spending would stimulate the economy; the mandatory spending provision would make old-age pensions change hands or "revolve" faster than other money. Also, the plan would not aggravate the deficit; it was to be funded by a "transactions tax," a multiple sales tax.

The plan, however, was criticized heavily. The political Left argued that the transactions tax was regressive. The Right feared that the tax would cut profits and the spending would undermine the incentive to work. Scholars and policy experts also disapproved. The tax was set at a seemingly low rate, but it implied much higher levies on finished goods. Many thought it unworkable to register farmers for the tax. Economists estimated that a 2% tax would have amassed only $4–$9 billion yearly (Twentieth-Century Fund Committee on Old Age Security 1936; National Industrial Conference Board 1936). No economist felt that the aged would spend faster than those taxed. Soon the Social Security Board (SSB) was orchestrating this criticism (Cates 1983, pp. 52–53).

Yet the plan was no utopian scheme. The aged would have the right to a large national pension, whose size would be determined by revenues from an earmarked tax. Such pensions would have been simpler to administer than the two-part system that won out. Even if the tax had netted only $6 billion per year, that would have meant pensions of approximately $50 per month, much higher than assistance. A national sales tax would have been regressive, but so was the tax on the payroll for social insurance.

In January 1935 a Townsend bill was introduced in Congress, and in April it was followed by an amended bill, taking into account many criticisms. But this bill was rejected handily in the House, in a standing vote mainly along partisan lines with Democrats in opposition, 206–56. The totals were low because many members, especially Western ones, wanted neither to support the bill nor to go on record as opposed. In

1939, another Townsend proposal passed through the Ways and Means Committee, but only because the movement's congressional supporters were joined by adversaries, who hoped to discredit the movement with a lopsided roll-call vote. The 1939 rejection, 302–95, was decisive—fewer members feared to go on record, with only 55 Republicans and 40 Democrats in favor. Although the movement remained active in the 1940s, no Townsend bill was ever again discharged from committee. The movement went unrecognized by its national-level opponents, and the plan failed to convince policymakers or the public.[5]

Despite these failures, innovations in old-age spending flourished. There were five main stages (see table 1). First, in the early 1930s states passed old-age pensions, and Congress promoted plans to aid these means-tested programs. Second, the administration saw through to enactment the old-age insurance and assistance titles of the 1935 Social Security Act. Third, assistance and insurance were advanced in 1939. Fourth, these programs suffered setbacks during wartime. Finally, at the end of the decade, old-age insurance was upgraded.

By 1934, 23 states had adopted old-age pension programs. Efforts by the national government followed. The Dill-Connery Bill, which called for national matching funds for pension programs, passed the House in 1933 and 1934 (Epstein [1938] 1968). Roosevelt's appointment of the Economic Security Committee in June 1934 led to the omnibus Economic Security Act (bill) of January 1935. This bill included old-age "assistance" providing matching payments of up to $15 per person per month for state-level, old-age pensions. It also included old-age insurance, a national program that provided benefits for those 65 years old and older who had qualified by paying a payroll tax levied on wage earners and employers on earnings up to $3000. This tax was to take effect in 1937; no benefits were to be paid until 1942. The bill became the Social Security Act in August 1935. In 1939 Congress expanded old-age spending. It added insurance for dependents and survivors, revised the formula to ensure larger payments at the beginning of the term, advanced the starting point for benefits to 1940 instead of 1942 and slowed scheduled increases in the payroll tax. Congress also increased the maximum benefits under old-age assistance to $20, allowing the states to increase them to a maximum of $40.

The next decade saw more downs than ups for old-age spending. In 1942, Congress froze previously legislated increases in the payroll tax

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[5] In January 1939, an American Institute of Public Opinion poll found that, of those familiar with the Townsend plan, 35% were in favor and 53% opposed, the rest having no opinion. In July 1941, only 19% were in favor and 39% opposed, with 42% having no opinion (Cantril and Strunk 1951, p. 542).

TABLE 1

U.S. SOCIAL SPENDING POLICIES FOR THE AGED, 1930–50

Period	Social Policy Developments
Recovery: The early depression and the first New Deal, 1930–35	California adopts first compulsory old-age pensions legislation (1929). Dill-Connery bill (1933–34) to give federal support to state-level old-age pensions debated in Congress. Twenty-three states adopt compulsory old-age pensions by the end of 1934.
The "reform" era and the second New Deal, 1935–37	Economic Security bill released, January 1935. The bill includes federal matching aid for old-age pensions, up to $15 per person. The bill also includes federal old-age benefits. A separate title includes a payroll tax; employers and employees each pay 1% of payrolls up to $3,000. The tax is scheduled to go into effect in 1937.
The late New Deal, 1937–40	1939 Social Security Act amendments provide insurance benefits for dependents and survivors, revise the benefit formula, advance benefits to begin in 1940, and hold employer/employee contribution rates to 1% through 1942. Matching payments for old-age assistance are increased to $20.
The prelude to war and the war years, 1940–45	Proposed increases in employer/employee contribution rate denied by conservative Congress seven times. The payroll tax rate is frozen until 1950.
The immediate post-war period, 1946–51	1948 Social Security Act amendments narrow coverage by redefining "employee" and excluding certain quasi-self-employed occupational categories. The Social Security Act amendments survive Republican opposition in the House in 1949 and are passed by the Senate in August 1950. New standards extend old-age and survivors' coverage to new groups, liberalize eligibility requirements, increase benefits overall, and extend federal matching provisions for medical care for the aged. Payroll tax rates are scheduled to increase to 2.0% by 1953.

SOURCES: Altmeyer (1966) and Amenta (1989).

(Leff 1988), and the middle war years were harsh for social spending generally (Amenta and Skocpol 1988). Moreover, Congress rejected President Harry S. Truman's bid to enact health insurance in 1946, and old-age insurance was amended in 1948 to exclude many previously covered workers. After the election of Truman and a Democratic Congress in 1948, however, a victorious bid to improve old-age and survivors' insurance began, culminating in August 1950. These amendments extended old-age and survivors' coverage to new groups, increased the level of benefits by about 67%, lowered eligibility requirements, and provided federal matching funds for care of the aged at medical institutions. The payroll tax was legislated to increase to 1.5% in 1950, and 2.0% in 1953 (Cohen and Myers 1950).

THE IMPACT OF THE TOWNSEND MOVEMENT AT THE NATIONAL LEVEL: COMPARISONS OVER TIME

Were these old-age spending policies influenced by the Townsend movement? To answer this we analyze successful and unsuccessful episodes of national old-age policy-making and compare them with predictions based on the models. As noted above, national old-age laws were passed in 1935, 1939, and 1950, and failed during the war years. We contrast old-age assistance and old-age insurance and analyze roll-call votes. These four cases provide evidence that the movement had its greatest impact in 1939, when it was strong both in mobilization and in political action—and when the pro–social spending coalition was moderately large.

With 450,000 members in February 1935, the movement had made a great leap forward in the second half of 1935, as can be seen through its dues-based finances (see table 2). In the first three quarters of 1935, OARP had gathered about $555,000, but in the last quarter alone brought in about $350,000, an unprecedented sum. That summer OARP also spread beyond the West. The movement peaked in membership near the start of 1936. Membership probably reached about 1.5 million (Holtzman 1963)—more than 18% of all those 65 years old and older. In the spring of 1936, membership slumped, dropping to 613,000 in 1938—7.2% of the aged. Yet the movement showed a resurgence of strength in 1939, embracing about 8.7% of those 65 years old and older. Membership declined during the war, leveling off at about 2.5% of the aged. Afterwards it degenerated; by 1949 it had dropped to less than 0.5% of those 65 years old and older (see Amenta and Zylan 1991).

The movement's political action campaign lagged well behind the mobilization of members. Its political activity centered on endorsing candidates for Congress—Republicans and Democrats—who were in favor of

TABLE 2

TOWNSEND MEMBERSHIP, RECEIPTS, ENDORSEMENTS, AND THE STRENGTH OF THE DEMOCRATIC PARTY PRO-SPENDING COALITION, 1934–49

Year	Paid Membership (in Thousands)	Gross Receipts (in Thousands of $)	Membership as % of Persons 65 and Over	Townsend-backed House Members Elected, in Actual Nos. and (% of Those Endorsed)	Democratic House Majority and Rating
1934	N.A.	84	N.A.	N.A.	216, strong
1935	N.A.	905	N.A.		
1936	N.A.	562	N.A.	72 (28.8)	242, strong
1937	N.A.	369	N.A.		
1938	612.5	453	7.20	147 (55.7)	97, medium
1939	761.6	622	8.69		
1940	646.9	689	7.16	132 (58.1)	106, medium
1941	468.7	636	5.05		
1942	297.6	486	3.11	172 (60.3)	10, weak
1943	271.8	424	2.75		
1944	269.6	628	2.66	172 (75.1)	52, medium/weak
1945	234.8	512	2.23		
1946	173.6	519	1.60	182 (66.2)	−57, weak
1947	134.3	418	1.20		
1948	92.7	353	.80	135 (65.5)	92, medium
1949	52.8	328	.44		

SOURCES.—Holtzman (1963, pp. 48–49, 172), U.S. Bureau of the Census (1975, p. 10), *Congressional Quarterly* (1985), U.S. Congress, House of Representatives (1936), and Amenta (1989).

the plan.[6] Chosen by the movement's national leadership, the selections were publicized in the *Townsend National Weekly* (1938–50) and backed in rallies held by local clubs. Although the movement failed to endorse one-half of those voting for the plan in 1935 and running for reelection in 1936, in 1938 the endorsement policy worked, with 147 of its candidates winning. This was more than twice the number elected in 1936 and OARP's success rate had doubled to about 56%. The number of Townsend-endorsed House members averaged more than 150 in the 1940s, reaching a peak in the middle of the decade.

Other factors, however, especially the left-wing Democratic party coalition that took power in the 1930s, may have aided social spending. This pro-spending coalition is defined by the election of a president allied with the labor movement and other insurgent forces and a Democratic majority in Congress large enough to overwhelm Republicans and representatives of undemocratic regimes, who opposed spending (Amenta 1989). From 1933 to 1953, the White House was held by a reformist, labor-supported Democrat, and so the coalition depended on the size of the Democratic majority in Congress (see table 2). The largest prospending coalitions were in power from 1935 to 1939. From 1939 to 1943, the strength of the coalition was medium. From 1943 to 1945, the coalition was weak and did not regain its prewar level of health until 1949.

The political mediation model would expect that the movement would be most influential when it was strong in both membership and endorsements. Yet its influence would be magnified by a moderately strong Democratic coalition. We would expect that a very strong Democratic coalition by itself—as in the years from 1935 to 1938—would lead to widespread social-spending gains, including gains for the aged. A very weak coalition—as during the war period—would make any spending innovations impossible. For these reasons, we expect that the movement had its greatest impact in the late 1930s—when the movement was effective in both mobilizing membership and electing members of Congress and the Democratic coalition was moderately strong. The comparison of the legislative episodes of 1939 and 1949–50 is instructive because, although the Democratic margin was similar, the movement was stronger in 1939. In addition, we expect that the movement's endorsements would be more likely to convert Republicans than Southern Democrats because the latter were from nondemocratic political systems.[7]

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[6] In 1938, 169 Republicans and 77 Democrats were endorsed. In 1942, 1946, 1950, and 1952, more Democrats than Republicans were supported. Endorsements ranged from 206 to 285 in biannual elections from 1936 to 1952.

[7] Examined separately, the movement's membership and its political action do not correspond to the ups and downs of public-spending policies in the 1930s and 1940s.

The Townsend movement had nothing to do with the earliest bids to enact old-age pensions in the states and little to do with the Social Security Act. As the Committee on Economic Security was constructing its proposals, the movement formed a political backdrop, putting pressure on the administration to propose something and on Congress to vote for something for the aged, accounting for some of the popularity of federal funds for old-age pensions. But the committee had planned at the outset to address economic insecurity due to old age, and the House had already passed matching funds for old-age pensions in 1933 and 1934. The movement was harmed by its incompetence in conventional politics. The first Townsend bill was poorly drawn up and amending the Economic Security bill was beyond its powers. The movement's regional isolation meant that only western representatives feared voting against it. The old-age spending programs that passed corresponded only marginally to the Townsend plan. Old-age assistance went only to the needy, and Congress, led by the Southern delegation, watered down the language concerning the adequacy of benefits (Quadango 1988). Old-age insurance began by collecting taxes without paying benefits. By contrast, the Townsend plan meant immediate, generous, and equal pensions to all of the aged as a right, in a pay-as-you-go manner. Old-age assistance was means tested and stingy; old-age insurance was to be given only to qualified recipients on the basis of previous wages. The movement opposed the *insurance* program—the administration's long-term solution—but could ally with those in favor of improving *assistance*—as the movement and the administration saw assistance as a short-term solution to poverty in old age.

One key circumstance was different during 1939. The movement had endorsed 147 winning House members including three on the important Ways and Means Committee (U.S. Congress, House of Representatives 1939). That year the SSB and the Advisory Council (U.S. Congress, House of Representatives 1939, pp. 3–43) proposed "variable grants" for old-age assistance, a program designed to appease southern Democrats that would keep pensions low in the South, place the fiscal burden on the federal government, and lower federal spending elsewhere (U.S. Congress, Senate 1939, pp. 95, 180). Instead, however, the maximum

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The movement came too late to influence the drive for old-age pensions and the Economic Security bill, taking off *after* the Social Security Act. Although the movement reached a recorded maximum of members in 1939, a year that old-age programs were upgraded, membership stabilized during the war, a poor period for social spending, and was minimal by 1950, when amendments passed. As for the political action, the movement did not make widespread endorsements until 1936, after the 1935 Economic Security bill. Moreover, the endorsement policy was successful during the war years—a dismal time for public spending.

federal matching payments for old-age assistance were increased from $15 per person to $20, and thus the issue was resolved in favor of the Townsend movement. Its influence was noted by Abraham Epstein: "If you keep the change to raise the Federal grant to $20 . . . you will merely inform all and sundry that all you have to do is to organize another crackpot group and Congress will raise it to $50, or $60, or $70, or $80. When are you going to stop this thing? Well, they will say 'It is Dr. Townsend that did the job,' and the Dr. Townsend movement will grow" (U.S. Congress, Senate 1939, p. 188).

In the wake of Townsend victories in the 1938 elections, Congress was anxious to give *something* to Townsend supporters. Because the Townsend plan and social insurance conflicted dramatically and the movement could not overwhelm administration forces, concessions were all the movement could hope to achieve.[8]

The war period, a third episode, demonstrates that a moderate level of movement strength in membership and political action was not enough to cause changes in spending, because when the political alignment in Congress is unfavorable, spending innovations and improvements are impossible. The Townsend movement kept its issue before Congress, in hearings led by Senator Sheridan Downey of California. Against its inclinations, the SSB drew up plans for a "double-decker" social insurance program, with each aged person receiving a flat amount while those who qualified received additional money according to previous earnings (Cates 1983, chap. 3). But the hearings were interrupted by the news from Pearl Harbor, and little was left of the pro-spending coalition by 1943. Old-age and survivors' insurance had its payroll tax cut instead.

In 1949, a Democratic coalition similar in size to that of 1939 came to power, and once again moderate changes in social spending occurred, though only in old-age insurance. Even with the collapse of its membership, the movement may have helped to elect representatives favorable not only to the Townsend plan, but to other old-age spending proposals. To determine the Townsend influence we analyze three key roll-call votes in the House. Our model expects two things: for the Townsend movement to influence the vote of members of Congress from states with voting rights, and for the movement to have only a marginal effect, as its membership had dropped severely by this point.

The pro-spending forces won two victories on rules forbidding floor amendments to the proposed bill. Republicans and conservative Democrats opposed the rule, which was intended to prevent crippling amendments. On both votes, Townsend-endorsed Republicans were more likely to cross their party's line, while Townsend-endorsed Democrats were slightly less likely to cross theirs. On the second gag rule vote the differences were substantial—among Republicans. Townsend Republicans were six times as likely to defect from the party-line position as their party colleagues. On a third vote, to recommit the bill in favor of a weaker Republican substitute (the Mason amendment), the House voted the measure down, mainly along party lines. Townsend Republicans were nearly four times as likely to vote against their party as their colleagues. By contrast, the Townsend endorsement did not persuade Democrats from the South to vote for social spending. On the first two votes, Townsend Southern Democrats were actually more likely than their non-endorsed counterparts to vote against progressive social security measures. Only on the third vote, to decide on the adoption of the gag rule, were Townsend-supported Southern Democrats less likely than other Southern Democrats to cross over (8%–17%). The movement aided the cause, but did not have as strong an impact as in 1939.[9]

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[8] Old-age insurance was amended to resemble more closely the Townsend plan's pay-as-you-go basis. These changes were proposed by the SSB and supported by administration Keynesians and business groups favoring reduced taxes (Leff 1988).

Overall, these episodes of national policy-making support our model. A lack of congressional support and feeble lobbying efforts during the deliberations over the Social Security Act minimized the influence of the movement. The movement had its greatest impact in 1939 when it was strong in both membership and political action. Yet the movement's influence required a specific political condition: a medium-sized, social-spending coalition. Although the movement was relatively strong in its endorsements during the war period, it was not enough to overcome a Congress opposed to social spending. The movement had some impact in 1950 through the long-term effects of the endorsement policy. But the lack of membership support meant that endorsed representatives who voted against old-age spending did not fear electoral reprisals. How endorsed members voted also provides support for our perspective. Townsend-endorsed Southern Democrats, who came from undemocratic political systems, were much less likely to dissent from the anti-spending line than Townsend Republicans. Moreover, the movement had its greatest success in old-age assistance, rather than old-age insurance. In assistance, the plans of the movement and those of the administration were not contradictory. Both groups could agree on the need for pensions. At the national level, however, the movement gained only a few new old-age benefits and was never fully accepted as a legitimate representative of the aged.

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[9] On none of the votes did Townsend support tip the balance. On the first vote, Townsend influence was minimal, not enough to account for the 19-vote margin of victory. Although the Townsend influence was greater on the second vote, that one ran almost strictly according to party lines. The Mason amendment was defeated handily. For details, see the *Congressional Record,* 1949, vol. 95, pt. 10, pp. 13818–819, and pt. 11, pp. 13972–973.

THE IMPACT OF THE TOWNSEND MOVEMENT IN THE STATES: MULTIPLE-REGRESSION ANALYSES

Gauging the influence of the movement at the state level requires a focus on old-age assistance. By the end of 1938 each state had passed a program, but eligibility restrictions and benefit levels varied substantially (Amenta and Carruthers 1988). For instance, California was paying approximately $32 per month per person and Arkansas approximately $6—a disparity unwarranted by differences in incomes or costs of living.

Within the states the Townsend movement was at first concerned chiefly with the promotion of its national goals. Lobbying in state capitals often focused on passing legislative memorials to Congress entreating it to enact the Townsend plan. Many states passed such memorials, signaling that the movement was a force in state politics, but opening the way to the co-optation of the movement because memorials only gave recognition to the aged, but no advantages. The Townsend movement also pushed for improvements in old-age assistance (see, e.g., Putnam 1970, p. 122). In these bids the movement was able to coordinate with groups that opposed the national Townsend plan and wanted to work within the system defined by the Social Security Act. We first model the determinants of the *average old-age pension* per month at the end of 1940 (U.S. Social Security Administration 1950), a measure indicating generosity in old-age assistance. This measure was regressed against measures associated with socioeconomic and political theories of public spending policies. Many of these variables may have influenced the movement as well as policies, and so we control for these influences.

To capture socioeconomic approaches, the first measure is the percentage of *aged* people, those over 65 years old (U.S. Bureau of the Census 1935, pp. 36–37; 1941, pp. 5, 43). Also examined is the influence of the Depression. The measure *depression in manufacturing* operationalizes the degree of economic decline in industry and incorporates wage-earner employment in manufacturing at the Depression's low point in 1933 as a proportion of employment in 1929 (U.S. Bureau of Census 1938, pp. 765–69) and rate of unemployment from 1930 to 1933 (U.S. Social Security Board 1937, pp. 58–59) in a standardized index, which scores negatively for severe depressions; this figure is multiplied by a categorical variable for 20 industrialized states (Amenta and Carruthers 1988). Similarly, the measure *farming depression* is the average of gross farm income in 1932, 1933, and 1934 divided by average gross farm income from 1924 through 1928 (U.S. Bureau of the Census 1932, p. 608; 1935, p. 589).

Our first political measures concern long-term features of state and party systems. *Voting rights* is the natural logarithm of the percentage of eligible voters voting in the 1932 presidential election (U.S. Bureau of the Census 1975, pp. 1071–72). Moreover, in the 1930s many states were dominated by traditional, patronage parties (Shefter 1983). *Traditional party organization* strength ranges from a high of five in states where such organizations predominated to a low of one (Mayhew 1986, p. 196). To measure *administrative strength,* a middle-range characteristic, we score one for each year until 1929 that the labor commissioner had rule-making authority in the administration of safety laws (Brandeis 1935, p. 654). We expect that states with bureaus exercising such administrative powers would promote spending programs. In the shorter run, spending may have been encouraged where *Democrats in democratic political and open party systems* controlled the governments in the 1930s; such regimes would be likely to follow the lead of the national party. States with competitive parties, without dominant traditional party organizations, and with Democratic control of the government for four years or more during the 1930s are scored one; others zero (see Hansen 1983, p. 158). We also measure the timing of the *adoption of old-age pension* legislation, which scores states from one to 10 according to the number of years by 1939 that a state had passed a compulsory law (Epstein 1968, pp. 534–35; U.S. Social Security Board 1937, pp. 161–62; 1938), assuming that the earlier the program the greater the spending on it.

Movements, especially labor, and other organized groups may have also influenced pensions. *Union density* takes union members in 1939 as a percentage of the nonagricultural employed in 1940 (Troy and Sheflin 1985, p. 7-3; U.S. Bureau of Census 1948, pp. 194, 196). Next, we consider the per capita membership of the *Fraternal Order of Eagles* (1927, 1928, 1929, 1930), which began lobbying for old-age pensions in the 1920s. We calculated two measures of the strength of the Townsend movement and its collective action. *Townsend clubs* is the per capita number of clubs 1934–50 (Holtzman 1963, pp. 50–51). The overwhelming majority of these clubs were in existence by 1940, when the second wave of mobilization peaked. Examining the direct influence of clubs captures potential for disruptive activity and for sustaining other collective action, such as election endorsements. *Townsend-endorsed House members* is the percentage of a state's delegation won by Townsend candidates in the break-through election of 1938. This measure is an indirect gauge of influence on state-level elections; working for congressional candidates doubtless affected state politicians from overlapping districts.

Two measures are closely correlated (.73), and we enter the two measures separately in the models below.

We employed regression analysis separately on each of three theoretical groups: movements and interest groups; socioeconomic conditions; and political structures and circumstances. Using the results of this first round, we estimated models with indicators from all groups and then eliminated measures that proved insignificant. In each model we used as a control measure *per capita income,* initially computed for 1929 (U.S. Bureau of the Census 1975, pp. 243–45). Most measures of social spending "efforts" take into account the income level of the state or country. Thus, we expect a strong and positive effect of this measure, but do not otherwise interpret it. The preliminary regressions (results not shown, but available on request) indicate that social-movement factors potentially had a great influence on old-age assistance. Yet standard socioeconomic and, especially, political models also do well in explaining average pensions. Are the effects of Townsend movement measures due to the relationship between socioeconomic and political conditions and group mobilizations?

To test this we estimated a model including the Townsend measures and socioeconomic and political factors. Even when we control for economic and political explanations of social spending, the Townsend movement has an independent influence (see table 3). In model 1, we used a stepwise procedure to eliminate the measures from initial regressions that did not aid the explanation. This model, which, with adjustments, explains about 74% of the variance, includes the control measure and measures of voting rights, patronage-party organization, administrative strength, open Democratic party rule, and the Townsend political action measure, which was significant at the .05 level. Model 2, which is similar but includes the measure of Townsend clubs, performed almost as well, explaining 72% of the variance, with adjustments. Here the Townsend indicator was not significant, however, at the .10 level.[10]

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[10] A referee suggested that we examine the issue of spatial autocorrelation (see Anselin 1988; Doreian 1980, 1981). If this were a problem, it would make our OLS estimates less efficient. Because of the political autonomy of the U.S. states and mass media, we do not think there are strong theoretical grounds for it. However, we explored the issue, constructing a contiguity matrix with ones for states that spatially adjoin and zeros otherwise. We employed both "spatial effects" and "spatial disturbances" specifications (Doreian 1980), using the SPACESTAT statistical package (Anselin 1990). The OLS residuals for the models presented showed some evidence of spatial autocorrelation, but the substantive results changed very little when the spatial autocorrelation models were fitted (results not shown, but are available). The regression coefficients never changed sign, and significance levels changed only slightly. In no case did a coefficient for Townsend movement measure become less significant than in the OLS specification. Thus,the OLS results are not misleading.

TABLE 3

FOUR FULL MODELS OF THE SIZE OF OLD-AGE PENSIONS, 1940 AND 1946–48

INDEPENDENT MEASURES	1940		1946–48	
	Model 1	Model 2	Model 3	Model 4
Movement model:				
Townsend-endorsed House members, 1938	.146**	. . .	.252**	. . .
	(1.68)		(2.20)	
Per capita Townsend clubs, 1940	. . .	.112	. . .	.183*
		(1.01)		(1.49)
Political (opportunity) model:				
Voting rights	.237**	.218**	.199*	.251**
	(2.32)	(1.86)	(1.55)	(1.94)
Traditional party organization	−.225***	−.231**	−.180*	−.197*
	(−2.20)	(−2.08)	(−1.63)	(−1.66)
Administrative powers	.213**	.216**	.174**	.182**
	(2.38)	(2.36)	(1.87)	(1.91)
Democratic control in open, competitive system	.095	.086	.169**	.164*
	(1.07)	(0.95)	(1.77)	(1.67)
Control variable:				
Per capita income	.557***	.576***	.456***	.433***
	(4.86)	(4.95)	(4.01)	(3.72)
F	22.76	21.56	18.53	17.08
Adjusted R^2	.735	.724	.691	.672

NOTE.—Coefficients are standardized and *t*-statistics are in parentheses. For definitions of measures, see text. $N = 48$.

* $P < .10$.

** $P < .05$.

*** $P < .01$.

In the 1940s the movement continued to elect members to Congress and to pressure state governments, which had two new incentives to raise assistance: the 1939 amendments, which increased the matching payments, and inflation due to war. To gauge the movement's influence we regressed the average monthly old-age pensions in 1946, 1947, and 1948 against indicators from the best-fitting models from previous trials. Because the measure of Townsend clubs gauges the movement *in 1940* its disruptive and other effects may not have registered by then. The movement also refined its endorsement policy, and we use the average Townsend-endorsed share of the House delegation in elections from 1938 through 1946. The results indicate that the movement had an effect independent of other factors. Model 3, which is similar to model 1, explains

69% of the variance, and the endorsements measure is significant at the .05 level. In model 4, which is similar to model 2, all measures are significant at the .10 level or better—including the measure of Townsend clubs—and the model explains about 67% of the variance.[11]

All in all, the movement and its political activities had a consistently positive effect on pensions in the 1930s and 1940s, despite strong influences of party and state. The findings also suggest that the political activity of the movement had a major impact on the initial size of pensions in 1940, but less of an effect in the next decade. By contrast the organizational strength of the movement had little impact on initial legislation, but was key to the improvement of old-age assistance in the 1940s. Organizational might and political action boosted old-age assistance, but in an uneven and historically contingent way.

Was the movement heard as a legitimate voice for the aged? To answer this, we employ the measure *Townsend memorials*—a sum of state resolutions demanding the national adoption of the Townsend plan (Holtzman 1963, p. 192)—as the best indicator of how politicians viewed the movement.[12] States with unresponsive political parties and systems might discourage memorials, although not as vehemently as they did pensions; state and party champions of high spending might not be as supportive of bids to validate the Townsend plan. We employ the same political model as before, analyzing memorials passed through 1939 and through 1945. The results (in table 4) indicate that the movement was crucial to the passage of memorials. The club measure predicts memorials in both periods. Traditional party organization has a consistently negative effect and administrative powers a positive one.

SUCCESS AND THE TOWNSEND MOVEMENT: A QUALITATIVE COMPARATIVE ANALYSIS

We want to know not only the determinants of pensions and the passage of memorials separately, but also combinations of these outcomes. In some states the movement approached membership in the polity: high

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[11] In these models, the measures of voting rights and per capita income are also updated to 1940. When a lagged dependent measure is introduced, results indicate that the endorsement and club measures have effects on *changes* in pension levels from 1940 to the end of the decade (not shown, but available from authors).

[12] Our measure of recognition is a middle-level one, standing somewhere between "consultation" and "inclusion." Memorials validated the Townsend plan and the Townsend movement as a representative of the aged, but did not ensure full inclusion of the movement on all old-age policies. Thus, nowhere did the movement become a full member of the polity. Full membership would have entailed the passage of a state-level Townsend plan, with the movement involved in its implementation.

TABLE 4

FOUR MODELS OF TOWNSEND MEMORIALS, 1939 AND 1945

INDEPENDENT MEASURES	1939		1945	
	Model 1	Model 2	Model 3	Model 4
Movement model:				
Townsend-endorsed House members	. . .	. . .	.442*** (3.51)	. . .
Per capita Townsend clubs, 1940	. . .	.525*** (4.38)	. . .	.469*** (3.66)
Political model:				
Voting rights	.249** (1.94)	. . .	. . .	. . .
Traditional party organization	−.415*** (−3.36)	−.151* (−1.32)	−.203* (−1.68)	−.167* (−1.36)
Administrative powers	.306** (2.40)	.255** (2.34)	.252** (2.14)	.245** (2.10)
F	8.68	16.22	11.86	12.34
Adjusted R^2	.329	.493	.409	.420

NOTE.—Coefficients are standardized and t-statistics are in parentheses. For definitions of measures, see text. $N = 48$.

* $P < .10$.

** $P < .05$.

*** $P < .01$.

old-age pensions and the acceptance of the movement; elsewhere it was rejected, and old-age assistance remained stingy. Some states gave concessions, as was true at the national level; at the state level, there were instances of co-optation—the movement was recognized, but pensions were low. In appraising combinational arguments, multiple regression can be marred by multicollinearity and the exhaustion of degrees of freedom (Ragin 1987, p. 162).

To address these issues, we employ qualitative comparative analysis (QCA). This examines many combinations of potential causes and generates the simplest combination of them leading to outcomes of interest (Ragin and Drass 1988). We argue that there is more than one path to public spending and that QCA can locate these paths. Yet QCA can employ only a limited number of (categorical) measures. One guide is to use measures from multiple-regression analyses (Griffin et al. 1991). We employ the two movement measures and the political measures of voting rights, traditional parties, administrative capacities, and Democratic party rule. Although these measures imply 64 combinations, many did not exist. For instance, no state lacking voting rights had patronage parties or administrative powers or Townsend clubs. We assume that nonexistent combinations would not have resulted in high pensions or memorials because this would mean that all *logical* possibilities were *socially* possible—too strong an assumption.

To determine which states had high pensions, we take the standardized residuals from the regression of old-age pensions on per capita income, the control measure. Those states with large, positive residuals had the strongest programs because, relative to the state's income level, pensioners were doing better; we do not discount the possibility of poor states having generous old-age assistance. We categorize 17 states as having "high" pensions.[13]

One also must decide how to evaluate "contradictory" cases—a combination of potential causes that sometimes leads to the outcome in question, sometimes not. To code these contradictions as "positive" outcomes is to examine the conditions under which the outcome at hand was *possible*. To consider contradictions as "negative" instances is to locate combinations that *always* led to the outcome. We rely on a third procedure: recoding contradictions by their main tendencies, counting them as positive if more than half of the instances lead to the outcome, negative otherwise, and then reanalyzing the data (see Ragin 1987, pp. 113–18).

Although the mediation model predicts more than one route to high pensions, it also anticipates roadblocks: a lack of voting rights *or* the presence of traditional parties will prevent high pensions. One route implies social movements; in states with voting rights and open parties—where the movement was strong in *both* organization and political action—pensions should also be strong. Generous pensions are also implied by states with open and democratic politics and either Democratic party rule or administrative powers, or both.

The initial analyses locate three possible roads to high pensions (see chart 1). As expected voting rights *and* open parties were necessary for high pensions; alternatively, a nondemocratic political *or* a traditional party system prevented high pensions. Otherwise, one of the routes to

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[13] Our decisions in constructing the six independent measures were determined mainly by theoretical considerations, partly by breaks in the data. For voting rights we took a strong view, discounting only the lowest nine states—all former states of the Confederacy. We counted a state as having traditional parties if it scored three, four, or five on Mayhew's (1986) five-point scale, 13 states qualifying. The 15 states whose labor commissions won rule-making authority before 1920 were scored as having administrative powers. We used a relatively inclusive strategy for the movement: for organization (clubs), the cutoff was 10 clubs for 10,000 persons, with 23 states qualifying. For Townsend-backed House members (endorsements), the standard was 53% of the delegation, a natural break in the data that included 19 states.

CHART 1

PATHS TO HIGH OLD-AGE PENSIONS, 1946–48, QCA ANALYSES

Step 1. An Analysis of Cases in which High Old-Age Pensions Were Possible

VOTING RIGHTS*traditional parties*DEM RULE*endorsements +						
Positive	*TT* Terms:	3	33%	Raw Terms:	3	13%
Contradictory	*TT* Terms:	1	11%	Raw Terms:	2	9%
VOTING RIGHTS*traditional parties*ADMINISTRATIVE POWERS*DEM RULE +						
Positive	*TT* Terms:	3	33%	Raw Terms:	3	13%
Contradictory	*TT* Terms:	1	11%	Raw Terms:	3	13%
VOTING RIGHTS* traditional parties*ENDORSEMENTS*CLUBS						
Positive	*TT* Terms:	0	0%	Raw Terms:	0	0%
Contradictory	*TT* Terms:	4	44%	Raw Terms:	17	74%

Step 2. An Analysis of Cases in which High Old-Age Pensions Were Certain

VOTING RIGHTS*traditional parties*DEM RULE*endorsements*CLUBS +						
Positive	*TT* Terms:	2	50%	Raw Terms:	2	50%
VOTING RIGHTS*traditional parties*ADMINISTRATIVE POWERS*DEM RULE*clubs						
Positive	*TT* Terms:	2	50%	Raw Terms:	2	50%

Step 3. Three Routes to High Old-Age Pensions, with Contradictions Resolved

VOTING RIGHTS*traditional parties*ADMINISTRATIVE POWERS*DEM RULE +						
Positive	*TT* Terms:	4	50%	Raw Terms:	5	31%
VOTING RIGHTS*traditional parties*DEM RULE*CLUBS +						
Positive	*TT* Terms:	4	50%	Raw Terms:	8	50%
VOTING RIGHTS*traditional parties*ENDORSEMENTS*CLUBS						
Positive	*TT* Terms:	4	50%	Raw Terms:	12	75%

NOTE.—A measure in all capital letters indicates its presence; a measure in lower case indicates its absence.

"*TT*" refers to "truth table"; "Raw" refers to the number of actual cases adhering to the combination; DEM RULE refers to Democratic party control. An asterisk (*) indicates *and* in Boolean analysis; a plus sign (+) indicates *or* in Boolean analysis.

high pensions is suggested by political models—a combination of administrative capacities and Democratic party rule. A second is expected by the social-movement model—the presence of organizational strength of the movement *and* success in endorsements.

When the contradictions are recoded, the mediation model is greatly supported. The analysis indicates three routes, each requiring voting rights and an open party system. First there is a "standard political" route through Democratic party rule in the context of administrative powers. Next there is a "movement model" in which both the movement's organizational strength and successful endorsements are required for high pensions. Finally, there is the combination of Democratic party rule and the presence of Townsend clubs.

Although they might discourage the passage of memorials, traditional parties are not expected to preclude them by their presence. After all, memorials cost nothing and might appease the movement. There are three main routes to memorials when contradictions are replaced by main tendencies and only voting rights is necessary in each (results not shown). The first route requires strength in Townsend clubs and endorsements and also Democratic party control and a lack of administrative capacities. The other two require open-party systems and administrative capacities; in one of these, Townsend club strength is also required, indicating that this influenced memorials more than endorsements did.

In the best case for the Townsend movement, a state would have high old-age pensions and would have recognized the movement through the passage of memorials—this was as close as the movement came to becoming a member of the polity. The case of concessions would include a strong assistance program but no memorials. The case of co-optation is the opposite; the movement was recognized through memorials, but assistance was not generous. Finally, in some states the movement failed to be recognized, and pensions were low. "New benefits" means being categorized as having high pensions, and "recognizing" the movement means passing one or more memorials. All told, the movement achieved something like membership in the polity in 12 states (see chart 2). In five states the movement won concessions, while 10 states were cases of co-optation, and in 21 states the movement failed completely. Movement strength in organization and endorsements correlates with the level of success achieved, but does not entirely explain it.

In the analysis of membership, when the contradictions are replaced with their main propensities, QCA supports the mediation model (see chart 3). Polity membership results from movement power in both dimensions, in the context of favorable political conditions. Two paths, which include the bulk of the cases, require both aspects of movement strength, organizationally and through political action. Yet movement strength is not sufficient; one route requires administrative capacities and the second requires Democratic party control. The third path is through Democratic party control and administrative powers, but without Townsend endorsements. A movement can appear to succeed without having put out much effort—the case suggested by the strong form of the opportunity model.

We expect concessions, the more favorable of the partially successful outcomes, to occur where generous pensions do not derive from movement strength in both dimensions, and this is what we find. The two paths to concessions work through democratic political systems, without traditional parties—and both imply Democratic party control. However, the influence of the movement is only partial in each case. One route requires success in endorsements, but weakness in organization; the

CHART 2

TOWNSEND MEMBERSHIP IN THE POLITY, CONCESSIONS, CO-OPTATION, COLLAPSE

Level 1: Twelve States Where the Townsend Movement Approached Polity Membership

1. Colorado*
2. Washington*
3. Oklahoma
4. Arizona*
5. North Dakota*
6. Idaho
7. Wyoming*
8. California*
9. Massachusetts
10. Minnesota*
11. Oregon*
12. South Dakota*

Level 2: Five States Where the Townsend Movement Received Concessions

1. Utah
2. Nebraska*
3. Kansas*
4. Iowa*
5. New Mexico

Level 3: Ten States Where the Townsend Movement Was Co-opted

1. Florida*
2. Montana*
3. New Hampshire
4. Wisconsin*
5. Nevada
6. Michigan
7. Illinois
8. Indiana*
9. Arkansas
10. Georgia

Residual: Twenty-one States Where the Townsend Movement's Prospects Collapsed

1. Louisiana
2. New York
3. Ohio
4. Maine*
5. Missouri
6. Connecticut
7. Rhode Island
8. Texas
9. Vermont*
10. Pennsylvania
11. New Jersey
12. Tennessee
13. Mississippi
14. Alabama
15. South Carolina
16. Maryland
17. North Carolina
18. West Virginia
19. Kentucky
20. Virginia
21. Delaware

NOTE.—For definitions of the three levels of success, see text.
* States where the movement scored high on both membership and endorsements.

other, the opposite. Partial movement strength under favorable political conditions brought benefits but not recognition.

By contrast, we expect that, in the context of favorable circumstances, movement strength in organization only might lead to co-optation. A context of traditional political parties, regardless of movement efforts, can also lead to this minimal level of success. This point of view is supported (see chart 3). One route works through traditional parties and movement strength in both dimensions. Under pressure, patronage parties recognized the movement, but did not pass high pensions. A second route works through strong movement organization, in an open party system with voting rights, but without Democratic party rule or adminis-

CHART 3

Paths to "Membership," "Concessions," "Co-optation," and "Collapse"

Three Routes to Polity Membership for the Townsend Movement

VOTING RIGHTS*trad parties*ADMIN POWERS*ENDORSEMENTS*CLUBS +
Positive *TT* Terms: 2 40% Raw Terms: 5 50%
VOTING RIGHTS*trad parties*DEM RULE*ENDORSEMENTS*CLUBS +
Positive *TT* Terms: 2 40% Raw Terms: 5 50%
VOTING RIGHTS*trad parties*ADMIN POWERS*DEM RULE*endorsements
Positive *TT* Terms: 2 40% Raw Terms: 2 20%

Two Routes Leading to Concessions for the Townsend Movement

VOTING RIGHTS*trad parties*admin powers*DEM RULE*endorsements*CLUBS +
Positive *TT* Terms: 1 50% Raw Terms: 1 50%
VOTING RIGHTS*trad parties*ADMIN POWERS*DEM RULE*ENDORSEMENTS*clubs
Positive *TT* Terms: 1 50% Raw Terms: 1 50%

Three Routes Leading to the Co-optation of the Townsend Movement

VOTING RIGHTS*TRAD PARTIES*admin powers*DEM RULE*ENDORSEMENTS*CLUBS +
Positive *TT* Terms: 1 25% Raw Terms: 1 25%
VOTING RIGHTS*trad parties*ADMIN POWERS*dem rule*endorsement +
Positive *TT* Terms: 2 50% Raw Terms: 2 50%
VOTING RIGHTS*trad parties*dem rule*endorsements* CLUBS
Positive *TT* Terms: 2 50% Raw Terms: 2 50%

Three Routes Leading to the Collapse of the Townsend Movement

voting rights*trad parties*admin powers*DEM RULE*endorsements*clubs +
Positive *TT* Terms: 1 20% Raw Terms: 7 39%
VOTING RIGHTS*TRAD PARTIES*admin powers*endorsements*clubs +
Positive *TT* Terms: 2 40% Raw Terms: 8 44%
VOTING RIGHTS*TRAD PARTIES*ADMIN POWERS*dem rule*endorsements
Positive *TT* Terms: 2 40% Raw Terms: 3 17%

NOTE.—All results are with contradictions resolved into their main tendencies (see text). A measure in all capital letters indicates its presence; a measure in lower case indicates its absence.

"*TT*" refers to "truth table"; "Raw" refers to the number of actual cases adhering to the combination; DEM RULE refers to Democratic party control. An asterisk (*) indicates *and* in Boolean analysis; a plus sign (+) indicates *or* in Boolean analysis.

trative capacities. Although the third route—administrative powers in the context of a democratic political and open party system—is difficult to appraise, overall the analysis of co-optation provides support for the mediation model. The causes of movement collapse mainly parallel those of membership. The QCA locates two routes through traditional parties and a failure of endorsements. The third route involves the absence of voting rights, traditional parties, administrative capacities, both movement dimensions, *and* the presence of Democratic party rule—the typical Southern state.

DISCUSSION AND CONCLUSION

Using several comparative strategies can help to make theoretical sense of a case study (Amenta 1991). Comparisons over time and across policies and states helped to answer different questions about the influence of the movement. Our approach also suggests that studies of the influence of movements need to take into account the determinants of movements as well as their organization and actions. These determinants may influence what seem to be successes won by the movement. More than that, however, studies of the development of public spending policies need to pay attention to the different routes to them (Therborn 1989). States could reach high old-age spending not only by standard statist and party ways, but also through routes paved by the Townsend movement.

For state bureaucracies and left-center parties to aid a movement, its constituency needs to be electorally desirable, and the movement cannot have "displacement" goals (Gamson 1990, app. A). The aged were a desirable constituency, but the Townsend plan sought to displace the administration's programs, limiting its influence. Although the movement made some notable gains, it never won membership in the polity and did not survive postwar prosperity. Old-age insurance, the program that preempted the name "social security," has perhaps raised the aged to a status where legislation and bureaucratic and fiscal practices combine to enforce their interests, but different forces won these victories (Derthick 1979). The movement aided those outside the system—those in the generation of the movement's activists—by upgrading old-age *assistance,* the main form of public social provision for the aged until 1950.

Analyzing the causes of social-spending policies, we find some support for economic, political, and movement models. Our analyses also indicate that the American experience can be examined with models based on European cases. With respect to social movements, this article provides evidence that movements need to be strong both organizationally and in standard politics to gain recognition and benefits for their supporters. Endorsements need organizational backing to make them effective, and organizations without political support are vulnerable. Strategic and tactical diversity can only aid a movement.

Yet the impact of social movements is mediated strongly by political conditions. Nowhere was the movement able to achieve both new benefits and recognition without favorable conditions in the polity. Among long-term political circumstances, democratic rights and open parties had gatekeeping roles. The key middle-range condition concerned state bureaus promoting public protection, and the main short-term condition concerned the rule of a Democratic party that in the 1930s and 1940s was almost a functional equivalent of a labor party on the European model (Greenstone 1969). Both facilitated movement gains. In states where the political or party system thwarted the goals of the movement, people contributing to the movement got little for their dues. By contrast, in states where politics were democratic, parties were open, and state or party sponsors were available, the movement did win advantages. Townsend was a hero for the aged but only on some issues, and his movement required help.

The results also question the degree to which the American political system has been "open" to the influence of social movements (Kitschelt 1986). As a system, it was simultaneously open and closed. Some parts of the system encouraged movements and helped them to succeed. Other parts—those without voting rights and open party systems—discouraged movements and prevented them from succeeding. Representatives of national-level nondemocratic political systems and traditional party systems made it difficult for movements to achieve more than partial victories. Multiple points of entry into the system make it seem more open to influence than it has been. The extension of voting rights throughout the nation and the decline of the traditional party organizations have opened the system more. Yet the expanding role of campaign money in American politics in the past two decades may have a similar discouraging and dampening effect on the efforts of movements (Edsall 1984).

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Young White Adults: Did Racial Attitudes Change in the 1980s?[1]

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The widespread belief that racism among young white adults has increased in the 1980s is scrutinized using 12 racial policy questions from the General Social Surveys and the National Election Studies. Under the assumption that age effects can be treated as negligible, the article evaluates the importance of period and cohort effects in shaping the present racial attitudes of adults who have come of age since 1959. After outlining two possible patterns of differences among cohorts that could have resulted from the impact of historical circumstances during the formative years of early adulthood, the article concludes that there is no indication of decreasing tolerance among cohorts coming of age in the 1980s. Similarly the period effects are seldom significant over the years from 1984–90 and thus show no consistent decline in racial liberalism.

According to many commentaries, a resurgence of racism occurred in the United States in the late 1980s (see, e.g., Szulc 1990). Manifest first on college campuses, racial incidents have also occurred in a number of metropolitan areas, especially among working-class youth. Yet to scholars who use national surveys to study racial attitudes these examples of racial strife seem to conflict with broader empirical evidence. Over the last 25 years, the attitudes of the general population of white adults have steadily become more supportive of black advancement on most, although not all, issues (Schuman, Steeh, and Bobo 1988; Firebaugh and Davis 1988).

However, no research to date has systematically examined very recent data or explored the possibility, so reasonable in light of the highly publicized racial incidents of the last few years, that there has been a reversal in attitudes primarily among young adults growing up in the 1980s long

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after the civil rights movement of the early and mid-1960s. Furthermore, few scholars have examined attitude trends toward racial policies, such as affirmative action (for the most cogent discussion to date, see Sigelman and Welch [1991]). To address these issues, we assess the direction of change in racial attitudes from 1984 through 1990 among white adults who reached the age of 18 after 1959. We focus on 12 important racial policy items that were asked at least twice in national surveys between 1984 and 1990.

CAUSES OF CHANGES IN RACIAL ATTITUDES

The three most frequently cited causes of changes in public attitudes are life-course factors associated with aging, cohort or generational differences among groups of people who share a common life event such as birth date, and changes in individual attitudes produced by events or conditions that occur near the time of measurement, that is, by period effects.

The bulk of previous research on racial attitudes has found that both changes in individual attitudes and changes occasioned by differences among cohorts contribute positively to the development of racial liberalism in the last half of the 20th century (Schuman et al. 1988; Smith 1981, 1985; Davis 1975). The reports of an upsurge in racism specifically among young white adults during the 1980s naturally raises the possibility of important cohort factors in racial attitude change.

As early as 1956, in the first of several articles to explore attitudes toward desegregation, Hyman and Sheatsley (1956) sketched a generational model of positive change. In their view, a process of constant liberalization occurs as older, less tolerant generations are replaced by new generations that have been socialized to be less hostile than their parents to racial integration. In addition, successive generations of Americans have attained higher levels of education, and Hyman and Sheatsley assumed that education, on its own, would liberalize thinking about race. The mutually reinforcing effects of cohort and education on tolerance, noted also in the work of Myrdal (1940) and Stouffer (1955), led Hyman and Sheatsley to liken the process to an ever-improving spiral that would not "soon, if ever, reverse itself" (1956, p. 39). In their model of racial attitude change, Hyman and Sheatsley did not pinpoint specific ages as particularly formative for racial attitudes but implied a learning process that stretches from childhood at least through early adulthood. Over the intervening years this now-classic formulation has assumed a prominent place in the literature on racial attitude change since it seemed to fit well the gradually liberalizing trends revealed by national surveys (Hyman and Sheatsley 1964; Greeley and Sheatsley 1971; Taylor, Sheatsley, and Greeley 1978; Hyman and Wright 1979).

Recently, however, sociologists have begun to define cohort effects in a more specific way. Starting from the suggestions of Mannheim ([1928] 1952), much work of the last several years has stressed the impact of specific historical events and changes on the character of generations. Public events experienced at a "critical period in the life course," usually defined as the years of adolescence and early adulthood, may produce a distinctive set of attitudes in an entire generation that persists throughout its lifetime (Roberts and Lang 1985; Schuman and Scott 1989). Sharing great events and changes supposedly creates a distinct generational consciousness. Thus we speak and read about the Great Depression generation (Elder 1974), the sixties generation, or the generation of young Nazis in prewar Germany (Weil 1987; Braungart and Braungart 1990).

It is possible that racial attitudes—especially policy attitudes—have been influenced by similar types of effects, since the national population can be thought of as consisting of cohorts that differ in the racial events they experienced during their adolescence and early childhood. Some cohorts went through this potentially critical life period at the height of the civil rights movement when legal segregation was successfully challenged; some, during the later years of the urban riots, the busing controversy, and the Nixon period of benign neglect; and some—the youngest now available for survey interviews—during the Reagan era with its rising hostility to affirmative action.and its increasing focus on inner-city crime and deterioration.

With these various generational effects in mind, we can conceive of several patterns of cohort differences, with a cohort defined as all the people who became 18 in the same year.[2] Our particular concern with possible changes in the racial attitudes of young adults coming of age during the 1980s leads us to hypothesize that one of two patterns of cohort differences is most likely when, for reasons explained below, we use 1960 as our starting point:

1. A pattern of positive, linear differences indicates that racially liberal attitudes have increased monotonically even across the cohorts of the 1980s (the Hyman and Sheatsley [1956] prediction).
2. An inverted U-shaped curve shows racially liberal attitudes have increased across the cohorts of the 1960s and 1970s but have decreased as cohorts came of age in the Reagan years.[3]

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[2] We begin at 18, the age generally accepted as the beginning of adulthood, rather than earlier because we are bound by the definitions used to draw the samples of the National Election Study (NES) and the General Social Survey (GSS).

[3] A variation of this hypothesis—that positive differences among the 1980s cohorts may have decreased but not reversed—can be evaluated by testing for a much gentler curve than the inverted U-shape. However, the test for curvilinearity we employ may

If the changes that have taken place in American society as a result of the desegregation begun in the 1950s and 1960s continue to socialize young adults in the way Hyman and Sheatsley posited, then pattern 1 would provide the better fit to our data. If, however, the negative public events and the atmosphere of the 1980s have spawned a post-civil-rights generation of young adults divergent enough in their outlook to shift the attitudinal trend to a downward course, we would expect to find that pattern 2 is more characteristic of our data. Because there are differences in the content of the questions that we will describe below, we expect that some issues might display pattern 1, but others should definitely display pattern 2 if there has indeed been any substantial reversal of attitudes among young white adults. We also consider below a possible interaction of period and cohort that might indicate negative intracohort trends, even though the pattern of intercohort differences is consistently positive.

THE COHORT-PERIOD MODEL AND THE VARIABLES EMPLOYED

It is important that research on cohort effects be conducted within the framework of an explicit age-period-cohort analysis. If life-course ("aging") factors also lead to changes in racial attitudes, the estimation of separate cohort and period effects becomes highly problematic (Glenn 1976, 1989; Fienberg and Mason 1985; Rodgers 1990). The question of to what degree aging may help explain changes in racial attitudes must be addressed directly.

Two hypotheses link the process of growing older with increasing social and political conservatism. The more widely accepted one states that, as individuals reach late middle age, they become more conservative as the result of a number of physiological, psychological, and social factors (Sears 1983). Davis (1975) confronted this hypothesis directly and found no evidence to support it, although he did not carry out a full-scale age-period-cohort analysis. More recently, Danigelis and Cutler (1989) have studied intracohort trends for cohorts born before 1939 and have concluded that aging is not accompanied by "either increasingly rigid or increasingly conservative attitudes with respect to race relations" (1989, p. 19; also see Danigelis and Cutler 1991). More general research on attitude change at the individual level uses panel data and covers a multitude of social and political topics and finds that members of the

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be too weak to detect this possibility, and, though our test for cohort-period interactions described below should pick up signs of such an effect, a rigorous test that treats the 1980s cohorts in a separate analysis must wait until more complete data exist for cohorts coming of age after 1987. See n. 5 below.

oldest age groups (58–65 and 66–83) are no more nor less likely to change their attitudes than any other age group over 33 (Krosnick and Alwin 1989). Yet another study using both cross-sectional and panel data concluded that "older people are as capable of attitude change as younger people" when they are exposed to "change-inducing experiences" (Tyler and Schuller 1991).

The second hypothesis also makes a connection between aging and conservatism but much earlier in the life course. The supposed idealism of youth gives way by early middle age under the pressures of marriage, parenthood, and work—for example, the radical Yippie who becomes a stockbroker. If this aging effect indeed exists, we would expect intracohort trends on social and political attitudes to show a noticeable decline in liberalism between young adulthood and early middle age. However, previous data on racial attitudes indicate that cohorts, regardless of birth date, became more, not less, integrationist over the period from 1972 to 1990 (Schuman et al. 1988).

Since there is no convincing evidence that either of these life-course effects operates as hypothesized, we are willing to assume explicitly that aging effects for racial attitudes are negligible and can be excluded from our interpretations. We have taken the precaution, however, of eliminating those cohorts who would be older than their late forties by 1980 in order to guard against the possibility that they may be more susceptible to influences associated with aging than are younger cohorts. Thus, our analysis includes only those cohorts coming of age during the 1960s, the 1970s, and the 1980s.

The analytic model that we have constructed from multiple cross-sections (Fienberg and Mason 1985) assumes that racial attitude trends are composed, therefore, only of cohort and period effects. The year between 1984–90 in which an attitude was measured represents possible period influences. Since not much change is likely to have occurred over a span of only seven years, we expect that the estimated values for this variable will be small or even close to zero. Cohorts are defined by the year in which an individual became 18, and their main effects should conform to either pattern 1 or 2 previously described above. As a test for curvilinearity in the relationship of cohort to racial attitudes, we employ a quadratic term, cohort squared.

Additional Variables Needed

Since background factors may affect our separate estimates of these effects, we include several additional variables in the model. Thus, our strategy differs from designs that concentrate exclusively on period and cohort (Firebaugh and Davis 1988). In addition, because the assumption of additivity may not hold for racial attitudes, we search for interactions that have strong substantive meaning or have assumed importance in other research (Glenn 1976; Schuman et al. 1988). The interaction that concerns us especially is cohort paired with period, but we also examine the interactions of cohort with both region and education and of period with region.

Significant interactions of cohort with education or region would suggest that the cohort effects are not general but vary within important subgroups of our study population. Thus a reversal in the upward generational spiral may be typical of the 1980s cohorts in the North but not in the South or among the highly educated but not among high school graduates. By the same token, period effects may differ across the two regions, with trends being negative in the North but positive in the South, for example. A negative interaction of period and cohort would indicate that the attitudes of cohorts coming of age in the 1980s have been adversely affected by the policies of the Reagan administration, an outcome we would expect if individuals still in the formative years of late adolescence and early adulthood are more sensitive to contemporaneous influences than individuals who have passed that life stage (Mannheim 1952). Thus even when cohort differences are positive for a question, period effects within cohorts may show that only the most recent cohorts have become less and less liberal over the years 1984–90.

The background variables in our model include education, region of residence, gender, and family income.[4] With recent research in mind, we hypothesize main effects for each of these variables. Although education, the most important factor, has usually been related positively to racial attitudes (Schuman et al. 1988), some evidence suggests that additional years of schooling do not always liberalize attitudes toward racial policies (Jackman 1978; Jackman and Muha 1984; Weil 1985). For this reason, we expect that education in these analyses will have only a weak positive—or perhaps no—effect upon racial tolerance. In addition, controlling for formal education turns the cohort variable into a purer measure of other socializing experiences, such as the quickening pace of integration, that differentiate cohorts.

We use region to represent the influences that different subcultures exert upon adolescents and young adults, as there is a good deal of evidence that Southerners are socialized to be less liberal on racial issues than Northerners (Taylor et al. 1978; Schuman et al. 1988; Wilson 1986).[5] Scattered evidence suggests that women as a group are more supportive than men of policies that aid the poor, the unemployed, the sick, and people in need (Cook 1979; *Public Opinion* 1982; Baxter and Lansing 1983; Shapiro and Mahajan 1986), and we expect that, in a multivariate analysis, the positive effect that being female has on racial attitudes will become even more pronounced. Our predictions about the relationship of income to racial attitudes are mixed. On the one hand, the link between income and education leads to the expectation that the effect of income, like the traditional effect of education, would be positive. On the other hand, rational choice theory suggests that opposition to social programs that would require additional taxes might grow as income increases.

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[4] Education is represented by a five-point scale designed to capture the major educational milestones (1 = less than graduation from high school, 2 = high school graduation, 3 = some college, 4 = college graduation, and 5 = more than a college education). The level of education among the subsample of adults we are studying is very high, averaging about 13 years for both the GSS and the NES. The definition of South is based on Census Bureau categories and encompasses the South Atlantic and both the East and the West south central regions. North refers to the states in the remaining regions except Alaska and Hawaii whose residents are not sampled.

Analytic Approach

We use ordinary least squares (OLS) multiple regression as our method of analysis. This permits us to carry out a hierarchical analysis by which the quadratic and interaction terms can be added to the model one by one (Cohen and Cohen 1983; Jaccard, Turrisi, and Wan 1990). If the increase in the explained variance is not significant at or beyond the .025 level, the variable is dropped from the model. Only the final model for each question will be presented here.[6] The OLS regression produces results that are valid for dichotomous dependent variables when their distributions are not badly skewed, but, as a check, we repeated the analyses using logistic regression.[7] For half the questions that we considered, all the results were the same as those obtained from OLS. For the remaining six questions, there were some minor differences that we will note at the appropriate points in the discussion of our results. Far from contradicting any of our conclusions about the effects of cohort and period, however, these differences provide even stronger support for our findings.

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[5] Using region as a background variable does bring up the issue of controling for migration effects on interregional differences in racial attitudes. Several findings support our decision not to do so. First, the amount of this kind of migration in our GSS subsample of adults does not appear to be substantial, with only 7% of the residents in the combined cross-sections living in the other region when they were 16 years old. Furthermore, in a study similar to ours, whether migrants were included or excluded made little difference to the analysis of period or cohort effects (see Firebaugh and Davis 1988). We also recognize that some recent analysis indicates region may be a surrogate for a number of compositional factors that differ in their geographic distribution such as urbanization and percentage of the population that is black (Fossett and Kiecolt 1989).

[6] Since we do not adjust for the larger design effects of multistage area probability samples, we adopt a more restrictive level of significance than usual, that is, .025 instead of .05. The sizes of the cohorts coming of age from 1984 to 1990 in the GSS are extremely small in their first year, and, as a result, the distribution of responses for a cohort in that year may be quite unrepresentative. Because regression coefficients can be influenced by outliers especially at the beginning or end of a series, we have taken two steps to ensure that this aspect of regression, has not influenced our results. We have, as a first strategy, carried out parallel hierarchical analyses, one based on the full set of cohorts and the other excluding the three most recent cohorts. When there were no differences in the two regressions, we have reported the statistics for

THE DATA

Samples

Our measures of racial attitudes come from two sources: the National Election Studies (NES) and the General Social Surveys (GSS). Together these sources provide a wide range of racial attitude questions covering many topics.[8]

Both the GSS and the NES in the 1980s are based on national, full probability, face-to-face samples of people living in households in the coterminous United States who are 18 years old and older. (The NES confines its samples to citizens, and the GSS, to adults who speak English.) Our study population includes only those adults explicitly designated as white. However, for both the NES and GSS in these years the classification included Hispanics but not American Indians or Asians.

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the full set of cohorts. As a further step, we have tried to address more directly the possibility of outliers by converting the first year's data for each of the cohorts from 1984 to 1990 to missing values and recalculating the model chosen in the hierarchical analysis. In no case did this conversion change any of our results.

[7] The split of 76% opposed to 24% in favor of busing for the 1985 GSS represents the most uneven distribution among our dependent variables (see table 1 below), but 10 out of the 12 items we analyze are scales with more than two categories. Seven of the 10 have at least four categories.

[8] As indicated in table 1 below, coverage over time for most of the attitudes we treat in this study has been sporadic at best. Although the GSS is conducted annually, a plan by which core questions were rotated among years before 1988 has ensured that only one of our questions was asked in every survey. Coverage over time in the NES has been more uneven (Abramson 1990). This means that sample *N*'s across the set of years covered by our study will vary substantially depending on how many times a question was asked from 1984 to 1990. In addition, split-ballot experiments conducted during these years further reduced the sample *N*'s for AID TO BLACKS, AID TO MINORITIES, and SPENDING ON BLACKS. NO GOVERNMENT GUARANTEE was also the subject of an order experiment in the 1988 NES, but, since no significant order effects appeared for this question ($\chi^2 = 2.57$; $df = 4$; N.S.), we combined the data from the two forms. Although sample sizes for individual NES studies exceed those for GSS surveys, the overall *N* for GSS items will be larger since the GSS is an annual rather than a biennial survey.

There is one limitation in these data for our purposes. The exclusion from household samples of persons living in college dormitories may raise doubts about the relevance of our findings to the current racial situation on campuses. To the extent that liberalism is declining primarily among college students in the 1980s, we may be underestimating negative cohort or period effects. Our own data suggest, however, that we are obtaining a substantial portion of college students since, in the GSS sample, 27% of the cohorts coming of age 1985–90 (20% in the NES) gave their work status as "student." This percentage approaches the 1988 national college-going rate of 38.7% for white adults 18–24 years old (National Center for Education Statistics 1990).[9]

Attitude Questions

Nine of the 12 GSS and NES racial policy questions chosen for analysis fall into the category of "implementation questions" that deal with "approval or disapproval of steps the government might take to combat discrimination or segregation or to reduce racial inequalities in income or status" (Schuman et al. 1988, p. 86). The other three questions tap concerns about the relatively new issue of affirmative action, which can be conceptualized as a special type of implementation. These questions, therefore, form a logical group and should, in light of earlier research, be the most likely of all racial-attitude items asked in continuous national surveys to provide evidence that racial tolerance is declining.

From the 1960s to the early 1980s, attitudes on most implementation issues did not move in a liberal direction as did attitudes toward social distance and principles of racial equality (Schuman et al. 1988). Thus it seems reasonable to suppose that in a period of apparent retrenchment on civil rights, questions about government implementation of desegregation and affirmative action—particularly the latter since it has increasingly come under attack—would be the most likely to reflect decreases in liberal attitudes. Thus these questions involve precisely the kinds of issues that tap the racism supposedly exhibited by young adults in the 1980s (Sears and Kinder 1985; Sears 1988).

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[9] There are several reasons why the coverage is better than might be expected. Students at commuter schools such as community colleges are eligible for the sample as are students who live in private housing at large universities. Living off campus increases dramatically with class year, if a student survey taken at a midwestern university is typical (Office of Financial Aid 1987). First-year students normally live in dormitories, but, by their junior year, the vast majority have switched to private housing. Thus, even though our samples do underrepresent college students as a whole, the problem is mainly for the youngest students and for those who attend small, residential schools. Although there continue to be racial incidents on college campuses of all sizes, the problem certainly extends beyond first-year students and indeed beyond academia to other social contexts.

The logic of treating these 12 questions as, on the whole, representing racial policy issues should not obscure the fact that there are substantial differences among them, differences that are only partially captured by the labels "implementation" and "affirmative action." Two of the questions deal specifically with spending money on programs that assist blacks, two picture the government as acting to guarantee that blacks have equal opportunity and the right to live wherever they want, and four give the government the more positive role of making every effort to improve the general social and economic position of blacks and minorities and to ensure fair treatment in jobs. Under affirmative action, we have included two questions involving preferential treatment in education and employment. The third question compares the efforts of blacks with those of white immigrant groups and only implicitly refers to affirmative action policies on race.

In the subsequent analyses, we shall point out how some of these distinctions may have affected our results. We expect that questions dealing with more traditional government efforts to promote equal opportunity and nondiscrimination (e.g., the NO GOVERNMENT GUARANTEE and OPEN HOUSING items; see the Appendix) should be the most likely to show a pattern of cohort differences that fits a linear, positive model, since these issues have not been explicitly challenged by the Reagan and Bush administrations. However, questions that deal with government efforts to promote affirmative action or to provide, in any way, special favors to blacks or minorities should be most likely to show cohort differences that fit the inverted U-shaped pattern, since these policies have come under repeated and relentless attack by conservatives in Washington and elsewhere. Of course, the most compelling case for change either one way or the other would occur if all questions conformed to the same pattern of cohort differences.

The full wording of each question, with any variants, appears in the Appendix.[10] There, and in all reported analyses, the responses to each question have been ordered from least to most liberal. Volunteered responses, such as "Don't know," that usually account for about 5%–8%

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[10] The items AID TO BLACKS and AID TO MINORITIES differ only in the groups to which they refer, as the mnemonics indicate. The distinction apparently is a meaningful one since the average score is significantly lower for AID TO BLACKS ($P <$ .01) in both 1986 and 1988, the only years in which both questions were asked in a split-ballot design. In addition, the regression analyses for the two questions indicate a more complex set of relationships for AID TO BLACKS than for AID TO MINORITIES. As a result, we have retained both questions and treated them as separate issues (see Anderson, Silver, and Abramson 1988).

of the total number of responses in each year for most items, have been eliminated.

RESULTS

Main Effects of Cohort and Period

Table 1 reports the basic distribution of responses for the twelve racial policy questions at each time point between 1984 and 1990. The question labels used in this table will be retained throughout the remainder of the analysis.

The gamma statistic in the last column of table 1 indicates the bivariate association of a particular racial attitude with year of survey (period). Seven of the items in this table show an overall trend that is significant beyond the .025 level ($\gamma/SE > 2.24$). The direction of change, however, is not uniform: four questions move significantly in the direction of favoring integration or of giving assistance to blacks (NO GOVERNMENT GUARANTEE, BUSING, OPEN HOUSING, and HELP BLACKS), and three move significantly in the opposite direction (IN JOBS, AID TO MINORITIES, SHOULD OVERCOME PREJUDICE). Before trying to make sense of these divergent trends, we must take into account patterns of cohort differences that may influence our conclusions about the predominant direction of change. Indeed, until we estimate our full model, we cannot be sure that the remaining five questions in table 1 exhibit little or no change, since positive cohort effects may, in fact, be operating to cancel negative period influences.

In table 2, and in table 3 below, we present the results of the last step in a hierarchical OLS regression analysis for each of the 12 implementation and affirmative action questions. The two tables separate the independent variables into two groups. Table 2 presents the cohort and period effects and their interactions. Table 3 shows the effects of the demographic variables. Placed side by side, the two tables give all the unstandardized coefficients, the R^2s, and the total N's for each regression.

The main effects of cohort shown in column 2 of table 2 are almost all positive and for two of the eight questions without interactions (AID TO MINORITIES, HELP BLACKS) they are clearly significant ($P < .025$).[11] In addition, two of the three questions with interactions involving region or education have significant positive cohort effects among subgroups: Southerners in the case of OPEN HOUSING and the least educated in the case of BUSING.[12] The one remaining question (SPENDING ON BLACKS) has a highly significant positive coefficient for cohort, but the interaction between cohort and period requires that we treat this item below.

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[11] In the logistic regressions slight differences occur in the significance levels of the cohort coefficients for two questions. NO GOVERNMENT GUARANTEE, whose positive coefficient just misses significance in the OLS regression ($P < .05$), shows clearly significant cohort effects ($P < .025$) in the corresponding logistic regression. AID TO MINORITIES, on the other hand, has the opposite pattern—a clearly sig-

At the same time, the nonsignificant cohort effects for seven questions in table 2, though mainly positive, must be treated as indicating no differences among cohorts on these issues. Thus we find that the clearest patterns in table 2 are (1) increasing racial liberalism across the cohorts of the 1960s, the 1970s, and the 1980s on certain issues and (2) no change on other issues. It is important to note that all three of the affirmative action questions fall within this latter group.

Table 2 does not include a column for quadratic effects because none of the 12 items yielded a significant ($P < .025$) effect for the cohort-squared term. Since the absence of curvilinearity is pivotal to our analysis, we also recalculated the OLS regressions by grouping cohorts into categories that could be treated as a set of dummy variables. The pattern of differences among these cohort groups was examined for nonlinearity either in the sense of a reversal in racial attitudes among the youngest adults (the inverted U-shape) or a narrowing of the distances among the youngest cohorts (a curve that rises and then begins to level off). For almost every question, the coefficients for the two most recent cohort groups (adults coming of age from 1978 to 1990) indicated that they were usually more liberal on racial attitudes than the older cohort groups. Since the results from the dummy variable regressions corroborate the results of the OLS regressions that added a cohort-squared term, we conclude that cohort differences over the period from 1984 to 1990 cannot be described for any question in terms of either an inverted U-shape or a curve that levels off, whether it involves issues of traditional implementation or newer notions of affirmative action.

The main effects of period are not nearly as uniform as they are for cohort. Two questions (AID TO MINORITIES and SHOULD OVERCOME PREJUDICE) have highly significant, negative period effects from 1984 to 1990. These items are balanced by one question (NO GOVERNMENT GUARANTEE) with an overall positive period coefficient that is highly significant, one question (BUSING) with a highly signifi-

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nificant, positive cohort effect in the OLS analysis and an effect of borderline significance ($P < .05$) in the logistic regression.

[12] In addition, the cohort effect for AID TO BLACKS, which refers to white Southerners, is almost significant ($P < .04$) in the OLS regression and highly significant ($P < .01$) in its logistic counterpart.

TABLE 1

Changes in Racial Attitudes, 1984–90: Responses to Implementation and Affirmative Action Questions for Cohorts Coming of Age since 1959

Racial Policy Questions by Type	1984	1985	1986	1987	1988	1989	1990	γ (γ/SE)
Implementation:								
NO GOVERNMENT GUARANTEE (%) (NES):								
Agree strongly	. . .		26.4		16.9			
Agree somewhat	. . .		37.0		30.2			.257*
Neither	. . .		12.2		13.3			(7.975)
Disagree somewhat	. . .		16.7		25.8			
Disagree strongly (*L*)	. . .		7.6		13.7			
OPEN HOUSING (%) (GSS):								
Homeowner can decide	40.3	. . .	42.1	40.4	34.7	31.7	38.0	.036*
No discrimination (*L*)	59.7	. . .	57.9	59.6	65.3	68.3	62.0	(2.692)
AID TO MINORITIES (NES):								
Mean of seven-point scale (no interest = 4)	3.8		3.8		3.4			−.103* (−3.843)
AID TO BLACKS (NES):								
Mean of seven-point scale (no interest = 4)	. . .		3.3		3.1			−.095 (−2.095)
HELP BLACKS (%) (GSS):								
Strongly disagree	34.5	. . .	34.1	32.1	35.4	32.5	28.8	
Disagree	23.0	. . .	24.6	24.0	22.4	24.2	21.2	
Both agree and disagree	27.7	. . .	28.6	27.6	27.7	26.9	33.2	.045* (2.635)
Agree	9.2	. . .	8.7	11.6	10.4	11.0	11.3	
Strongly agree (*L*)	5.5	. . .	4.0	4.7	4.1	5.4	5.4	

FEDERAL JOB INTERVENTION (%) (NES):								
Not government's business	. . .		28.2		27.0			−.017
No interest, depends	. . .		36.2		40.0			(−.364)
Government see to it (*L*)	. . .		35.6		33.0			
BUSING (%) (GSS):								
Oppose	. . .	76.1	70.1	. . .	67.7	69.8	65.9	.109*
Favor (*L*)	. . .	23.9	29.9	. . .	32.3	30.2	34.1	(3.904)
FEDERAL SPENDING (%) (NES):								
Decreased	27.2		19.4		26.2			.035
Same	60.0		61.8		57.9			(1.248)
Increased (*L*)	12.8		18.7		15.9			
SPENDING ON BLACKS (%) (GSS):								
Too much	17.5	24.2	16.7	14.0	19.6	16.0	17.1	.023
About right	47.9	44.3	52.0	52.8	45.9	52.1	48.9	(.900)
Too little (*L*)	34.5	31.5	31.4	33.2	34.5	31.9	34.0	
Affirmative action:								
IN EDUCATION (%) (NES):								
Strongly against	. . .		46.9		49.5			
Against	. . .		21.9		20.9			−.030
For	. . .		18.8		16.0			(−.671)
Strongly for (*L*)	. . .		12.4		13.6			
IN JOBS (%) (NES):								
Strongly against	. . .		62.7		70.5			
Against	. . .		21.9		16.6			−.141*
For	. . .		9.9		7.0			(−2.733)
Strongly for (*L*)	. . .		5.4		6.0			
SHOULD OVERCOME PREJUDICE (%) (NES):								
Agree strongly	. . .		27.7		39.9			
Agree somewhat	. . .		32.8		32.1			−.214*
Neither	. . .		16.4		11.6			(−5.368)
Disagree somewhat	. . .		18.1		13.7			
Disagree strongly (*L*)	. . .		5.0		2.7			

NOTE.—(*L*) = most liberal response.

* $P < .025$.

TABLE 3

EFFECTS OF DEMOGRAPHIC VARIABLES, CONTROLLING FOR COHORT AND PERIOD, 1984–1990, FOR COHORTS COMING OF AGE FROM 1960 TO 1990

RACIAL POLICY QUESTION	CONSTANT[a]	COHORT	PERIOD	INTERACTIONS	
				Cohort	Period
Implementation:					
NO GOVERNMENT GUARANTEE[b]	1.15	.009	.237***	. . .	. . .
OPEN HOUSING[b]	1.25	.009***	.006	−.006* (North)	. . .
AID TO MINORITIES[b]	3.33	.012*	−.064**	. . .	. . .
AID TO BLACKS[b]	2.51	.027	−.056	−.042** (North)	. . .
HELP BLACKS[b]	1.67	.008**	.018	. . .	. . .
FEDERAL JOB INTERVENTION	1.78	.004	−.004	. . .	. . .
BUSING[b]	1.12	.015***	−.003	−.003* (Education)	0.024** (North)
FEDERAL SPENDING	1.85	.002	.013	. . .	. . .
SPENDING ON BLACKS[b]	1.63	.019**	.046*	−.003* (Period)	. . .
Affirmative action:					
IN EDUCATION	1.91	.003	−.011	. . .	. . .
IN JOBS[b]	1.79	.005	−.046	. . .	. . .
SHOULD OVERCOME PREJUDICE	1.89	−.001	−.153***	. . .	. . .

NOTE.—Variables: cohort = 1–31 (1960–90); period = 1–7 (1984–90).

[a] The reference group is Southern males.

[b] The analysis includes only cohorts coming of age from 1960 to 1985 for the NES and 1960 to 1987 for the GSS.

* $P < .025$.

** $P < .01$.

*** $P < .001$.

cant, positive period effect in the North,[13] and one question (SPENDING ON BLACKS) that moves from significant, positive period effects for the oldest cohorts to significant, negative period effects for the youngest cohorts as a result of the negative interaction of period with cohort. In the majority of cases, however, the effects, though more likely to be negative than positive, are not significant. Thus when variables representing both period and cohort are in the same model, fewer period effects than cohort effects are significant, and the period effects, unlike the consistently positive cohort effects, are mixed in direction. That the affirmative action items show a consistently negative relationship to period fits well with our understanding of popular attitudes on this issue during the 1980s. In fact, of all questions, the most highly significant negative effect is for SHOULD OVERCOME PREJUDICE, which shows that an indirect reference to affirmative action may be the most powerful.

The picture of period change that we obtain from the multivariate analysis of table 2 differs from that initially suggested by the bivariate percentages in table 1. Of the seven questions with significant gammas in table 1, only three (NO GOVERNMENT GUARANTEE, AID TO MINORITIES, and SHOULD OVERCOME PREJUDICE) continue to show significance in the multivariate context, and two of the three are based on a comparison of only two NES surveys covering the short time span from 1986 to 1988. In addition, one question (BUSING) now shows period change only in the North, and another (SPENDING ON BLACKS), which had not appeared to change over the years from 1984 to 1990 in the bivariate table, is characterized by period change that varies depending on whether older or younger cohorts are being considered. Thus finding fewer period effects in the multivariate analysis supports our expectation that not much individual attitude change would occur over the short time span (1984–90) we examine here.

The significant period effects that do result from the multivariate model seem to us to confirm distinctions among the survey questions based on content. The one question with a significant positive period effect focuses on governmental efforts to ensure equal opportunity (NO GOVERNMENT GUARANTEE)—a classic issue of the civil rights movement and the Supreme Court under Chief Justice Earl Warren. On the other hand, the items that exhibit negative period effects imply governmental action that is preferential toward blacks and minorities

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[13] The logistic regression for the affirmative action question IN JOBS also shows a significant negative interaction of period with northern residence. In this case, all cohort groups in the North are becoming less willing to give blacks preference in hiring and promotion during the period 1984–90. Again this result is compatible with our conclusions.

(AID TO MINORITIES and SHOULD OVERCOME PREJUDICE). Thus, it appears that there has been some tendency for attitudes about issues of preferential treatment to become less liberal in the mid- to late 1980s, though we must note that the tendency does not show up at present on all such items. The absence of interactions involving cohort or period for the three questions with significant period effects points to general trends within the entire population considered here, rather than just within the cohorts of the 1980s.

Interactions Involving Cohort or Period

As we have seen, the interaction terms in columns 4–5 of table 2 have influenced our conclusions to some extent for the questions involving implementation issues other than affirmative action. However, the impact has been slight since only five out of a possible 48 interactions (four hypothesized interactions × 12 questions) reach significance beyond the .025 level.

The two negative interactions of cohort with northern residence have slightly different impacts. For OPEN HOUSING the interaction reduces the strength of the cohort effect in the North to a level that is not significant, and for AID TO BLACKS the interaction converts a slightly positive effect in the South (.027) to a slightly negative and nonsignificant effect in the North (−.015). The interaction of cohort and education on BUSING indicates that, although the cohort effect weakens as education increases, it does not become negative. The interaction of period with northern residence, also characteristic of BUSING, means that over the years 1984–90 Northerners in all cohort groups have become more liberal in their attitudes toward busing, while Southerners have remained approximately the same. Thus in these four cases there is evidence of some change in the racial attitudes of various regional and educational subgroups, but almost no evidence that liberalism decreased, especially among the cohorts of the 1980s.

Only the negative interaction of period and cohort for SPENDING ON BLACKS suggests that the youngest cohorts have tended to become significantly less liberal over the years from 1984 to 1990 in the way that Mannheim's theory might predict. Yet this is only a single question. The absence of similar interactions in the 11 other questions implies that the socialization of young adults in the 1980s has generally *not* fostered illiberal racial attitudes and, furthermore, that the differences among cohorts have not tended to decrease from 1984 to 1990.[14]

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[14] One other question (FEDERAL JOB INTERVENTION) had a negative period and cohort interaction that would have been significant had we used the conventional cutoff ($P < .05$).

In addition, the interactions with region give little indication that the convergence of racial attitudes between the North and the South, characteristic of some issues studied over longer periods of time, has occurred during the 1980s (Schuman et al. 1988; Blauner 1989; Firebaugh and Davis 1989). Only the interactions of cohort with region for OPEN HOUSING and AID TO BLACKS suggest that recent cohorts in the South are approaching the liberalism of recent cohorts in the North. Moreover, the positive interaction of period and northern residence actually indicates a divergence between the regions in attitudes toward busing during the 1980s. However, with a maximum period of just seven years, the conclusion that there is little regional convergence during the 1980s on racial policy issues must be regarded as quite tentative. Similarly, we must regard findings about the interactions discussed here as interesting but hardly convincing until some or all of the findings replicate more consistently across questions and population groupings. The important points to remember are that (1) *only* one of these interactions implies anything negative about the cohorts coming of age in the 1980s and (2) there is no pervasive pattern of similar interactions across a large number of the 12 questions studied here.

Effects of the Demographic Variables

The unstandardized OLS regression coefficients for the demographic variables are listed in table 3. The main effects of education are positive and highly significant for nine of the 12 questions. Of the remaining three effects—two positive and one negative—none is significant. Thus these multivariate analyses have unexpectedly supported quite strongly the hypothesis that education promotes more liberal racial attitudes even on questions dealing with policy implementation.

Family income, on the other hand, has a consistently negative influence on the racial policy items considered here. Thus, with education controlled, the higher the income the less liberal white attitudes are. Of the 11 questions with negative coefficients for family income, 5 are significant beyond the .01 level and 2 others are significant beyond the .025 level.[15] Almost all of the racial policy items whose wording directly states or implies spending money (e.g., AID TO MINORITIES, SPENDING ON BLACKS, and BUSING) have highly significant negative regression coefficients for family income, suggesting that those who have the means to support social programs of benefit to many blacks do not feel it is in their self-interest to do so. Those items that refer to nondiscriminatory

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[15] In the comparable logistic regression one of these two coefficients (HELP BLACKS), though remaining negative, does not attain the appropriate significance level.

TABLE 3

EFFECTS OF DEMOGRAPHIC VARIABLES, CONTROLLING FOR COHORT AND PERIOD, 1984–1990, FOR COHORTS COMING OF AGE FROM 1960 TO 1990

Racial Policy Question	Education	North	Female	Income	R^2	N
Implementation:						
NO GOVERNMENT GUARANTEE[a]	.166***	.107	.064	−.043	.053	1733
OPEN HOUSING[a]	.025***	.217***	.060***	.007	.033	3368
AID TO MINORITIES[a]	.191***	.257***	.182**	−.143***	.042	1861
AID TO BLACKS[a]	.210***	.928***	.229*	−.129**	.051	900
HELP BLACKS[a]	.149***	.199***	.061	−.041*	.033	3360
FEDERAL JOB INTERVENTION	.093***	.104	.005	−.026	.021	1302
BUSING[a]	.039*	−.054	.023	−.028***	.041	2744
FEDERAL SPENDING	.013	.094***	.026	−.037***	.011	2632
SPENDING ON BLACKS[a]	.095***	.158***	.021	−.044**	.047	1833
Affirmative action:						
IN EDUCATION	.002	.234***	.231***	−.069*	.029	1238
IN JOBS[a]	−.022	.007	.130*	−.046	.019	1222
SHOULD OVERCOME PREJUDICE	.275***	.254***	.112	−.008	.102	1300

NOTE.—Variables: Education = 1 (less than high school), 2 (high school graduation), 3 (some college), 4 (college graduation), 5 (more than college); South = 0, North = 1; male = 0, female = 1; income = 1 (< \$10,000), 2 (\$10,000–\$19,999), 3 (\$20,000–\$34,999), 4 (\$35,000–\$49,999), 5 (\$50,000 or more).

[a] The analysis includes only cohorts coming of age from 1960 to 1985 for the NES and 1960 to 1987 for the GSS.

* $P < .025$.

** $P < .01$.

*** $P < .001$.

policies, such as OPEN HOUSING and NO GOVERNMENT GUARANTEE, have coefficients that are not significant. Thus it appears that the type of policy mentioned in a question influences the regression results for the income variable.

As expected, the main effect of region is strikingly positive with Northerners being more liberal than Southerners on eight out of 12 items, all of which are significant beyond the .001 level. The four other questions have coefficients that are not significant, although the one for FEDERAL JOB INTERVENTION comes close ($P = .026$).

The gender variable, in its turn, also has a large effect. On five questions, the women coming of age since 1960 have been significantly more liberal in their racial attitudes than the men. In addition, across the remaining seven items in table 3, the effect of being female is positive in all cases.

DISCUSSION

The most important of our results is the absence of empirical support for an inverted U-shaped pattern of cohort differences that would have indicated decreasing tolerance among cohorts coming of age in the 1980s. For the majority of questions there appears to be little difference in racial attitudes among the cohorts of the 1960s, 1970s, and 1980s. The five questions that do show a discernible pattern assume the positive, linear form that is consistent with our first hypothesis.

Period effects, however, lose much of their impact once we have taken cohort into account. Only three questions, as opposed to seven in the bivariate analysis, have main effects for period that are significant beyond the .025 level, and they are mixed in direction with two negative and one positive. On a fourth question (SPENDING ON BLACKS) negative changes occurred specifically among the most recent cohorts of young adults.[16]

Two other important findings concern the ways that education and income, net of cohort influences and of each other, affect attitudes toward issues of affirmative action and toward the implementation of policies promoting racial equality. Despite the fact that Schuman et al. (1988) were not able to find a consistent relationship between education and attitudes on racial policy issues, the main effects of education displayed in table 3 suggest that, in a multivariate context, the relationship may be predominantly positive. Income, on the other hand, almost uniformly has a significant, negative relationship to attitudes of this kind.

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[16] It is important to note that the period we cover begins in 1984 and therefore cannot include changes directly associated with Ronald Reagan's election and first several years in office.

The models developed here suggest that we still have some distance to go in understanding the course of racial attitudes over time. The R^2s for the full models given in table 3 are extremely low, usually explaining less than 5% of the variation in racial attitudes. In addition, we note that the affirmative action and implementation questions have slightly different relationships. The cohort effects for the three affirmative action questions are not significant. Furthermore, period effects for affirmative action items tend to be negative even though only one of the three is significant. In contrast, those items that deal with government action to enforce nondiscrimination are likely to show positive period effects.

It appears from these analyses that socializing experiences conducive to integration and racial equality, when they operate at all, act to create more liberal cohorts of young adults. Thus it appears that the counterforces of the Reagan era have not produced a post-civil-rights generation that is distinctively illiberal in its racial attitudes. Furthermore, there is almost no evidence in these data that any of the public events of the last 30 years has had the kind of specific generational impact that might be hypothesized.

The fact that the negative changes we uncovered seem to be a product of period forces that in most cases impinge on everyone equally can be seen as cause for either optimism or pessimism about the future course of race relations. From a positive perspective, the legacy of the Reagan era appears to have been slight—we discover almost no negative cohort effects and only two significant negative period effects that apply to all cohorts. Yet, from a pessimistic standpoint, the highly significant period effect in the direction of victim blame on the item labeled SHOULD OVERCOME PREJUDICE suggests that a concerted attempt to justify indifference to black advancement might have a good deal of appeal to many white Americans.

More generally, we must try to reconcile our findings with the frequent news reports of decreasing liberalism among young people. The reasons for the discrepancy may be either methodological or substantive. Methodologically, we must be concerned about not having samples that are representative of the population to which we wish to generalize. We have already discussed the undercoverage of college students, though it appears to be less than might have been expected. In addition, there may be more serious problems with undercoverage of young, urban, working-class, white males, who are hard to locate and even harder to talk into being interviewed. Nonresponse from other sources could also eliminate groups that are most susceptible to illiberal influences. Although we feel somewhat confident that our samples are at least broadly representative, we have not been able to rule out differential nonresponse rates among all subgroups of young adults, nor determine whether coverage of urban, working-class, white males has at least been consistent over the years from 1984 to 1990.[17]

Finally our models may be misspecified. If it is not appropriate to assume that aging has negligible effects on racial attitudes, our regression estimates may not be trustworthy. For example, if the relation of age to racial attitudes is negative, as has sometimes been hypothesized, ignoring it would inflate cohort effects and deflate period effects, leading toward the kinds of results we have presented here. Including more independent variables also can change the nature of relationships. We need better information from respondents about the socializing experiences that apply to them personally, so that these variables can be used to gauge cohort effects directly, rather than through the surrogate of birth year (Glenn 1976; Rodgers 1982, 1990).

The substantive reasons for the discrepancy between our findings and news reports are more important to emphasize. We have given the broad view. However, there may be countertrends among certain groups of young white adults that cannot be traced in national samples of 1,500–2,000 adults because the groups are too small to be well represented. Racism may be increasing within these subgroups, and their actions and pronouncements may be inflammatory enough to be newsworthy despite their small size. What we have established here is that there is no widespread, systematic decline in liberal racial attitudes among those people entering adulthood from 1960 to 1990. It may turn out that the Reagan retreat on civil rights simply gave people holding clearly racist views license to speak and act out their hostilities.

It is also extremely important not to project the linear trend over the past several decades into the future in any automatic way. Indeed, past turnabouts in other areas have shown that abrupt changes in direction are possible, based on new issues or possibly on new appeals by influential leaders or demogogic candidates for leadership.[18]

Another hypothesis suggests that what is new is not the harassment of blacks, which has always been carried out by isolated individuals and fringe groups, but rather blacks' responses to it. With a new-found militancy, blacks in the 1980s have called attention to incidents of harassment and signaled their intention not to put up with demeaning treatment or to limit their behavior to types that are nonthreatening to whites.

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[17] In the samples for the 1989 and 1990 GSS, there appeared to be a sizable drop and then a slight rebound in the percentages of white males, 18–24 years old, who live in large and medium-sized SMSAs. From 1984 to 1990 the combined percentages for these two urban areas in the GSS samples are: 24.8, 26.5, 24.5, 28.1, 21.2, 14.7, and 19.6. There is also a considerable drop from 1988 to 1990 in the percentage of white males, 18–24 years old, who live in large and medium-size SMSAs and work either full- or part-time.

[18] See the sharp drop after 1970 on an item about federal intervention to desegregate schools, probably due to a sudden concern over court-ordered busing in the North, as graphed in Schuman et al. (1988, p. 94).

Finally, it is essential to keep in mind that what we are studying here is change over time. Our conclusions are reached in the face of quite high levels of support among young adults for the anti-integration or anti–affirmative action alternatives on several questions. For example, only one-third of our sample is willing to say they support federal action to see to it that black people get "fair treatment in jobs;" and more than one-third defend the right of a homeowner "to refuse to sell to someone because of their race or color." In fact we may not really have a contradiction at all to explain, given the limited nature of our present conclusions about cohort and period effects: that opposition to most racial policy issues is not a product of the political milieu of the mid- to late 1980s, that it cannot be attributed to the attitudes of adults who came of age during the years of the Reagan presidency, and that it is not in any major way becoming more widespread. The existence of several positive cohort differences suggests instead that—*other things being equal*—opposition to some of these issues, especially those dealing directly with nondiscrimination, should tend to lessen over time as new cohorts, more liberal than the average cohort of the past, continue to enter adulthood. Whether "other things" will in fact be equal in the future is, of course, a matter beyond the ken of our analysis of even the very recent past.

APPENDIX

Wording of GSS and NES Racial Policy Questions, 1984–90[19]

Implementation

NO GOVERNMENT GUARANTEE (NES) Equal opportunity for blacks and whites to succeed is important but it's not really the government's job to guarantee it. Do you 1. agree strongly 2. agree somewhat 3. neither agree nor disagree 4. disagree somewhat 5. disagree strongly? *Variant:* In 1988 "to succeed" was dropped from the question and "very" was added before "important."

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[19] Here, the response categories for each question are listed as they appeared in the original questionnaire. In some cases the categories had to be reversed for the regression analyses so that the responses would always be ordered from least liberal (fewest number of points) to most liberal (greatest number of points).

OPEN HOUSING (GSS) Suppose there is a community-wide vote on the general housing issue. There are two possible laws to vote on. One law says that a homeowner can decide for himself who to sell his house to, even if he prefers not to sell to blacks. The second law says that a homeowner cannot refuse to sell to someone because of their race or color. Which law would you vote for? Responses: 1. Homeowner can decide 2. No discrimination.

AID TO MINORITIES (NES) Some people feel that the government in Washington should make every effort to improve the social and economic position of blacks and other minority groups. Others feel that the government should not make any special effort to help minorities because they should help themselves. Where would you place yourself on this scale or haven't you thought much about this?[20]
Variant: In 1988 "minority groups" was replaced by "minorities."

AID TO BLACKS (NES) Some people feel that the government in Washington should make every effort to improve the social and economic position of blacks. Others feel that the government should not make any special effort to help blacks because they should help themselves. Where would you place yourself on this scale, or haven't you thought much about this?[21]

HELP BLACKS (GSS) Some people think that blacks have been discriminated against for so long that the government has a special obligation to help improve their living standards. Others believe that the government should not be giving special treatment to blacks. Where would you place yourself on this scale, or haven't you made up your mind on this?[22]

FEDERAL JOB INTERVENTION (NES) Some people feel that if black people are not getting fair treatment in jobs, the government in Washington ought to see to it that they do. Others feel that this is not the federal government's business. Have you had enough interest in this question to favor one side over the other? (IF YES) How do you feel? Should the government in Washington see to it that black people get fair treatment in jobs or is this not the Federal Government's business?[23]

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[20] The response scale ranges from "1" for "The government should help minorities" to "7" for "Minorities should help themselves" ("Haven't thought much about this" = 4).

[21] The response scale ranges from "1" for "The government should help blacks" to "7" for "Blacks should help themselves" ("Haven't thought much about this" = 4).

[22] The response scale ranges from "1" for "I strongly agree the government is obligated to help blacks" to "5" for "I strongly agree that government shouldn't give special treatment" ("I agree with both answers" = 3).

BUSING (GSS) In general, do you favor or oppose the busing of black and white school children from one school district to another? Responses: 1. Oppose 2. Favor.

FEDERAL SPENDING (NES)[24] Here are some more questions about federal spending. If you had a say in making up the federal budget this year, for which of the following programs would you like to see spending increased and for which would you like to see spending decreased. . . . Should federal spending on programs that assist blacks be increased, decreased or kept about the same? Responses: 1. Decreased 2. Same 3. Increased.
Variant: In 1984 the introduction to this question was worded, "Which programs would YOU like to see increased and which reduced?" In addition, the wording, "assistance to blacks," was used rather than "programs that assist blacks."

SPENDING ON BLACKS (GSS) We are faced with many problems in this country, none of which can be solved easily or inexpensively. I'm going to name some of these problems, and for each one I'd like you to tell me whether you think we're spending too much money on it, too little money, or about the right amount. . . . Improving the conditions of blacks.[25] Are we spending too much, too little, or about the right amount on improving the conditions of blacks? Responses: 1. Too much 2. About the right amount 3. Too little.

Affirmative Action

IN EDUCATION (NES) Some people say that because of past discrimination it is sometimes necessary for colleges and universities to reserve openings for black students. Others oppose quotas because they say quotas give blacks advantages they haven't earned. What about your opinion—are you for or against quotas to admit black students? Responses: 1. Against—strongly 2. Against—not strongly 3. For—not strongly 4. For—strongly.

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[23] The responses are "1" for "The government should see to it" and "5" for "It's not the federal government's business." In our analyses, the responses have been scored on a 3-point scale with "depends" and "no interest" at the midpoint.

[24] In 1984, programs for blacks was last in a list of 10 programs that included such things as improving and protecting the environment, dealing with crime, social security, and food stamps. In 1986, it was last in a list of eight such programs. In 1988, programs that assist blacks moved up to fifth place in a list of 10 programs, coming after improving and protecting the environment, financial aid for college students, government assistance for the unemployed, and space and scientific research.

[25] Improving the conditions of blacks is eighth in the list of problems, following such concerns as improving and protecting the environment, halting the rising crime rate, and improving the nation's education system.

IN JOBS (NES) Some people say that because of past discrimination, blacks should be given preference in hiring and promotion. Others say that such preference in hiring and promotion of blacks is wrong because it gives blacks advantages they haven't earned. What about your opinion—are you for or against preferential hiring and promotion of blacks? Responses: 1. Against—strongly 2. Against—not strongly 3. For—not strongly 4. For—strongly.

SHOULD OVERCOME PREJUDICE (NES) Irish, Italians, Jewish and many other minorities overcame prejudice and worked their way up. Blacks should do the same without any special favors. Responses: 1. Agree strongly 2. Agree somewhat 3. Neither agree nor disagree 4. Disagree somewhat 5. Disagree strongly.

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Characteristics of Successful American Protest Groups: Another Look at Gamson's *Strategy of Social Protest*[1]

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William Gamson's *Strategy of Social Protest* is considered to be a major contribution to the social-movement literature, but it has been criticized on methodological and theoretical grounds. The seriousness of the existing criticisms makes it useful to reassess Gamson's *Strategy*. This article first reviews the critical literature on *Strategy* and discusses Gamson's response to his critics. Second, the article reports the results of several analyses of the data set obtained by using the best statistical procedures available for assessing the relative importance of organizational and environmental variables and analyses of the major concerns raised by Gamson's critics and his response to them. The results suggest that displacement and group factionalism are the major predictors of protest group success. Implications of the analysis are discussed.

William Gamson's (1975, 1990) *Strategy of Social Protest* represents one of the most recent comprehensive efforts to identify the characteristics that distinguish successful protest groups from their unsuccessful counterparts. Defining a protest group as an excluded group "seeking the mobilization of an unmobilized constituency" (1975, p. 16) against an "antagonist [lying] outside of its constituency" (1975, p. 17), Gamson examined the careers of a representative sample of 53 U.S. protest groups active

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[1] We would like to thank James Miley and several anonymous *AJS* reviewers for their comments on earlier versions of this paper. Correspondence should be addressed to R. Scott Frey, Department of Sociology, Anthropology, and Social Work, Kansas State University, Manhattan, Kansas 66506.

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sometime during the period 1800–1945. He found that group goals, organization, tactics, and the social context in which the protest group operated, as well as the nature of the group's relationship with other groups, both antagonists and friends, were important determinants of a group's acceptance by and ability to gain new advantages from authorities.

Strategy is considered by many to be a major methodological and theoretical contribution to the social-movement literature (Jenkins 1983; McAdam, McCarthy, and Zald 1988; Morris and Herring 1987; Olzak 1989; Tarrow 1988), but others have challenged it on methodological and theoretical grounds (Goldstone 1980*a*, 1980*b;* Gurr 1980; Webb et al. 1983; Zelditch 1978). Despite the controversy that has arisen around it, *Strategy* remains a central work in the social-movement literature because it speaks to current resource mobilization issues.[2] In particular, *Strategy* contains a data set that can be analyzed to shed new light on the question of the relative importance of organizational and environmental variables in shaping protest-group success. While several case studies of various social movements have addressed this and related resource-mobilization issues,[3] only Gamson's data for a representative sample of protest groups are suited to a full assessment of the problem.

There have been several attempts to reanalyze the data presented in *Strategy*. Goldstone's (1980*a,* 1980*b*) reanalysis focuses on determining the relative impact of organizational and environmental variables on protest-group success, but his modifications of Gamson's data set are open to serious questions. Also, he does not examine all the organizational variables that Gamson and others consider relevant to group success, so his results are not an exhaustive test of alternative theoretical positions. Reanalyses by Mirowsky and Ross (1981) and Steedly and Foley (1979) are multivariate assessments of the full range of organizational and environmental variables used by Gamson, but they fail to deal with major shortcomings in Gamson's data set.

We make two contributions to the social-movement literature by assessing Gamson's *Strategy*. First, we review the critical literature on *Strategy* and discuss Gamson's response to his critics in order to develop a balanced assessment of *Strategy* and its place in the broader social-movement literature. Second, we report the results of a reanalysis of the data set using the best procedures available for assessing the relative importance of organizational and environmental variables, as well as concerns raised by Gamson's critics and his response to them. Our analysis not only advances the understanding of the controversy surrounding *Strategy*, but it represents a fuller assessment of the data set than has heretofore been available.

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[2] For an expanded discussion of the resource mobilization approach, see the reviews of Cohen (1985), Jenkins (1981*a*, 1983), Klandermans (1986), McAdam et al. (1988), Morris and Herring (1987), Rule (1988, pp. 170–99; 1989), and Tarrow (1988).

[3] See, for instance, the recent work of Amenta and Zylan (1991) and Staggenborg (1989*a*, 1989*b*), as well as the work of Barkan (1979), Downey (1986), Jenkins (1981*b*, 1985), Klandermans (1984), McAdam (1982), Morris (1981, 1984), Useem and Zald (1982), Walsh (1981, 1988), and Walsh and Warland (1983).

A CRITICAL ASSESSMENT OF *THE STRATEGY OF SOCIAL PROTEST*

Objections and Refutations

Multivariate analyses of Gamson's data suggest that findings reported in *Strategy* are robust (Mirowsky and Ross 1981; Steedly and Foley 1979). Moreover, a selected review of findings of historical and cross-cultural research indicates support for many of Gamson's conclusions.[4] Despite such support, Gamson's analysis has been subjected to three methodological criticisms: (1) failure to use multivariate techniques;[5] (2) improper coding of various group characteristics;[6] and (3) counting three pairs of interrelated groups as independent.[7]

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[4] Support exists for many of Gamson's conclusions on the effects of such variables as external sponsorship (e.g., Bailey 1974; Jenkins 1981*b*, 1985; Jenkins and Perrow 1977; Lipsky 1970; Schumaker 1975; Shorter and Tilly 1974; but see Kowalewski and Schumaker [1981] for qualifying statements); use of selective incentives (e.g., Jenkins 1981*b*, 1985; Olson 1965); pursuit of displacement goals (e.g., Schumaker 1975; Snyder and Kelly 1976); use of violence and/or nonviolent constraints (e.g., Goldfield 1989; Kowalewski and Schumaker 1981; Piven and Cloward 1977; Shorter and Tilly 1974; Tilly, Tilly, and Tilly 1975; but see Schumaker [1975, 1978] and Synder and Kelly [1976, pp. 157–58] for qualifying statements); and subject to violence and/or nonviolent constraints (e.g., Snyder and Kelly 1976; Shorter and Tilly 1974).

[5] Goldstone (1980*a*, p. 1019), Gurr (1980, p. 243), and Zelditch (1978, p. 1518) have all argued that Gamson's failure to use multivariate methods may have resulted in the identification of spurious, bivariate relationships.

[6] Improper coding took three different forms. First, Goldstone (1980*a*, p. 1021) argues that Gamson's decision to code the First International as a nondisplacement group was mistaken and that it should have been coded as a displacement group because its goal was the displacement of the capitalist class. Second, Goldstone (1980*a*, pp. 1020–21) maintains that Gamson's decision to code seven groups identified as receiving equivocal or partial new advantages as unsuccessful is questionable and that they should have been coded as securing new advantages. (These groups are the American Association of University Professors, Brotherhood of the Kingdom, International Seamen's Union of America, National Union for Social Justice, Native American Party, Dairymen's League, and the North Carolina Manumission Society.) Third, Goldstone (1980*a*, pp. 1022–25) indicates that five groups identified by Gamson as not achieving new advantages were eventually successful and should have been coded as gaining new advantages. (These groups are the National Brotherhood of Baseball Players, American Birth Control League, National Student League, Union Trade Society of Journeymen Tailors, and the American Anti-Slavery Society.)

[7] Goldstone (1980*a*, p. 1021) and Zelditch (1978, p. 1517) have criticized Gamson

Goldstone's (1980*a*) reanalysis of Gamson's data examines the effects of various parameters on group ability to secure new advantages by first correcting for the above three problems, and then expanding the analysis to include an additional dependent variable: timing of success for groups attaining new advantages. Only group success in attaining new advantages is included in the reanalysis, for Goldstone (1980*a*, p. 1019) argues that his results are "generally" applicable to group acceptance.

With respect to the issue of Gamson's failure to use multivariate techniques, Goldstone suggests that the displacement goal is "sufficient to invalidate all the zero-order correlations in which the impact of displacement goals was not factored out" (1980*a*, p. 1019, table 1). In fact, he reports that the bureaucracy parameter, identified by Gamson as having a moderately strong and positive effect on group success, had a slight negative effect on success when displacement was controlled in a reanalysis of the original data set (Goldstone 1980*a*, table 2). Goldstone concludes from his analysis that a number of Gamson's findings are confounded by the displacement goal and argues that this pattern also exists in Steedly and Foley's (1979, tables 5 and 6) multivariate analysis of Gamson's data set (Goldstone 1980*a*, p. 1028, n. 8; 1980*b*, pp. 1428–29).

Goldstone argues that Gamson's findings on group attainment of new advantages are characterized by other spurious relationships. However, these relationships are not a function of Gamson's failure to systematically control for displacement, but are a result of improper coding of group characteristics and the inclusion of several groups in the sample that are not independent of each other. Goldstone documents this point by demonstrating that several of Gamson's findings are refuted when data are corrected and displacement is included as a control variable. That is, Goldstone shows in his reanalysis that factionalism had no impact on group success (1980*a*, table 3) and that all nondisplacement groups were successful (1980*a*, table 4). These findings are interpreted by Goldstone as suggesting that organizational and tactical characteristics have little, if any, effect on a group's ability to gain new advantages. In effect, Goldstone concludes that Gamson's findings on success rate are simply an artifact of faulty methodological procedures.

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for including several groups in his analysis that are not independent of each other. Specifically, Goldstone (1980*a*, pp. 1021, 1023) identifies three pairs of groups that were pursuing the same goals and using similar tactics during an equivalent time period. (These include the American Anti-Slavery Society/National Female Anti-Slavery Society, National Student League/American Student Union, and the Young Peoples Socialist League no. 1/Young Peoples Socialist League no. 2.) Goldstone indicates that the inclusion of all six groups is misleading because "[if] one group succeeded, the paired group also succeeded [in gaining new advantages], by definition" (1980*a*, p. 1021). As a result, Goldstone argues that three of the six groups should not have been included in Gamson's analysis.

Goldstone (1980*a*, pp. 1029–37) expands Gamson's analysis by examining effects of organizational and tactical parameters on timing of group success. He argues that timing of success is not affected by organizational and tactical parameters, taken individually or in combination, but that it is a random process (Goldstone 1980*a*, figs. 2 and 3, tables 5–7). However, he finds that bureaucracy and selective incentives have negative impacts on timing of success. Moreover, he finds that groups were more likely to succeed during periods of national crisis. Goldstone concludes that timing of success is a positive function of a group's ability to sustain a challenge until crises emerge.

Goldstone (1980*a*, pp. 1037–42; 1980*b*, pp. 1431–32) presents a theoretical interpretation of the results of his reanalysis that contradicts arguments of Gamson and other resource mobilization theorists. He reasons that systemwide crises such as war and depression foster group success, for such crises may facilitate protest-group mobilization and weaken the capacity of authorities to neutralize protest groups. Protest groups are not, as Gamson argues, autonomous and calculating agents in control of their destinies; they are, instead, subject to social forces beyond their control. The implication is clear: if we want to explain why some groups succeed, we need not examine what groups do, but when and where they do it.

Gamson's Response to His Critics

Gamson (1990, pp. 145–80) indicates in his new edition of *Strategy* and in other works (Gamson 1988, 1989; Gamson and Modigliani 1989) that changes in U.S. society (especially the rise of the national security state and the increasing importance of television and the mass media in general) since 1945 have reduced the usefulness of the results and underlying theory of *Strategy* for understanding protest groups operating in the post-1945 period. Nonetheless, with the exception of protest-group use of selective incentives, Gamson (1990; Fireman and Gamson 1979) defends the results reported in the first edition of *Strategy*. He also does not deviate from his earlier dismissal of Goldstone's (1980*a*) criticisms (Gamson 1980).

Gamson rejects Goldstone's conclusion that the findings of *Strategy* are spurious. Gamson (1980, pp. 1051–53) notes that Goldstone presents evidence for the existence of a spurious relationship only for the case of bureaucracy and new advantages because other examples are based on a recoding of data that is highly suspect.[8] In fact, even the case of bureaucracy and new advantages is suspect, because Gamson notes that he never argued that bureaucracy had a strong and positive effect on new advantages; on the contrary, he argued that bureaucracy had a strong and positive effect on *acceptance*. Furthermore, Gamson (1980, p. 1054) indicates that Goldstone's analysis of timing of success is of questionable validity because the timing variable is based on a misunderstanding of coding procedures. In effect, Gamson concludes that the results of Goldstone's reanalysis are questionable because they are based on procedures that reduce data quality by blurring important distinctions between groups.

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[8] Methodological criticisms are dismissed by Gamson. First, he (Gamson 1980, p. 1059) disputes Goldstone's contention that the First International was a displacement

Gamson (1980, p. 1056) maintains that Goldstone's theoretical interpretation of group success, which emphasizes the role of crisis, has limited explanatory power (also see Gamson and Schmeidler 1984). That is, Goldstone's notion of crisis is so loosely defined and applied that it proves impossible to falsify. Furthermore, Gamson (1980, p. 1058) chides Goldstone for relying on an explanation that emphasizes forces beyond human control.

DATA AND METHOD

Procedure

As noted above, a major criticism of Gamson's analysis centers on his failure to use multivariate techniques that take into account several hypothesized determinants of group success simultaneously. In response, we used multivariate techniques to analyze the direct effects of 12 variables on group success in gaining new advantages for each of three samples. Samples were created from Gamson's (1975, pp. 187–89; 1990, pp. 319–21) published data. *Sample 1* ($N = 53$) consists of Gamson's original data set. *Sample 2* ($N = 50$) consists of the following changes in the data set suggested by Goldstone (1980*a*): (1) the First International was recorded as a displacement group; (2) 12 cases identified by Gamson (1975, pp. 187–89) as receiving either equivocal or partial new advantages or identified by Goldstone (1980*a*, pp. 1022–25) as being eventually

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group. Second, Gamson (1980, pp. 1044–47) maintains that Goldstone's suggestion that the 12 groups that attained either partial or equivocal success or were eventually successful should be coded as successful blurs important distinctions between these groups and those identified as successful. Third, Gamson (1980, pp. 1047–49) argues that Goldstone's claim that three pairs of groups were not independent is unjustified because groups in each pair were using distinct tactics, but admits that the inclusion of both groups of one pair (American Anti-Slavery Society and National Female Anti-Slavery Society) may represent an actual case of double counting. In sum, Gamson argues that Goldstone's comments regarding coding and double counting represent poor methodological advice.

successful in attaining new advantages, were recoded as receiving new advantages;[9] and (3) three cases of double counting were deleted.[10] *Sample 3* ($N = 49$) consists of what we consider to be justified changes in the data set: (1) following Goldstone's (1980*a*, p. 1021) argument, we recoded the First International as a displacement group; (2) three groups coded by Gamson (1975, pp. 187–89) as receiving equivocal new advantages were deleted from the sample because neither Gamson's identification of these groups as unsuccessful nor Goldstone's (1980*a*) identification of them as successful seems particularly justified;[11] (3) on the basis of Goldstone's (1980*a*, pp. 1020–21) argument, we recoded four groups coded by Gamson (1975, pp. 187–89) as receiving partial advantages as receiving new advantages;[12] and (4) the National Female Anti-Slavery Society was deleted because Gamson (1980, p. 1049, n. 8), in response to Goldstone's (1980*a*, pp. 1021, 1023) criticism, admits that it may represent a case of double counting.

Use of the three samples in a reanalysis allowed us to examine the validity of Gamson's (1975, 1990) conclusions in light of the existing criticisms. Analysis of sample 1 allowed us to assess the validity of the charge made by Zelditch (1978), Goldstone (1980*a*), and Gurr (1980) that Gamson's findings are spurious because he failed to use multivariate techniques in his analysis. Analysis of sample 2 allowed us (1) to examine the validity of Goldstone's (1980*a*, 1980*b*) charge that Gamson's findings are spurious because he failed to use multivariate techniques, miscoded characteristics of several groups, and counted three pairs of groups as independent when they were actually dependent and (2) to gauge the soundness of Goldstone's (1980*a*) reanalysis of group success in attaining new advantages. Analysis of sample 3 allowed us to assess the validity of both Gamson's conclusions and the strictures of his critics, as outlined above, with a sample consisting of what seem to be justified changes in the original data set.

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[9] These are the American Association of University Professors, the National Brotherhood of Baseball Players, Brotherhood of the Kingdom, International Seamen's Union of America, National Union for Social Justice, American Birth Control League, Native American Party, National Student League, Dairymen's League, Union Trade Society of Journeymen Tailors, American Anti-Slavery Society, and the North Carolina Manumission Society.

[10] These groups are the National Female Anti-Slavery Society, American Student Union, and the Young Peoples Socialist League no. 2.

[11] These groups are the American Association of University Professors, Brotherhood of the Kingdom, and the North Carolina Manumission Society.

[12] These include the International Seamen's Union of America, National Union for Social Justice, Native American Party, and the Dairymen's League.

Dependent Variable

Unlike Goldstone (1980*a*) and others (Mirowsky and Ross 1981; Steedly and Foley 1979), we used one measure of protest-group success in our reanalysis: new advantages secured from authorities by a group. New advantages was coded one if a group was identified as achieving new advantages and zero otherwise (see Gamson 1975, pp. 176–77, 187–89) according to the procedure outlined above for each of the samples. Timing of success was not included in our reanalysis because Gamson (1980, p. 1054), as noted earlier, has argued convincingly that Goldstone's (1980*a*) use of this variable was based on a misunderstanding of coding procedures. Furthermore, Goldstone does not provide sufficient information for constructing the timing variable.

Independent Variables

The 12 variables that Gamson found to be important determinants of group success in *Strategy* were used in the analysis. These are (1) whether or not the group was active during a crisis; (2) whether or not the group attempted to displace authorities; (3) whether or not the group had centralized authority; (4) whether or not the group was bureaucratically organized; (5) whether or not the group made use of selective incentives; (6) whether or not the group focused on a single issue; (7) whether or not the group was subject to factional splits; (8) whether or not the group had external support; (9) whether or not the group used violence; (10) whether or not the group was subject to violent attacks; (11) whether or not the group used nonviolent constraints; and (12) whether or not the group was subject to nonviolent constraints.

Analytical Method

There is no ideal statistical procedure for a problem with a dichotomous dependent variable, 53 or fewer cases, and 12 independent variables. Previous attempts to deal with this problem by using either factor analysis or cluster analysis (Mirowsky and Ross 1981; Steedly and Foley 1979) did not solve the problem of a limited number of degrees of freedom, since the factor structure is estimated using the small sample prior to the use of factor scores in regression analyses. If the estimation of the measurement (factor) and structural (regression) models were done simultaneously, as is the case with LISREL-style models, the large number of parameters to be estimated relative to the sample size available would be apparent.[13] Conducting factor analysis and regression independently does not eliminate the problem but simply masks it.

We chose an ordinary least squares (OLS) linear probability model to estimate the direct effects of the 12 independent variables on group success in achieving new advantages.[14] The logical alternative to the linear model is a logit or probit model estimated by maximum-likelihood methods. These nonlinear approaches fit a more appealing probability function to the data than the straight line of the linear model and, in large samples, they have desirable statistical properties. But the resulting coefficients are more difficult to interpret because of their nonlinear relationship to the probability of falling into a specified category of the dependent variable, and in small samples such as the ones used here the sampling properties are not known. In contrast, the linear probability model estimated by OLS regression performs reasonably well in small samples (Gillespie 1977; Knoke 1975).

We used two approaches in estimating standard errors for coefficients in the linear probability model. The simplest is to use the normal theory standard errors from an OLS estimate. Since the linear model violates the assumption of homoscedasticity and normality of residuals, the resulting standard errors are likely to be too optimistic. As an alternative, we have bootstrapped the linear model to estimate bootstrapped standard errors using 500 bootstrap replicates (Dietz, Frey, and Kalof 1987; Dietz, Kalof, and Frey 1991; Freedman and Peters 1984; Stine 1989–90). Note that while bootstrapping is a very general procedure, strictly speaking we are violating a rule of thumb that in bootstrapping regression models, the number of observations should be larger than the square of the number of independent variables. Thus we cannot have complete confidence in the bootstrapped standard errors either. But given the theoretical specification of the problem, and the data available, we believe these two techniques applied to the three samples described above yield the best understanding of the evidence that can be obtained.

Multicollinearity is often a serious problem when the number of parameters to be estimated is large compared with the size of the sample. High levels of collinearity degrade regression estimates by making coefficient estimates unstable and by increasing the magnitude of the associated standard errors. These problems make it difficult to be confident about population-parameter values. There are some collinearity problems in our data. To assess their impact, we used the procedures suggested by Belsley, Kuh, and Welsch (1980). In particular, we consider collinearity problematic when the condition index (calculated from the singular value decomposition of the matrix of independent variables) is greater than 5.0 and when there are two or more variance proportion indices associated with the corresponding eigenvalues that are greater than 0.5. Note that the first of these criteria is more stringent than Belsley et al.'s (1980) suggested "cut point" of 10 for the condition index. We discuss these results for each sample in turn, and we also report in the tables the tolerance values ($1 - R^2$ for the auxiliary regression predicting an independent variable as a function of the other independent variables) for each independent variable in each sample. While some collinearity is present, an examination of the diagnostics suggests that the problem is not severe enough to preclude drawing reasonable conclusions from the analysis.

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[13] Indeed, simulation work suggests that LISREL models do not produce reliable estimates with sample sizes under 50 (Boomsma 1982), while Bollen (1989, p. 268) suggests several cases for each free parameter in the model.

[14] Parameter estimates of a reduced model consisting of seven of the 12 independent variables were similar to estimates reported below for the full model. The seven variables included bureaucratization, centralized authority, use of selective incentives, use of violence, recipient of violence, displacement goals, and factionalism. Estimates of the reduced model are available from the authors.

RESULTS

Sample 1

Estimates for each of the three samples are reported in table 1. Results for sample 1 are based on Gamson's original sample. The collinearity diagnostics suggest that the estimated effects of using violence and nonviolent constraints may be confounded. (The condition index is 6.6 and the variance proportions are 0.46 and 0.50, respectively.) The estimated effects of "displacement goals" and "focus on a single issue" may also be influenced by collinearity. (The condition index is 14.6 and the variance proportions are 0.53 and 0.76, respectively.) Despite these minor problems, the regression coefficients all have the expected signs. The strongest effect is the decreased probability of success for groups that attempt to displace their opponents. The normal theory based *t*-value estimate suggests that the effect is significant at the 0.05 level, and the bootstrapped estimate is slightly above the 0.1 critical value. "Factionalism" exhibits the next strongest effect, with a coefficient that is significant at the 0.10 level for the normal theory based *t*-value estimate and at the 0.20 level for the bootstrapped estimate. The variables "subject to violence" and "active during a crisis period" have roughly equal effects, but only the latter has a statistically significant effect. Overall, the results are generally consistent with findings reported in *Strategy,* but the effects of most variables are small when compared with the estimated sampling error.

TABLE 1

ESTIMATES OF MODEL OF PROTEST GROUP SUCCESS FOR THREE SAMPLES

VARIABLE	SAMPLE 1 (*N* = 53)		SAMPLE 2 (*N* = 50)		SAMPLE 3 (*N* = 49)	
	Unstandardized Coefficient	Tolerance	Unstandardized Coefficient	Tolerance	Unstandardized Coefficient	Tolerance
Active during crisis	.2325 (1.84)* [1.75]**	.70	−.0568 (.68) [.81]	.64	.1209 (.96) [.93]	.68
Displacement goals	−.4111 (2.06)*** [1.78]**	.33	−1.0192 (7.89)*** [7.12]***	.30	−.3609 (1.96)** [1.48]*	.34
Centralized	.1213 (.96) [.83]	.70	−.0008 (.01) [.01]	.73	.0534 (0.42) [0.42]	.67
Formal bureaucracy	−.0125 (.08) [.07]	.48	.0861 (.94) [1.03]	.53	.0893 (.60) [.57]	.50
Selective incentives	.0633 (.33) [.28]	.53	−.1166 (.96) [.83]	.50	.2095 (1.09) [1.12]	.48
Focus on a single issue	−.0592 (.30) [.34]	.50	−.4849 (3.39)*** [2.00]**	.44	−.0204 (.13) [.10]	.52

Factionalism	−.2735	.60	−.1224	.64	−.3370	.59
	(1.99)**		(1.45)*		(2.48)***	
	[1.61]*		[1.40]*		[2.08]***	
External support	.0963	.78	−.1731	.76	−.0356	.79
	(.71)		(2.06)***		(.27)	
	[.73]		[1.79]**		[.23]	
Used violence	.1019	.41	−.1136	.37	.3040	.40
	(.46)		(.80)		(1.44)**	
	[.42]		[.47]		[.92]	
Subject to violence	−.2560	.76	−.0500	.71	−.1938	.72
	(1.34)*		(.38)		(.97)	
	[1.21]		[.37]		[.84]	
Subject to nonviolent constraints	.0015	.43	.1859	.36	−.1656	.40
	(.01)		(1.66)*		(1.00)	
	[.01]		[1.44]*		[.88]	
Used nonviolent constraints	.1308	.48	.0166	.50	−.0298	.50
	(.84)		(.18)		(.20)	
	[.72]		[.11]		[.14]	
Constant	.4416		1.5558		.7940	
	(1.76)**		(8.90)***		(3.39)***	
	[1.65]*		[5.87]***		[2.48]***	
R^2	.554		.778		.595	
Adjusted R^2	.420		.706		.460	

NOTE.—Normal theory t-values in parentheses; bootstrapped t-values in brackets, based on 500 bootstrap replications.
* $t > 1.30$, $P(t) < .20$.
** $t > 1.68$, $P(t) < .10$.
*** $t > 2.02$, $P(t) < .05$.

Sample 2

Sample 2 is based on Goldstone's recommended changes in the data set. Several features of the results are worth noting. First, the intercept term is greater than 1.0, which would produce an illegitimate probability of success for a group that scored zero on all independent variables and would yield high results for most cases. Second, collinearity may again be degrading the estimates for "displacement goals" and for "focus on a single issue." (The condition index is 16.7 and the variance proportions are 0.56 and 0.83, respectively.) Third, the sample produces estimates in which a single variable, displacement goals, dominates the model. The coefficient of -1.02 suggests that groups with displacement goals are almost certain to fail, whatever their other characteristics. The coefficient is significant at the 0.01 level for both t-value estimates. The next most important variable is focus on a single issue, for which the normal theory t is significant at the 0.05 level and the bootstrapped t at the 0.1 level. External support has a negative effect that is also significant at the 0.05 level for the normal theory based t-value estimate and at the 0.1 level for the bootstrapped estimate. This is a rather unexpected finding, since one would expect that such support would facilitate rather than retard a group's efforts.[15] Being subject to nonviolent constraints has a coefficient of roughly the same size, but again the positive sign is surprising since one would expect such constraints to affect success adversely. Factionalism and use of violence have negative effects of about equal magnitude, but they are not significant at conventional levels. Thus, the results based on the sample suggested by Goldstone (1980*a*) are consistent with his central theoretical argument in that displacement goals is the most important predictor of group success. The effect of single-issue focus is puzzling, but the effect is much weaker than that of displacement goals.

Sample 3

Sample 3 includes many, but not all of Goldstone's recommended modifications of Gamson's data. Collinearity seems to influence the effects of displacement goals and single-issue focus. (The condition index is 13.9 and the variance proportions are 0.50 and 0.71, respectively.) The displacement variable has the strongest effect, but it has only about one-third the size of the effect as that reported for sample 2. The normal theory based t-value is significant at the 0.10 level, but the bootstrapped t is significant at the 0.2 level. Factionalism has nearly as strong a negative effect, and it is significant at the 0.05 level for both *t*-value estimates. The use of violence also has about the same effect as displacement and factionalism on the probability of group success, but once again the effect is not significant by conventional standards, perhaps as a result of collinearity. The use of selective incentives, subjected to violence, and subjected to nonviolent constraints have nontrivial effects, though they are not significant.

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[15] Collinearity diagnostics do not indicate any serious problems influencing estimates for this coefficient.

Coda

In comparing results for the three samples, we find clear evidence that it is hard for groups with displacement goals to succeed in achieving new advantages. This result is consistent with previous analyses of Gamson's data. We also find relatively consistent evidence for the adverse effects of factionalism—the effect is strong and significant in samples 1 and 3 and of moderate strength though not significant in sample 2. The importance of factionalism has not been much discussed in the literature at this point. Formal bureaucracy has little effect on success in any sample. The results for other variables are equivocal, with no strong effects consistent for all three samples. There is mixed evidence for the effect of crisis period and being subject to violence with samples 1 and 3 showing moderate effects, while trivial effects are reported for these variables with sample 2. In samples 1 and 3 use of violence has moderate positive effects on success, but the effect of this variable is moderate and negative in sample 2. In sample 2, external support has a moderately strong negative effect on success, while the effects of such support are essentially zero in the other two samples.

The most important differences in results are between samples 2 and 3. Sample 1 produces results similar to sample 3, which incorporates all modifications to Gamson's original data on which there is consensus. The results for sample 2 suggest that only displacement goals has an overwhelming effect on success. Sample 3 suggests a strong, but not dominant effect for displacement goals and nearly as strong an effect for factionalism. Which sample should be given the greatest weight in drawing conclusions?

Statistical diagnostics provide limited guidance for choosing between the samples.[16] Ultimately, readers must judge the arguments behind each decision related to coding and double counting. For Goldstone's modifications to Gamson's data set there are 10 cases for which we do not think a consensus exists. These 10 cases constitute the difference between sample 3 and sample 2. We have dropped three cases that Gamson treated as failures but that Goldstone recoded as successful and in which the evidence seems equivocal.[17] All three had nondisplacement goals. We have retained Gamson's coding of nonsuccess for five groups that Goldstone argued were successful.[18] Four of the five groups had nondisplacement goals. Finally, we retained two groups that failed to gain new advantages that Goldstone dropped.[19] Every coding change proposed by Goldstone that remains in conflict with Gamson's arguments is one that strengthens the negative correlation between displacement goals and success.[20]

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[16] The model has a higher R^2 and adjusted R^2 when estimated with data for sample 2 than for sample 3. This might suggest that sample 2 is in some sense "better." But R^2 and the associated F-tests are intended for comparisons of models within a sample, rather than for comparison of the same model across samples. Indeed, with a categorical dependent variable, R^2 cannot always achieve its theoretical bounds of 0 and 1,

If Goldstone is correct about all of these cases, then his argument about displacement goals determining success is also correct. But we maintain that sample 3 presents a more balanced view of the data than either Gamson's original coding or Goldstone's full set of modifications. We therefore give sample 3 considerable weight in our interpretation of the results. We do note, however, that the only major discrepancy between

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and thus it may be a flawed descriptive guide (Morrison 1972; Dietz 1979, app. A). We also note that the variance of the dependent variable in sample 2 (0.182) is smaller than in sample 3 (0.242) so that the explained sum of squares in sample 3 (0.144) is roughly the same size as that in sample 2 (0.142). Such a pattern suggests that the difference in R^2 figures for the two samples may be a result of more limited variance in sample 2. An alternative approach to goodness of fit with categorical dependent variables is based on errors in classifying cases. The predicted values for the dependent variable can be dichotomized by categorizing cases with a prediction above 0.5 as a success and those 0.5 or below as a failure. A measure of association comparing categorized predictions with the actual values of the dependent variable can then be interpreted as a measure of fit. For instance, the lambda for sample 2 is 0.833, while the lambda for sample 3 is 0.632. This suggests that the fit is better for sample 2, but not by so wide a margin as suggested by the R^2 figures. Another way to assess the behavior of the model in the two samples centers on examining predicted values falling outside the 0–1 range. For sample 2, 34% of the predicted values exceeded 1.0, while one was less than zero. For sample 3, 14% were greater than 1.0, while 4% were less than zero. Thus, the estimates for sample 3 are somewhat better bounded than the estimates for sample 2.

[17] These are the American Association of University Professors, Brotherhood of the Kingdom, and the North Carolina Manumission Society.

[18] These are the National Brotherhood of Baseball Players, American Birth Control League, National Student League, Union Trade Society of Journeyman Tailors, and the American Anti-Slavery Society.

[19] These are the American Student Union and the Young Peoples Socialist League no. 2.

[20] Indeed, in sample 2 every nondisplacement group succeeds while only four out of 16 displacement groups succeed. In sample 3, the comparable numbers are five and three.

the two samples is the degree to which the displacement goals variable either dominates the analysis or is simply the most important of several variables determining group success.

CONCLUSIONS

Critics such as Goldstone (1980*a*, 1980*b*) have argued that Gamson (1975) and various proponents of the resource mobilization approach miss the most fundamental fact about the determinants of successful social protest: protest groups cannot succeed if they espouse displacement goals. Groups that espouse reformist goals often succeed, but such success is largely a result of being active during times of national crisis. The ability of groups to mobilize resources and the actions of supporters and opponents matter only to the extent that these factors influence organizational persistence and thus the probability that a group will be active during a period of national crisis.

Our results suggest that the history of social protest in America is not quite that simple. It is true that displacement goals have placed protest groups at a serious disadvantage, reducing their chances of success by perhaps 40%. And groups that were active during a national crisis were more likely to succeed than those that were not, though this effect is much weaker. But the adverse effects of factionalism are consistent across samples and are stronger than the effects of any other variable except displacement goals. Factionalism reduces the resources available to each faction and diverts available resources from use in the struggle against opponents outside the group. Thus problems of organization seem to have been quite important in shaping the success or failure of protest groups.

A model that incorporates both strategy and structural constraints, not unlike that originally suggested by Gamson, seems appropriate for explaining social-movement success. Factors beyond the control of the movement are critically important. Goals that threaten established power are very difficult to achieve. The historical period during which the group is active is important, with success more likely to occur during crises. The strategy of opponents has an influence, with organizations that have been subjected to violence more likely to fail. But the dynamics and tactics of the organization also matter. Above all, organizations must remain unified to achieve their goals. And, as Gamson noted, success comes to the unruly, with those using violence more likely to succeed than those who do not. Not every variable proposed by Gamson seems to matter, but there is more to success than the scope of an organization's goals or the period in which it tries to achieve them. Some displacement goals are achieved, and not all groups with modest goals succeed.

Students of social movements would be well advised to direct more attention to organizational problems of internal movement politics and factionalism. Such an emphasis is present in Gitlin's (1980, 1987) work, which provides an interesting analysis of how the interplay between the mass media and a social movement can facilitate the formation of factions. In a similar vein, Downey's (1986; also see Barkan 1979) examination of the Clamshell Alliance indicates how a strenuous effort to prevent factionalism can itself paralyze a protest group. The strong effect of factionalism also makes sense in light of recent efforts to link social psychology to social-movement theory (e.g., Hirsch 1986, 1990; Klandermans 1984; Snow et al. 1986).

This recent work, while not focused on factionalism per se, suggests a process that may destroy a movement's effectiveness. When factionalism begins, the new faction competes with the parent for resources. External supporters will at best be divided between factions; at worst, they will be too frustrated to support either faction or they will be divided in loyalty and easily alienated. To compete for resources, leaders of each faction will highlight and enhance the differences between them. Often they will denigrate the other faction, thus exacerbating the split. Each faction, seeing the other as the greatest obstacle to its own success, will spend ever more effort on attacking former allies. The need to clearly differentiate from the other faction may lead to internal demands for ideological purity that alienate members and produce still more internal divisions. This cycle continues to alienate members and supporters and diverts each organization from efforts to achieve broader goals. All of this leaves the organizations ineffectual and vulnerable to outside attacks.

It is interesting to note that the recent work that we draw from examines modern protest movements. There is reason to believe that Gamson (1990) may have been premature in his suggestion that the results of *Strategy* are not entirely applicable to the post-1945 era. While the detailed dynamics of protest have certainly been influenced by the rise of the national security state and the mass media, we believe that these new influences act largely to intensify and reshape the internal politics of protest groups. Internal politics remain as critical to the success of excluded groups today as they did to those groups that were active during the 150-year period examined in *Strategy*.

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Book Reviews

The Society of Individuals. By Norbert Elias. Edited by Michael Schröter. Translated by Edmund Jephcott. Cambridge, Mass.: Basil Blackwell, 1991. Pp. x+247. $44.95.

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The Norbert Elias industry continues to flourish. *The Society of Individuals,* originally published in German in 1987, is composed of three essays written at widely dispersed periods of Elias's long intellectual career. The first, written alongside *The Civilising Process,* dates from the late 1930s. The second was completed in 1950, while the third dates from as late as 1987. In view of the disparate origins of the papers, that the book forms an integral whole is a testimony to the intellectual power of Elias's work. At the risk of some simplification, it could be said that every outstanding social thinker concentrates upon a strictly limited number of themes, to be attacked in depth and from various angles. Elias's persistent concern was with the relation of self to society, understood in historical perspective. *The Society of Individuals* provides ample evidence of the richness of Elias's thinking as applied to this most fundamental issue in social analysis.

Self and society, Elias emphasizes, are not two separate entities, but are intrinsically and irremediably interconnected. What is "society" and what is the "individual"? Both terms, Elias says, seem transparent and familiar, but upon examination turn out to be very complex. In the first essay, Elias establishes a position from which he does not later deviate: he sets himself against all views which claim that a society is an organic whole dominating the lives of individuals. He opposes with equal vehemence the methodological individualism which insists that individuals are in some way "real" whereas social processes are not. Each of these contrary positions, as he points out, has political implications. Holistic interpretations readily lead to the view that individuals are morally unimportant when compared to the collective, while the other standpoint often finds it difficult to accept that there are any communal values at all. Each rival perspective, in fact, has been too influenced by its attitude toward moral values: the point, sociologically speaking, is to strip away these aspects and instead concentrate on the realities of social interaction. Individuals and society exist in circumstances devoid of purpose, the condition and result of the endless forms of social association which tie people to one another. Given the time at which it was written this essay

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is an extraordinary achievement, anticipating issues which came to be generally explored in social theory only at a much later date.

Self-consciousness, Elias argues in the second essay, is not just a given phenomenon. Modernity is associated with specific conceptions of self which have a profound impact upon social life. The advent of modern European society produced the emergence of a less egocentric view of the social universe than had previously prevailed: people were increasingly able to see themselves "from a distance," in a decentered way. Here Elias elaborates a standpoint that has interesting connections with the ideas of Kohlberg and Habermas. People in less-developed societies, like children in all forms of social order, have little access to forms of experience which allow them to see themselves as others might see them. People are not yet "individualized" in the ways characteristic of more complex systems. The development of individualization goes along with, and depends on, growing restraint on emotions or feelings, which become subject to greater internalized control. This, of course, is the very process Elias seeks to document in *The Civilising Process*.

In the concluding section of the book, which contains Elias's final reflections on these problems, the discussion concentrates on changes in what Elias calls the "'I' and 'we' balance." The "I—we identity" formed in different societies varies in a radical way, in both its individual and social components. "I" and "we" statements never have a static character, whether looked at in terms of social evolution or of individual development. Thus the "I" of a 40-year-old individual does not refer to that person when she or he was 10; yet, of course, there are continuities of development which at the same time link the "I" of the child and of the adult. It seems on the face of things as though a move toward greater individualization would mean tipping the balance toward the "I" identity. However, Elias seeks to show, more individualized consciousness actually depends on a more thoroughgoing socialization of self. For the "I" is pinpointed in relation to a far greater array of social influences: what a person "is" today is formed in terms of a multiplicity of social criteria, such as date of birth, type of occupation, school records, and so forth.

It is not really possible in a short review to convey the subtlety and delicate insights that Elias offers in this book. Suffice it to say that it is a theoretical work of very high order. It was awarded the European Prize for sociology and social sciences in 1988 and fully deserved that distinction.

The Meaning of General Theoretical Sociology: Tradition and Formalization. By Thomas J. Fararo. New York: Cambridge University Press, 1989. Pp. xi+387. $42.50.

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As an infant science with a rich history of philosophical reflection, contemporary sociology is working to build a collection of powerful theories that simultaneously reflect cherished insights and yield a wealth of testable predictions. While inductive research remains the major component of the empirical side of the sociological portfolio, the portfolio is being increasingly diversified to include tests of propositions derived from formal theories. And while discourse on the foundational thinkers remains a strong element of the theoretical side of the portfolio, the portfolio has substantially diversified to include systematic theories, with exemplars specified at both micro- and macrolevels and expressed in both logical and mathematical language. Thus, the possibility of cumulating reliable knowledge about behavioral and social phenomena is strengthened; and the Holy Grail of finding a few basic principles from which disparate phenomena can be predicted seems less distant.

But as the theoretical enterprise develops, some pressing questions arise: How do all the elements fit together? Are there unifying strands? What methods of theory building will accelerate formalization and unification?

In *The Meaning of General Theoretical Sociology: Tradition and Formalization,* Thomas J. Fararo provides an extraordinarily cogent and comprehensive examination of theoretical sociology, of its roots and traditions, its methods, and its key substantive questions. Fararo argues persuasively that theoretical sociology comprises a single research tradition, that what appear as conflicting theoretical traditions are in effect "intersecting and communicating subtraditions" of the single tradition (p. 342). The key substantive questions concern the "emergence, maintenance, comparison, and transformation of social structures" (p. 62). To study these basic questions, Fararo proposes a "process worldview" whose key idea is "recursive generativity": *"The interactive nexus of human organisms generates transformations in both the individual humans as such and the advancing nexus they constitute"* (pp. 47–53; italics in the original). The process worldview leads naturally to a set of methods whose centerpiece is a nonlinear dynamical social systems framework. Within this framework, a principal goal is to formulate and prove four types of theorems, which correspond to the four key substantive questions.

Having examined, in chapters 1 and 2, the philosophy and the methods of general theoretical sociology, Fararo turns, in chapters 3 and 4, to consideration of two key theories, action theory and structuralism. Fararo analyzes and contrasts what he sees as three branches of action theory: (1) analytical action theory, focused on Parsons's work, (2) interpretive sociology, and (3) adaptively rational theory, including both the tradition of Homans and Emerson and the rational choice theory propounded by Coleman. In the chapter on structuralism, Fararo distinguishes two branches, network analysis and Blau's macrostructural theory. A key element in this chapter is the notion of biased networks, to whose development Fararo has made important contributions. Both chapters 3 and 4 are characterized by incisive and sustained analysis, deploying the methods developed in chapter 2 and based on the process worldview described in chapter 1. Fararo's investigations show over and over again that applying the right tool can yield important insights and results. Throughout, the spirit is that of cumulative theorizing, with many theoretical inquiries still ahead.

This brief overview does not do full justice to the richness of either the ideas or the methods. Fararo has written a thoughtful, careful book that probes searchingly the theories and metatheories of sociology. Among other things, the book includes a detailed section relating positivist with instrumentalist and realist conceptions of theory, a superb introduction to the methods of systems theory, and a gem of a sentence concerning theoretical analysis: "A point worth remembering is that when a theoretical model is under analysis, all statements are with reference to possibility, not actuality" (p. 76). Finally, sounding a theme from Merton's classic essay, "Sociological Theory" (*American Journal of Sociology* 1945), Fararo argues for a greater emphasis on the development and study of *theoretical methods*.

The combination of philosophical issues, sociological questions, and theoretical methods that characterizes Fararo's book makes it likely that the book will become a standard reference for working theorists. The book, or sections thereof, is also appropriate for a variety of graduate courses, including courses in metatheory, classical sociological theory, contemporary sociological theory, social psychology, macrosociology, network analysis, and tools courses. Two features enhance the book's usefulness as a reference text and as a course text: a one-page chart delineating the logical interrelations among the main ideas in the book (p. 7) and the concluding summary which distills the main arguments of the book into a sentence outline (pp. 342–49). In sum, Fararo's book substantially enriches theoretical sociology.

Communities of Grain: Rural Rebellion in Comparative Perspective. By Victor V. Magagna. Ithaca, N.Y.: Cornell University Press, 1991. Pp. xii+277. $42.50.

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Communities of Grain is distinguished by an extraordinary wealth of reading in secondary sources. The footnotes alone for the chapters on rural change in England, France, Russia, Spain, and Japan will provide graduate students and scholars with valuable guidance to current knowledge in these areas. Magagna is somewhat less successful in distilling theoretical insights from this material, but the book is nonetheless a worthwhile attempt to bring together historical, anthropological, and sociological accounts of rural organization and conflict in a variety of settings.

Magagna argues, in a vein now familiar to social historians, that class-based analyses—in this case of peasant societies—are incomplete at best and downright misleading at worst. The basis for rural organization, and the framework for rural conflicts, was the community as a whole. This could be a village community based on close settlement and on the shared control of land or of agrarian routines. Or it could be a parish community based on the moral, social, and cultural ties among dispersed smallholders. Or it could be a squatter or migrant labor community based on the shared struggle for existence and on close contact in field labor gangs and crowded settlements.

Magagna concedes that readers may object to grouping such diverse social organizations as the Russian *mir,* Spanish *pueblos,* and the English commercial farm village. However, Magagna makes his case that, despite differences in social organization, for members of agrarian populations the main point of reference for political and social issues was not their companions in economic status or opportunity, but their entire local rural community, from its rich peasants and elders to its poor widows and day laborers. It was as members of this community that they made their decisions regarding family, livelihood, and (if need be) political conflict with landlords and agents of the state.

This perspective is undoubtedly correct, and the case is worth making. Moreover, Magagna goes far beyond simple Chayanovian views of a self-centered peasantry, noting that there are a variety of types of community and usefully distinguishing between redistributive communities, regulative communities, and open commercial communities. In the first, the community corporately allocates resources (e.g., the Russian *mir*); in the second, the community corporately regulates families' use of their own property and resources (e.g., French and English open-field villages); in the third, the community provides ritual anchoring and various insurance and public-goods functions, but families use their private resources as they please (e.g., dispersed settlement or enclosed-field villages in England and France).

Nonetheless, while this book is valuable for turning us toward an important social reality, it is far less successful in drawing causal relations between the kinds and degrees of community and the kinds and degrees of rural rebellion. Magagna offers several hypotheses in his conclusion, but they are vague, asserting that more authoritarian regimes will provoke more community resistance, that community bonds will weaken when people have alternative forums in which to protect themselves from elite depredations (i.e., regional and national representative institutions), and that strong ritual displays indicate (cement?) strongly cohesive communities.

None of these conclusions is drawn with enough sharpness or audacity to challenge what sociologists have already found in Durkheim and Weber. Much of this weakness stems from a questionable selection of cases. Magagna has followed his data and his interests, rather than let his hypotheses guide case selection. He fails to compare and contrast community organization and patterns of rebellion over time and space; for example, he claims, regarding 17th-century French peasant rebellions, that, "although regionally confined largely to the west, south, and parts of the center, these episodes reveal a common logic that throws into sharp relief the dynamics of community politics and bureaucratic domination in rural France" (p. 137). But they in fact do not—they throw into sharp relief the *differences* between the west, south, and center of France and the much more commercialized north and east.

Magagna also includes the case of Japan to add a non–Western European contrast, largely because of the marvelous data on Japan's early modern peasant uprisings (*ikki*) collected and now available to nonspecialists. Yet aside from showing that Japan, too, had peasant communities, this case adds little to the comparative analysis, because peasant villages in preindustrial Japan were in many ways much like those of Western Europe. It would have been far more instructive to examine Prussia (where authoritarian landlord rule did *not* provoke rebellion) or China (where the *failure* of state power to control banditry, to maintain irrigation and grain reserves, and to limit bureaucratic corruption and abuse *did* provoke rebellion), namely, to look at areas where peasant organization and rebellion showed distinctive patterns.

Readers will not find in Magagna another Eric Wolf or Theda Skocpol with major insights on rural rebellion. However, readers seeking a rich survey of rural community institutions in preindustrial Europe will find this a highly valuable work.

Collective Action and the Civil Rights Movement. By Dennis Chong. Chicago: University of Chicago Press, 1991. Pp. 261. $42.50 (cloth); $16.95 (paper).

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New York University

Students of social movements and collective action inevitably confront the freedom-rider problem: Why contribute to efforts to achieve goods from which all will benefit—especially when one seems to have little to gain? In his lively and closely argued *Collective Action and the Civil Rights Movement,* Dennis Chong gives a novel and plausible answer to why people engage in what he calls "public spirited" collective action.

Chong addresses two questions: How do rational individuals decide to participate in such social movements? How do individual decisions translate into collective outcomes? In answering these he develops a general model of collective action, drawing mainly on the civil rights movement—which is so well studied that Chong can construct his theoretical treatise without going beyond the secondary literature. Chong works from a rational-choice vantage point, from which the decision to engage in collective action resembles a prisoner's dilemma, a game in which individuals have no incentive to cooperate—to their mutual disadvantage. Following Anthony Oberschall, Doug McAdam, and Aldon Morris, Chong views public-spirited movements as drawing on social organizations and networks. Thus the situation resembles an iterated prisoner's dilemma, in which cooperation is possible.

Yet people are likely to participate only if potential collective benefits are supplemented by social gains from participation. At this point in the argument Mancur Olson meets Erving Goffman. Or perhaps Chong is reuniting the Adam Smith of the *Theory of Moral Sentiments* with that of the *Wealth of Nations* in proposing that a concern for one's reputation often underlies participation. People want to maintain friendships. Someone who expresses public-spirited political views is expected by others to act on them and is sanctioned accordingly. Participation has its own rewards, but Chong sees these as only somewhat helpful in explaining why people engage in collective action. These "expressive" benefits such as feelings of efficacy and thrills from engaging in collective endeavors are minimal in losing causes. Who enjoys shouting in the wilderness? Chong concludes that paradoxically many will participate only when collective action verges on success. Expressive benefits can transform a prisoner's dilemma into an "assurance" game—a more tractable collective action problem. This implies that some, the truly committed, must pay the heavy start-up costs of organization and must bring the movement to the brink of victory.

From these arguments Chong develops a dynamic model of mobilization. Chong posits that movements usually attempt to win concessions from governmental authorities and examines the parameters of leadership, community, obligations, resilience, success, government responsiveness, and opposition. To his credit, he does not assume as do most resource mobilization theorists that demands are constant and high and of little explanatory value. Chong also argues that initial pushes to mobilization might come from the supply or demand side and that activist administrations can spur mobilization. He cleverly twists the model to display various strategies to aid mobilization and implies that the decline of the civil rights movement was caused not by disagreements among movement organizations or by repression, but mainly by the movement's successes.

This is merely the skeleton of the argument, and along the way Chong provides insightful analyses of other collective action problems. For instance, he argues persuasively that nonviolent protest designed to provoke a violent response by opponents—a key tactic in the civil rights movement—is an all-or-nothing situation in which everyone must cooperate else the collective good is lost. In this public relations game, as Chong calls it, one unruly protester will discredit all and justify violent retaliation; thus each protestor has a convincing reason to stick to the collective pact. He notes that leaders of movements might learn from this example to orchestrate campaigns to make them appear as if each person's effort is required for success.

Although the many tables, graphs, and charts frightened me into shelving this book for a month, in Chong's presentation they are not at all inhibiting. His disputes with theoreticians such as Russell Hardin and Albert O. Hirschman will captivate specialists, but will not oppress other readers, for Chong's arguments are enlivened with examples and anecdotes and findings from related social movements. Though marred by occasional awkwardness—Chong repeatedly calls activism "rabble-rousing"—the book is fun to read and is suitable for undergraduate and graduate instruction.

The book is not without problems. Chong's arguments are often persuasive, but his evidence is mainly anecdotal. Nowhere does he advance a systematic research design to answer questions about the overall rise and decline of the movement and its successes and failures. Moreover, Chong's arguments beg a number of questions. Chong argues that previously organized groups and networks are crucial to staging mobilizations, but does not indicate *which* organizations and networks might serve as conduits for political action. In any case, with issues as varied as the environment, peace, and civil and women's rights, how would one predict which public-spirited movement an individual would join? He notes the importance of truly committed activists in getting the movement to the brink of success, but he does not indicate their sources. Chong is convincing in arguing that reputations can matter, but why do people desire such reputations in the first place? I doubt that similar explanations might be employed for different participants in the civil rights movement. Civil and voting rights were crucial for all African-Americans, especially in the South. Whatever benefits, reputational or otherwise, the movement had in store for a white volunteer, these could not have been the same. Chong also stumbles in confronting what might be called the freedom-rider problem: Why are people willing to engage in high-risk collective action? The reasons that bring one to put one's life on the line may not be similar to those that persuade one to protest or boycott. Finally, Chong's conceptualizations and analyses of the successes and failures of movements are at best schematic. As Chong notes, mobilizing is only a necessary step in achieving success. Yet the additional circumstances under which movements will win legislation to aid their causes and the role of the government and the opposition are left sketchy.

All the same, no single book can do all things, and what Chong attempts he does well with admirable style and verve. All students of social organization and social movements, especially those addressing specific research questions with middle-range theories, can profit from Chong's penetrating analysis of why people join public-spirited movements.

Revolutions of the Late Twentieth Century. Edited by Jack A. Goldstone, Ted Robert Gurr, and Farrokh Moshiri. Boulder, Colo.: Westview Press, 1991. Pp. xxii+395. $55.00 (cloth); $19.95 (paper).

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McGill University

Revolutions of the Late Twentieth Century has its origins in the desire of Gurr and Moshiri to analyze the most recent revolutions in the light of modern social science theory. The actual existence of the book clearly depended, however, on the considerable drive and determination of Jack Goldstone. It is particularly noticeable that the analytic categories employed in the case studies are those of Goldstone's important *Revolution and Rebellion in the Early Modern World* (1991). Distinctions are accordingly drawn between state breakdown (seen to derive from a conjunction of the state's fiscal problems, elite alienation, and popular discontent—all fueled, for Goldstone, by demographic change), revolutionary process, and revolutionary outcome. One valid reason for reading this book is to see how Goldstone's model fares when confronted with modern revolutionary experience. But the book will attract readers for another reason: the case studies—Vietnam, Nicaragua, Iran, Poland, Afghanistan, the Philippines, Cambodia, Zimbabwe, South Africa, and the *Intifada* of the West Bank and Gaza—are simply excellent, concise, elegant, and informative. Moreover, the cases are nicely varied, ranging from those state breakdowns which lead to reform and democratization to others which result in genuine revolutions. Reasons of space rule out consideration of particular cases, but it is possible to say something about the nature of contemporary revolutions and about the analytic factors used to structure analysis.

The justifications offered for considering late-20th-century revolutions as having a specific character of their own are interesting and convincing. First, the regimes which collapse are less traditional agro-literate polities than semimodern and neo-patrimonial in character. This is not to say that traditional principles are of no use in understanding events. That many regimes, archetypically that of the Shah, became "autonomous" from their societies—through relying on foreign aid, which provides the modern weaponry to destroy some opponents—only at the cost of isolation so splendid that no one came to their defense would not have surprised Tocqueville. Second, Goldstone insists that the inability of the superpowers to control their clients, something he holds to have been particularly evident in the last two decades, has allowed novel space for revolution. This point is all the more convincing because it is not misused to deny the need for sustained attention to the impact of international forces. In this connection, it is worth noting that postwar revolutions are made likely by dependency on a single power; put differently, contemporary revolutions are strongly linked to and intertwined with nationalism. Third, many of the cases show that revolution has come to depend on democratic coalitions of fragments of the middle class and the intelligentsia. But such coalitions have rather different goals and agendas than do revolutionary parties, and this—together with the presence of skillful third-party mediators—makes it at least possible that some contemporary revolutions may be sufficiently self-restrained as to allow the introduction of liberal democracy rather than of an alternative power system.

Goldstone's analytic factors prove their worth in every case study, and accordingly make one fully realise how helpful has been his general contribution. But two critical points should be made. First, Goldstone's factors make most sense when used loosely. Differently put, the reductive combination of Malthusianism and rational choice theory seen, especially in his own major work, in statements about the importance of blocked mobility is not really convincing: elites can be alienated for a whole host of reasons, and sometimes when their immediate career prospects are not in doubt. Second, many of the cases make it very obvious that Goldstone has neglected one key factor in his otherwise impressively eclectic argument. Given the chance of reform, most people prefer to avoid the perils of death on revolutionary barricades. In other words, it is political exclusion which radicalizes quite as much as do socioeconomic conditions of whatever sort. When such exclusion is combined, as with Marcos and Somoza, with inefficient nepotism, the chances of revolution become extremely high.

In a nutshell, this book is extremely high powered and thought-provoking, and it accordingly deserves the largest possible audience. I hope that there will be many later editions so as to allow these cases to be updated and others to be added. Obviously, we now need to hear more about the demise of Soviet state socialism, and especially about the way in which nationalism was used to bury it. But the interest shown here in routes to democracy also suggests sustained consideration of cases such as Korea and Taiwan. Democratic coalitions hold together best against regimes which ruin the economy *and* exclude politically. How stable are authoritarian regimes that manage capitalism successfully?

The Animal Rights Crusade: The Growth of a Moral Protest. By James M. Jasper and Dorothy Nelkin. New York: The Free Press, 1991. Pp. x+211. $22.95.

George W. Wenzel
McGill University

In *The Animal Rights Crusade,* Jasper and Nelkin undertake an extremely complex task—the description and analysis of the animal rights movement (ARM) as a growing focus of popular moral action. Just the first of these, given the many variants identifiable within the corpus of the movement, is a most formidable task. Here, the authors have chosen to examine the complex that is the ARM in terms of specific protest foci (chaps. 6–11), moving from the basal situation of relations between humans and wildlife to the commoditization of animals by the science, business, and entertainment industries.

The Animal Rights Crusade is successful in its first objective, that of describing the myriad variations that mutate within the animal protection movement. Analytically, however, this book is less successful, mainly because it relies heavily on narrowly drawn vignettes about ARM protesters, their activities, and the reactions they provoke. Ultimately, this approach provides much needed (and appreciated) deep description, not to say color, but says little about structural relations (whether ideological, political, or economic) within the movement or as they exist between protesters and the larger society.

Oddly, there are several aspects of the movement that are absent from such an overview work. The book is almost exclusively concerned with recent (post-1960) animal rights developments in the United States. Thus, little material is presented about the long historical and religious roots of the movement in the United States. Another missing element concerns the consciousness raising/action role of the movement in Europe, where animal rights issues strongly influence near-mainstream Green political activity in Sweden, France, Germany, and, to a lesser degree, the United Kingdom. Finally, the book does not clearly distinguish the liberationists' path within the ARM, a description that would have been useful if only because it would define the extreme of protectionist fundamentalism, a major theme here.

In many ways, I see *The Animal Rights Crusade* as, rather than half-full, half-empty. It is, in fact, a solid introduction to the activities and direction of the modern animal rights movement in the United States. Unfortunately, it depends too much on brief sketches and not enough on detailed analysis. The only chapter that does go beyond the descriptive, "Philosophers as Midwives," does an excellent job of explaining the Singer-Regan dynamic that exists within one stratum of the movement.

In too many other instances, the readers are left to achieve a measure of critical depth on their own. Chapter 6, "Animals in the Wild," illustrates this point. In this chapter, perhaps the most successful animal rights action so far in North America, the harp seal protest, is relegated to barely two full pages. It is especially disturbing that no attempt to understand the efforts of the Canadian government to meet the objections of the protest is presented. (The interested reader can find more information in *Seals and Sealing in Canada: Report of the Royal Commission* [Ottawa: Supply and Services Canada].) Even worse, the authors accept the movement's own perspective on its liberal responsiveness to the concerns of indigenous peoples (p. 88). There is considerable evidence, however, to suggest that the bare accommodation that has been achieved between the ARM and native peoples is minimal and primarily serves ARM interests. Unfortunately, this lack of wider perspective is not limited to the discussion of ARM responses to the consumptive use of wildlife. Indeed, the whole rural versus urban dialectic that underlies such larger issues as the need to overthrow the industrial farming system is missing from this presentation.

In the closing paragraph of *The Animals Rights Crusade,* the authors caution that to treat of the movement "as irrational, kooky, or terrorist is to miss the popular appeal of its moral intuition and political beliefs." I would submit that many of the fundamentalist tactics employed by ARM protesters, while certainly not irrational or kooky, do speak volumes about the political beliefs of the movement's hard-core elements. Leaving aside the assaults committed by the liberationists, a wide gap still remains between democratic conversation and the impatience that pervades much of the ARM's mainstream.

Dishing It Out: Waitresses and Their Unions in the Twentieth Century. By Dorothy Sue Cobble. Urbana: University of Illinois Press, 1991. Pp. xiv+327. $34.95.

William Finlay
University of Georgia

The increasing number of women in the labor force is sometimes cited as a cause of union decline in the United States. According to this argument, industries in which women are heavily represented are difficult to organize. Dorothy Sue Cobble's account of waitresses and their unions, however, dispels the myth of the hard-to-organize female worker. The period during which women came to dominate the occupation of waiting tables—between 1900 and 1950—was also the period in which unionization of eating and drinking places by the Hotel Employees and Restaurant Employees International Union (HERE) reached its peak. The percentage of women in the occupation rose from 40% in 1900 to over 80% in 1950; the percentage of unionized eating and drinking establishments rose from less than 6% in 1910 to 25% in 1950. In cities like Oakland and San Francisco, more than 80% of these establishments were unionized in 1961.

Waitress unionism, Cobble suggests, belongs to the tradition of militant craft unionism—which she terms "occupational unionism"—that is customarily associated with the struggles during the 1930s of longshoremen, seamen, and miners. The philosophy and practice of the HERE waitress locals was very similar to that of unions in these other industries: they sought to control entry into their occupation via the mechanism of the union-run hiring hall instead of having unchecked hiring by employers; they preferred to share the work during tough economic times rather than have workers laid off by seniority; they favored self-regulation over managerial authority to maintain work standards and craft jurisdictions.

The occupational unionism of waitresses, like that of longshoremen and seamen, reflected their preunion economic and social circumstances—long hours and low pay, a high rate of interemployer mobility, a sense of craft pride that emphasized the inherent value of their work and the often overlooked skills necessary to do their jobs well, and a work culture based on the strong personal ties that developed among waitresses on the job. Unlike other female workers, therefore, waitresses developed distinctive occupational communities which in turn provided the foundation for their union.

Not only did the occupational unionism of waitresses mirror that of workers in predominantly male industries with similar conditions of work, but there was in addition a symbiotic relationship between these male and female workers—waitress unionism reached its apex in cities like Oakland, San Francisico, and, to a lesser extent, Detroit and Los Angeles where waitress union drives received the most support from male workers in other industries. Cobble thus rejects the argument made by some feminist scholars that working-class men have been hostile to female organization, although she notes that the male culinary workers seldom supported their female counterparts.

Over the long run, however, waitress unionism failed to maintain its initial successes. By 1975 less than 10% of all eating and drinking establishments were unionized. By 1980 HERE membership had dropped below 1950 levels. In its inability to maintain its gains, waitress unionism was significantly different from the other occupational unions of the era. What had gone wrong?

Cobble cites some general factors instantly recognizable to anyone familiar with the decline of the U.S. labor movement after 1950. Like other businesses, hotels and restaurants moved into territories unreceptive to the unions: the Deep South, the Southwest, and the suburbs. The Taft-Hartley Act and the conservative political climate of the postwar years encouraged antiunion activity. Other factors, however, were more specific to waitress unionism. Perhaps the most significant of these was the disappearance of independent proprietorship and the rise of chain-operated restaurants under conglomerate control. The fast-food emporiums in the suburban shopping malls with their young, part-time workforces and family-oriented menus were an infinitely more formidable proposition for union organizers than the coffee shops along the San Francisco or San Pedro waterfronts. The decline of waitress unionism was hastened also by the eruption of generational conflicts within HERE itself. Traditionally, HERE leaders had pursued a policy of gender separatism to advance their members' interests; in the 1970s younger activists rejected this policy in favor of equal treatment under the law for men and women. The working-class feminism pioneered by HERE seemed outmoded in a society in which discrimination on the basis of gender was illegal.

Cobble has written an excellent analysis of the rise and fall of waitress unionism. My only complaint is that she did not devote enough attention to explaining why this movement never flourished in cities like Chicago, Philadelphia, and Pittsburgh. If, as she argues, the support of male workers in other industries was critical to waitress unionism, much could be gained by learning whether this support was withheld in these cities and, if so, why it was withheld.

The Pro-Choice Movement: Organization and Activism in the Abortion Conflict. By Suzanne Staggenborg. New York: Oxford University Press, 1991. Pp. xiv+229. $27.50.

Clarence Y. H. Lo
University of Missouri

Staggenborg's *The Pro-Choice Movement* portrays the hard political choices that an important part of the women's movement had to make in the inhospitable 1980s. But more, the volume is a meticulously researched exemplar of historical sociology, in which recent events gain added significance because of their connection to theories of political change. To conceptualize her findings, Staggenborg mainly uses resource mobilization theory (now the leading paradigm of social movements researchers) and contributes to current discussions about cycles of protest, bureaucratization, movement-countermovement interaction, and polity membership.

Chapters 2–4 convincingly show that the pro-choice movement must be placed in the context of the cycle of 1960s protests. The civil rights, student, and feminist movements innovated tactics and mobilized constituencies, contributing to the rise of the abortion rights movement. The remaining chapters present Staggenborg's major contribution, her analysis of the later stages of the cycle. A victory for the movement (in this instance the 1973 *Roe* decision legalizing abortion) eventually enabled pro-choice groups to become more legitimate, garner increased contributions from established organizations, hire paid staff, develop explicit procedures, and organize local affiliates. This professionalization and formalization helped the pro-choice movement not only to survive, but also to grow over the years.

Staggenborg presents anomalous evidence to question two theories—the iron law of oligarchy (that political organizations inevitably become hierarchical and conservative as they persist) and the Piven and Cloward thesis (that bureaucratization kills protest movements, p. 154). She carefully documents the degree of formalization, which varied over time among pro-choice organizations (p. 94); a few (e.g., the Reproductive Rights National Network) remained decentralized and pursued broad goals with militant tactics. Contrary to Piven and Cloward, formalized groups at times also organized direct action. Staggenborg takes from us our unwarranted historical generalizations but she also leaves us with little new sense of deep historical causes, taproots, or structural developments. We only see organizations reacting strategically to exogenous (and unexplained) events, in this case the Supreme Court decisions in 1973 and 1983, which upheld legalized abortion and led the pro-choice movement to bureaucratize and grow.

So let us appreciate, then, this finely crafted, primary-source scholarship on the past, with its sense of multiple, contingent consequences. Pro-choice victories, besides leading directly to the expansion of the movement, mobilized an opposing antiabortion countermovement, the threat of which eventually also increased the mobilization of the original pro-choice movement (p. 81). The pro-choice movement emerged stronger, but more narrow—committed to the single goal of keeping abortion legal—emphasizing electoral politics and the lobbying of Congress. Staggenborg's history should not be used to verify a general proposition about social movements, for surely the threat of a countermovement does not always increase mobilization in the original movement. In fact, Staggenborg's book shows us that the live history of postmodern movements cannot be force-fit back into the cells of numbered propositions, which were the fashion of resource mobilization theory around 1980. Rather, she shows us that at a particular historical juncture, the pro-choice movement made strategic responses to events; it was galvanized into action when antiabortionists lobbied Congress to pass the 1976 Hyde Amendment and when the Supreme Court decided in 1989 in favor of state regulation of abortion (p. 137).

Staggenborg also seeks to depict the empirical complexities that muddle the sharp contrast that Tilly and Gamson have drawn between polity members (who win concessions and are routinely consulted by government) and political challengers (p. 153). Her portrayal of the pro-choice movement before the 1973 *Roe* decision rightfully shows that the movement then had many of the characteristics of a challenger. Important parts of the movement favored broad feminist goals and direct-action tactics, had a decentralized and informal structure, won little support from established interest groups (but mobilized activists from those organizations), and had little success in electoral politics. Staggenborg then makes the questionable argument that the movement later became a polity member, particularly around 1990 as pro-choice support helped to elect candidates like Douglas Wilder in the Virginia gubernatorial race (p. 139).

Staggenborg rightly sees the polity member/challenger distinction as a continuum. The distinction, however, was never meant to imply that the polity includes all groups who organize politically, for one can contend for power and still be a challenger. The distinction was meant to show that with the modern state, political power has become centralized and very unequally distributed. Some groups like the antiabortion movement in the Reagan and Bush years were highly favored, not only with Presidential speeches and symbolism, but also with legislation, federal agency decisions to restrict abortion, and a flood of antiabortion appointments to the judiciary and to the Supreme Court, the ultimate arbiter. Staggenborg's assessment of the movement is that it has survived the doldrums, proud and strong. But the painful fact is that there is now an anti-*Roe* majority on the Supreme Court, all set to trample wild. What we need for the 1990s is an analysis of the forces that may rescind women's rights to safe and legal abortion.

Tales of Nationalism: Catalonia, 1939–1979. By Hank Johnston. New Brunswick, N.J.: Rutgers University Press, 1991. Pp. xviii + 243. $40.00.

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Free University, The Netherlands

Tales of Nationalism is a case study of an ethnic nationalist movement—the Catalan movement in Catalonia, Spain. But, it is more than an average case study; it tries to elaborate aspects of movement mobilization that until recently have been neglected. Although he does not aim at replacing resource mobilization as the dominant approach in social movement literature, Johnston attempts to reformulate mobilization theory by emphasizing the primary significance of culture. This is a timely endeavor as the interest in culture in the area of social movements is growing rapidly. Johnston's study is not only theoretically innovative, it takes new methodological roads as well. It is an attempt to introduce discourse analysis in the context of mobilization, recruitment, and movement participation.

The author begins with the assumption that the rebirth of nationalism after periods of suppression can perhaps be predicted if more attention is paid to cultural processes. One need not to go that far to appreciate the importance of cultural processes in movement mobilization. Including culture in the analysis expands the time frame, because culture has its roots in the past Johnston argues. He, therefore, starts his analysis with a brief overview of Catalan history demonstrating that Catalan nationalism was already present in previous centuries. When Franco came to power any expression of regionalism was repressed by force. Because Catalonia—especially Barcelona—was the paragon of everything Francoism abhorred, suppression in that region was worse than anywhere else in Spain. Every single feature of Catalan culture (language, literature, songs, traditional dances, and so on) was outlawed down to the details of everyday life. Thirty years later a movement encompassing nationalists, clerics, communists, socialists, migrant workers, intellectuals, and the Catalan middle class succeeded in reclaiming what Francoism had wanted to erase.

The core question of Johnston's study is how forces that were so incompatible elsewhere could unite Catalonia. It will not come as any surprise that his answer points to the significance of culture. In his attempt to answer this question, the author draws on in-depth interviews with 82 militants from the Catalan movement about their personal histories. The following is a review of some key observations.

Contrary to what theories on that matter suggest, Johnston observed that state repression was far from complete; in the private sphere especially ideological dominance was never total. He demonstrates how a "subculture of opposition" continued to exist. What Francoism tried to eliminate—the Catalan life-style—was kept alive in private homes by parents who taught their children Catalan language, symbols, and literature. In addition, nonpolitical organizations such as the church, scouting groups, choral societies, and natural conservation associations kept the Catalan heritage alive.

Much of what follows in chapters 4–8 are accounts of how people from the main currents in Catalan society—workers (both socialists and communists), nationalists, and Catholics (clerics and the middle class)—gradually reframed their definitions of the situation so that they eventually adhered to the same master frame that connected Catalan nationalism with anti-Francoism. Of course, the Francoist government helped a lot by continuing its repressive policies, even after it had agreed to modest liberalizations. Catalans were quick to reclaim and exploit this new space to organize and express themselves, which in turn led to new acts of repression, some of which were crucial in drawing new constituencies into the movement.

At this stage of the mobilization process familiar aspects of mobilization-like organizations and friendship networks enter the equation. Overarching structures of elites from different social sectors of Catalonia began to act together in organizing opposition to Francoism and created the bridge between framing processes and mass mobilization (chap. 9). All this culminated in an extended period of ten years of mass mobilization. Long enough, Johnston argues, to apply the concept of "culture of mobilization." During these years Franco and Francoism died (chap. 10), but the "culture of mobilization" continued to exist several years longer producing election results diverging from those in other parts of the country. Is Johnston's case for the inclusion of culture in theories of mobilization convincing? I leave it to the reader to decide for him/herself, but want to conclude with a few remarks.

What if Francoism had not repressed regional culture so vehemently? Would there still have been a Catalan movement and would that movement have taken an anti-Francoist direction? In other words, is it Francoism or culture or both that created anti-Francoist nationalism? To what extent were the developments described unique to Catalonia? I was puzzled that hardly any reference was made to developments in other parts of Spain or Europe. It is difficult to imagine that Catalan mobilization was uninfluenced by events elsewhere. During the same period of mobilization that the author describes (the 1960s and 1970s), mass mobilization was taking place all over Europe. Students, workers, and clerics alike were mobilizing against their governments, universities, employers, and so on. It is again hard to imagine that it was just Catalan culture that kept the Catalans going.

Finally, a methodological remark. The interviews with militants were retrospective. In recalling their personal histories, individuals tend to see more continuity in their lives than there may have been. Events in the past become redefined according to new frames. Hence, some elements of the "subculture of opposition" may be constructions in retrospect. This also makes it difficult to conceive of discourse analyses of these interviews as analyses of the "daily play of member interaction." At best it concerns rehearsals of interactions in a more or less distant past. All this is not to say that Johnston has lost his case for culture as an important element in movement mobilization. On the contrary, by looking through the glasses of culture he observed aspects of mobilization that might have been otherwise overlooked.

Ethnicity and Nationalism in Post-Imperial Britain. By Harry Goulbourne. New York: Cambridge University Press, 1991. Pp. xiv+271. $54.95.

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The resurgence of ethnic nationalism throughout the world has been a central concern of social scientists since the 1970s. The dismemberment of the Soviet Union and Yugoslavia into ethnically defined republics makes this topic all the more salient today. Thus, Harry Goulbourne's *Ethnicity and Nationalism in Post-Imperial Britain* is timely. And, though Goulbourne focuses exclusively on the British experience of this phenomenon, and specifically on immigrant (diasporic) politics, the issues he raises have much wider relevance.

Goulbourne's purpose in this book is to show how Britain has come to define its national community in terms of ethnic affinity, rather than in terms of a larger multiethnic, multiracial entity sharing an allegiance to the British state. Specifically, he focuses on the ways in which the most influential attempts to redefine the postimperial British national community have excluded the nonwhite former colonial peoples who immigrated to Britain since World War II. While these immigrants may be formally recognized as British citizens, this recognition is constructed in such a way that the legitimacy of their presence in the British Isles remains questioned. As a result, Goulbourne claims, Britain is now in the vanguard in the development of a "decisively new" breed of nationalism that is ethnically defined and that leads to what he calls the "communal option" (e.g., a congruence between "the people" and the state, or state recognition and support of autonomous corporate entities representing "the people"). The communal option, Goulbourne notes, is espoused by both proponents of the politics of difference and indigenous cultural conservatives (a.k.a. "exclusivist conservative culturalists" or "little Englanders").

Goulbourne is not willing to attribute this outcome simply to racism. He argues that in the retreat from empire, an ethnic perception of the

British nation emerged among the indigenous (English) population. This cultural perception of British nationhood manifested itself in rejection of colonial immigrant groups, and this reception experience, in turn, encouraged ethnic mobilization and diasporic politics among Britain's immigrant minorities. Goulbourne illustrates this process with case studies of the diasporic politics of Sikh and Guyanese communities in Britain. A third case study, focusing on Cypriot immigrants, was planned but could not be completed in time for inclusion in this book.

The book unfolds in two stages. Following a brief introduction, the first three chapters are devoted to discussion of the concepts of nation (community) and nationalism and of traditional versus ethnic forms of nationalism. The author does not attempt to build a tight analytical framework; rather, in these chapters he offers reflections and commentary on a series of loosely connected issues and problems surrounding the amorphous notions of community and nationhood.

The empirical material is presented in the following three chapters. Chapter 5 traces the development of an indigenous ethnic nationalism in Britain, primarily as expressed through nationality and immigration laws. Chapters 6 and 7 examine the reception experience and nature of diasporic politics among Sikhs and Guyanese, respectively.

In all three empirical chapters, Goulbourne focuses on the actions and statements of political elites. The case studies of Sikh and Guyanese diasporic politics are based on over 30 in-depth interviews with community spokespersons and political leaders, as well as on an unspecified number of group interviews and visits to community centers, *gudwaras,* and so forth. But despite the availability of this material, the chapters draw most heavily on "official" sources such as political party statements or newspapers.

The strength of this book lies in the many provocative issues that it raises. Goulbourne clearly mourns the trend to organize according to "supposed primordial affinities," instead of working to build an all-embracing national community. His concluding chapter offers suggestions (perhaps optimistic) for altering the present course of nationalism in Britain. This position is likely to stir strong responses from those on both sides of the issue.

But I found myself wishing for more. The book is primarily a combination of description and commentary, rather than analysis. I would have appreciated fewer general remarks and a greater emphasis on the *analytical* discussion of the connections between the decline of empire and the ethnic "rediscovery" of majority groups, and of the dynamics connecting colonial background, reception experience, and diasporic politics. Moreover, though Goulbourne conducted interviews with leaders of the Scottish Nationalist Party, he says relatively little about how the rise of the SNP or Celtic nationalist movement fits into his general framework. Sociologists are also likely to find the overwhelming focus on official political actors narrow, and will wish that more material had been included from the case-study interviews. Nonetheless, *Ethnicity and Na-*

tionalism in Post-Imperial Britain raises important issues. And, while many readers will be at least generally familiar with the politics of race in Britain and with the Sikh movement, Goulbourne's discussion of the Guyanese situation is particularly valuable in bringing this relatively unknown case to the attention of scholars.

Economic Origins of Antisemitism: Poland and Its Jews in the Early Modern Period. By Hillel Levine. New Haven, Conn.: Yale University Press, 1991. Pp. xiii+271. $30.00.

Victor Karady
CNRS

In his well-researched and carefully composed study of Jewish-Gentile relations in matters economic, political, intellectual, and properly social during the long period leading to the final partition of the Polish kingdom (1795), Hillel Levine develops his arguments with two paradigmatic images as starting points. The first one refers to "the broken bridge" in the gentry estate managed by a Jewish lessee (*arendar*), which, though a nuisance for all, will never be repaired. It represents the self-perpetuating backwardness of the country, where none of the economic agents has a vested interest in infrastructural investments of common utility. This may serve as an introduction to one major topic of the book, the exploration of reasons for Poland's failed modernization. In the second image the Jewish tavernkeeper emerges as the truly emblematic figure of rural Eastern Europe in premodern times. He embodies in a persisting feudal framework the social separateness and economic particularism of Jews performing, to the benefit of the gentry, an essential function in the additional and disguised exploitation of the peasantry. His role is to divert scarce monetary resources from productive and innovative outlays and manifests an aspect of Polish society's final break from Western-style development. The main thrust of the study owes much to the "interpretive science" developed in social anthropology by Clifford Geertz and to attempts in contemporary sociology (from Max Weber to Peter Berger) to connect consciousness or ideological structures to economic and social practice, whereby causal relations are underplayed to the advantage of multifactored or circular explanatory schemes, especially those related to the collective self-perception of groups and the meanings they conferred on their behaviour.

A substantial introductory chapter focuses on structural features of Polish feudalism, marked between 1500 and 1700 by the "second serfdom," the Reformation, and the vigorous Counter-Reformation. The stress here is laid on the ideological interpretation of backwardness as demonstrated in the "Sarmatian myth," a fundamental element in the gentry's retrograde nationalism, in the renewed strength of inbred Catholicism, as well as in the social construction of the generalized "lack of order" in the body politic: the latter implicated in the decline of royal authority to impose reforms, the decision-making impotence of the gentry, the various degrees of political incapacity of burghers, peasantry, and Jewry, and so forth. If the second serfdom somewhat boosted Polish grain exports (at very high human costs) and—ironically enough—contributed to urbanization and economic modernization in the West, it also condemned the local economy to inefficiency and underdevelopment in the long run.

In this consolidated feudal setup Jews themselves proved unable to play the role of economic modernizers, unlike their coreligionists in the West. The second chapter analyzes the consequences of the incorporation of Jewry into feudal households as lessees, innkeepers, or involuntary moneylenders and the conflict situation it elicited between gentry, burghers, and peasantry. The central piece of the book is dedicated to a thoughtful, abundantly documented and cautiously balanced analysis of the "cultural sources of entrepreneurship." Far from the "Protestant ethic" theory or from classical statements on "capitalism and the Jewish spirit" (as found in Max Weber, Sombart, or Simmel), the author takes pains to identify some of the intellectual assets or liabilities which could serve as cognitive bases of or hindrances to entrepreneurialism in the Polish Enlightenment where the "rhetoric of reform" was amply exploited by publicists with Western education. He succeeds in tracing anticommercial conduct back to scholastic realism (objectivated also in "monastery realism") and to discourses of the Counter-Reformation. On the contrary, philosophical nominalism appears to have prepared the field for innovative capitalism via "fiscal nominalism," "covenantal causality," "the intensive use of credit and fully transferable instruments of exchange," and the "positive evaluation of trade," as it came to be expressed later in mercantilism. In Poland negative attitudes toward commerce, notably those mediated by the Jesuits, contributed to the country's separation from the West which was about to embark upon its industrial revolution. As for Jewish theology and intellectual tradition, closely scrutinized by Levin, in spite of early ambiguities they tend to secure cultural preconditions of enterprise. This must have helped to place Jews at an advantage in economic competition with Gentiles. Such dissimilar changes of Gentile and Jewish mental structures and economic conduct paved the way to both modern anti-Semitism and the persistence of Jewish alienation from modern Polish culture.

The Jewish tavernkeeper acted as an important link in the causal chain which was conducive to economic underdevelopment and its corollaries. On the eve of the last partition, the Polish gentry drew—more and more through Jewish lessees (with some 20%–30% of Polish Jews involved in the production of intoxicants)—as much as 40% of their income from their monopoly to sell alcohol primarily (and often forcibly) to their own serfs. The operation of Jewish taverns increased gentry revenues by the use of unmarketable grain surpluses. By the same token it unproductively siphoned off the surpluses of serfs to maintain the capacity of the gentry to import luxury goods, added to the overexploitation and demoralization of the peasantry (*dezolacia*), and maintained the Jews in their ambiguous position as the indispensable intermediaries regarded as responsible for economic backwardness. It also helped the gentry's rejection of modernization and the upkeep of its romanticized image of Poland's national past. Jews had to bear the brunt of both peasant rebellions and the moral ignominy of perpetuating the cycle of underdevelopment. No wonder that in this socioeconomic context the reform rhetoric and legislation, particularly as it was developed during the debates of the Four Year Sejm before the final partition, included no improvements of the Jewish condition in Poland. Formerly serfs of the Royal Chamber, then serfs in many (gentry) chambers, Jews implicitly became legal nonpersons in partitioned Poland.

Levin's analysis connecting the roots of anti-Jewish resentment with the failed modernization of Poland rests basically on subtly interpreted sources of intellectual history. It should read better when complemented with less general, more regionally based monographs dedicated to the social conditions of the belated economic take-off in Eastern Europe and its relevance to Jewish-Gentile relations. A description of the distinction between conflicting Jewish traditions, of their differential impact on local groups, and of the balance of power and the interest relations between Jews, gentry, burghers, central and regional authorities, and so forth would yield clues that would help answer the question: Why was the integration of Jewry doomed to historic failure in the Polish lands (as well as in Rumania and Russia), while elsewhere (in Hungary, Bohemia, and Vienna) it ultimately succeeded to various degrees?

The Heidegger Controversy: A Critical Reader. Edited by Richard Wolin. New York: Columbia University Press, 1991. Pp. 315. $35.00.

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Most critics agree that Richard Wagner's music has little to do with his political beliefs, that it should be judged on its merit, apart from the maestro's avowed anti-Semitism and the prominent place his music was accorded in Hitler's Germany. But what about Martin Heidegger's philosophy? How much weight should we give to his ill-fated involvement with National Socialism? Does existentialism spell fascism?

Richard Wolin has assembled a collection of materials bearing on these fascinating questions. In particular, he makes available to American readers postwar debates in Germany and an account of the controversy over Heidegger's legacy that stirred France in the 1980s after Victor Farias published his provocative study ***Heidegger and Nazism***. The author also offers a discussion of his own, where he skillfully navigates between the two extremes, one equating Heidegger's existentialism with

Nazism and the other exonerating his philosophy as inimical to the Nazi spirit.

The first issue that needs to be addressed concerns Heidegger's culpability for Nazi crimes. The case against the German philosopher can be summed up as follows. Heidegger greeted National Socialism as a "great awakening," heralding its role as a movement destined to reverse Europe's precipitous decline. He supported the *Gleichschaltung* legislation that barred Jews from the German Civil Service, distanced himself from his Jewish students during the Nazi years, and allowed the 1941 editions of *Being and Time* to appear without the original dedication to his non-Aryan teacher, Edmund Husserl. Declaring that the "Fuhrer alone *is* the present and future of German reality and its law," Heidegger spurned academic freedom as license to avoid responsibility and juxtaposed to it freely chosen commitment to National Socialism. In 1933, Heidegger accepted the rectorship at the University of Freiburg; in this position he tolerated, if not encouraged, the repression of liberal faculty members and made numerous speeches exhorting students to embrace fascist ideals. His lifelong association with prominent Nazi figures such as Eugene Fisher, the founder of the Institute for Racial Hygiene, also bespeaks Heidegger's identification with the Nazi cause.

These gruesome facts notwithstanding, the case against Heidegger is not cut-and-dried. Heidegger resigned his rectorship after 10 months in order, he later insisted, to show his displeasure with the Nazi pressure to replace undesirable deans with party faithfuls. Nor was he entirely unsympathetic to the plight of Jews. The dedication to Husserl appeared every time his magnum opus was reprinted, except in 1941, when Heidegger agreed to drop it (although not the crucial footnote acknowledging his debt to Husserl), and then only after he was led to believe that the new edition might otherwise be suppressed. As he pointed out in his 1966 interview for *Der Spiegel,* he had forbidden book burning on campus, declined to hail Hitler as German professors were expected to do in their classes, and from the mid-30s on offered a thinly disguised critique of Nazi racial philosophy. His dissenting views apparently irked the Nazi officials, who tried to block his publications, hindered his travel abroad, and finally drafted the aging professor along with other "dispensable faculty" into the *Volkssturm* in the waning days of World War II.

In spite of these mitigating circumstances, the Denazification Committee did the right thing when it barred Heidegger from teaching for five years (although he was given a pension that allowed him to continue his philosophical work). The most damning evidence against Heidegger is not what he did during the Nazi years, but what he did not do after the war. He never openly condemned the Nazi atrocities. Nor did he acknowledge that there was anything wrong with National Socialism as an idea, whose "inner truth and greatness" he praised as late as 1952. National Socialism, Heidegger reasoned, simply failed to live up to its true potential; it was thwarted by rabid racists who blocked the nobler, spiritual course charted by Heidegger in his rectoral address. Heidegger's moral instincts failed him completely when he equated Hitler's Germany with Roosevelt's America. Each side sinned against truth and practiced terror, he lamented; both succumbed to modernity with its relentless drive to rationalize the world, to subjugate nature, to mechanize human existence.

I found Heidegger's philippics against pragmatism, which Heidegger equated with crass positivism, particularly revealing. Along with many of his contemporaries, Heidegger dismissed pragmatism as "the most barren Americanism, according to whose fundamental principle that is true which succeeds." This sentiment is ironic, for many insights central to Heidegger's project—radical historicism, ontological uncertainty, political commitment, the need to balance public order with individual self-expression—are clearly adumbrated by American pragmatists. James, Dewey, and Mead would develop similar themes, but to what a profoundly different effect—which brings us back to the elective affinity between existentialism and fascism.

To the extent that the question of meaning boils down to the question of means, Heidegger's teaching has to be judged a sinister affair, for the means he chose to fill his philosophical abstractions with concrete historical sense were ugly indeed. Then there is his programmatic commitment to irrationalism, antihumanism, and antimodernism that does in retrospect seem to dovetail with certain Nazi practices. Still, I do not believe that Heidegger's existentialism, to say nothing of the existentialism championed by Kierkegaard and Sartre, is inherently conservative and antidemocratic. Too multivocal to lend itself to an unambiguous political rendering, it does have a humanistic potential that was revealed by American pragmatists who whittled away at some of the same sources that nourished existentialists. There are many factors that help account for the democratic route pragmatists chose to travel, including their tolerance, compassion, and ambivalence—the very humanism that Heidegger spurned as flabby nihilism and that he was determined to transcend through his hard-boiled "will-to-power." A formidable intellect, he proved deficient in intelligence—that practical faculty which balances our capacity to reason with our ability to feel, to empathize.

Richard Wolin has assembled an important volume that should attract not only scholars interested in Heidegger but also a wide range of practicing sociologists. Students of the Nazi era, sociologists of knowledge, scholars involved in the nascent trans-Atlantic dialogue, even ethnomethodologists interested in glossing over techniques and strategic accounting will find this engrossing, rewarding reading. I highly recommend this book to the sociological community.

Making Iron and Steel: Independent Mills in Pittsburgh, 1820–1920. By John N. Ingham. Columbus: Ohio State University Press, 1991. Pp. xi+297. $45.00.

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In *Making Iron and Steel* John Ingham sets out to correct the usual picture which interested parties carry of the steel industry in Pittsburgh around the turn of the century. Instead of the figure of Andrew Carnegie dominating newly formed and gigantic U.S. Steel, he makes the case that those companies which were not swallowed up in mergers, though individually small in comparison to the giant, were still numerous and powerful enough to belong in the foreground of that picture. Their strength rested on the facts that in 1901 their collective productive capacity was larger than that commanded by Carnegie's U.S. Steel, that many of them could be traced back to antebellum beginnings in the iron industry, and that most of them had limited themselves to differentiated specialty steels where they were shielded from competition with Carnegie and his enormous output of rails and beams for the growing railroad and construction industries.

Furthermore, the owners of these remaining independent mills were dominant in another way that Carnegie was not—together with the leading merchants and bankers they had composed most of the core of Pittsburgh's upper class since before the Civil War, and their descendants continued to enjoy that social supremacy well into the 20th century. The further facts that most were Presbyterians or Episcopalians, of British descent, born into well-to-do families, members of prestigious social clubs, prone to building very large houses close to each other, and partial to endogamous marriages, lends support to the idea that this was a close-knit upper class, important enough to overshadow Carnegie (who of course was never accepted into their circle) in the pictorial representation which the author hopes to revise with this book.

He includes a clear explanation of the technological changes which expanded the iron industry into the iron and steel industry, an account of some of the decisions and strategies which strengthened or weakened individual owners through this period of change and growth, a brief history of the sometimes benign but often antagonistic labor-management relations, and an overview of some of the political, social, cultural, and philanthropic activities in which these iron and steel magnates were "masters of all they surveyed."

He does not say very much about the nature and sources of his data and the methods he used to draw conclusions from them because he has already done this for the same data in his earlier book, *The Iron Barons: A Social Analysis of an American Urban Elite, 1874–1965* (Westport, Conn.: Greenwood Press, 1978). John Leggett provided a description and an appreciation of Ingham's methods of collecting and using data in his review of *The Iron Barons* (see *American Journal of Sociology* 85:976–78) in 1980. In the book under review Ingham helps the reader by putting time-ordered data into his tables so that one can follow decade-by-decade changes in the lists of rolling mills and steel works, in the companies' technologies and productive capacities, and in the social class locations of their owners.

The author's two books belong together. Anyone contemplating reading either might consider both and should probably begin with *The Iron Barons*.

Engineering Culture: Control and Commitment in a High-Tech Corporation. By Gideon Kunda. Philadelphia: Temple University Press, 1992. Pp. 297. $34.95.

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Engineering Culture is a welcome attempt to subject one of the buzzwords of the contemporary business world—"corporate culture"—to empirical scrutiny and analysis. Kunda disagrees with those who see the recent interest in corporate culture as evidence of a sincere desire for democracy and worker participation on the part of management, and portrays it instead as a form of normative control over the emotions, attitudes, and behavior of subordinates. Much of his description of the social dynamics of corporate life will be familiar to readers of *The Organization Man* and *Moral Mazes*. However, Kunda also demystifies recent corporate rhetoric and provides an interesting analysis of the ambiguity of individuals' commitment to the organizations which employ them.

The empirical basis of this analysis is an ethnography of the engineering division of High Technologies Corporation ("Tech"). Kunda describes Tech as a large, international corporation dominated by an engineering perspective; while tensions between engineers and managers may be growing, "the day of the MBA has not yet fully arrived" (p. 28).

Tech's employees describe it as a nonauthoritarian, informal, flexible work environment that rewards individual commitment, initiative, and creativity. However, Kunda sees in this an effort to prescribe a "member role." Tech managers have chosen to limit (though not to eliminate) the use of traditional bureaucratic rules and authority, relying instead on an elaborate and pervasive organizational ideology which emphasizes Tech's people-orientation and its desire to contribute to society. The object is to win commitment, to encourage employees to be "self-starters." Kunda provides a perceptive and subtle, if somewhat repetitious, analysis of the sources of this ideology (which range from top managers to internal experts to ostensibly objective "outside observers").

His discussion of employees' reactions is the most interesting part of the book. "Presentational rituals" (meetings with senior management, training sessions, work group meetings) and a variety of routine settings (informal interaction, office decorations) reveal that Tech employees' commitment to the member role is ambivalent. Management seeks to "engineer culture" in order to develop employees driven by intense emotions, strong beliefs, and authentic experiences of corporate loyalty. Employees frequently do embrace the prescribed member role; they affirm the ritualized expressions of corporate culture and consciously "talk the culture" in their informal interactions. Yet, Kunda finds that this is almost always tempered by simultaneous role distancing. Ritual occasions contain built-in breaks during which members question the reality of corporate claims; informal interaction and even office decorations are flavored by irony and cynicism about the member role. Since "unqualified role embracement . . . is felt by many to be undignified" (p. 177), members seek to construct an "organizational self" with the ability to balance role embracement and role distancing successfully, the ability to display and maintain an air of ambiguity. Kunda correctly notes that this calls conventional notions about organizational commitment into question; rather than being opposite points on a continuum, attachment and detachment can coexist.

Since employees are aware of the ideological character of corporate culture, why do they embrace the member role at all? At times, Kunda suggests that corporate culture undermines the possibility of authentic experience outside the member role, condemning members to an inherently self-defeating quest for stable meanings in "the race to meet corporate standards of accomplishment" (p. 222). This argument is less than persuasive, however. What keeps them from seeking meaning in the nonmember part of their selves—quitting, detaching from organizational goals, and so forth?

Since Tech's employees do not appear to be doing so, they must be getting something that they really want. This argument is more consistent with what sociologists already know about the socialization and aspirations of managers and engineers. Robert Jackall's *Moral Mazes* emphasizes that managers come to organizations with strong desires for upward mobility. In the context of ambiguous organizations such as Tech, where the route to the top is not clearly spelled out, team playing and organizational loyalty becomes a means of sustaining a successful career. Similarly, many studies have found that engineers often are motivated primarily by a desire to do "cutting edge" technical work. As long as organizations "deliver the technical goods," which Tech clearly does, engineers are willing to accept a variety of organizational constraints. Kunda acknowledges that such trade-offs help to explain Tech employees' embracement of the prescribed member role (pp. 174–77). But he does not focus clearly enough on corporations' ability to satisfy the aspirations that managers and professionals bring to the workplace from outside the corporate culture.

Case studies such as this one inevitably leave unanswered many questions about the engineering of corporate culture. Are organizations that are struggling financially able to sustain the claims that underpin the member role? When conventional bureaucratic organizations attempt to become flexible organizations such as Tech, do the participatory, quasi-democratic elements involved in this type of management provoke resistance from middle managers concerned to protect a degree of bureaucratic power? Finally, is corporate culture undermined by a stronger sense of managers as "product preventers" in organizations which are not dominated by an engineering perspective? These questions simply indicate that *Engineering Culture* is not the final word on corporate culture. A more comprehensive, synthetic work will have to await further studies to supplement Kunda's insightful book.

The Descent of Icarus: Science and the Transformation of Contemporary Democracy. By Yaron Ezrahi. Cambridge, Mass.: Harvard University Press, 1990. Pp. xii + 354. $35.00.

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Discussions of the role of science in the modern state have affirmed alternately a benign rationalizing effect of science on state intervention, consistent with a technocratic ideology, and a mystifying use of science to cloak political objectives with a seemingly universalist rationality. In *The Descent of Icarus,* Yaron Ezrahi seeks to transcend this apparent antinomy by considering science as a political *and* ideological resource for resolving the tension within liberal democracies between individual, voluntarist values and the necessity of public action. In a historical account tracing parallel developments in science, philosophy, literature, and art, and with a focus on the United States, Ezrahi charts the rise and recent fall of this kind of appropriation of science and the instrumental paradigm of action it contains.

Science was deployed by states not for the substantive recommendations it produced but for its embodiment of an instrumental paradigm of action, a way of formulating and justifying actions in terms of their consequences. This instrumentalism was institutionalized in both science and the state through the construction of what Ezrahi calls an "attestive visual culture." In 17th-century natural science, Newton, Boyle, and their colleagues at the Royal Society constructed a space of visual attestation of scientific experiments as the proper context for producing facts, as opposed to the private, unaccountable settings of mysticism and speculative philosophy. Later Priestley and Faraday would elaborate the notion of science as knowledge "produced and confirmed through public witnessing." By the 18th century both the ethical and institutional elements of the attestive visual culture were transferred to politics, with states legitimating their actions by opening up a space in which actions were visible to public scrutiny. Founders of the United States, including

Paine and Jefferson, explicitly argued for subjecting public actions to the same criteria and for public attestation of state actions.

Though the use of science to confront the liberal-democratic problem of action is a general concern, Ezrahi identifies several distinct ways this has been accomplished. First, the transfer of naturalist thinking from Newtonian mechanics to classical political economy, and its latter-day variants in Austria and Chicago, allowed for the limitation of state action justified by the identification of a spontaneous public order. Second, pragmatists and social reformers evoked science as a way of arriving at an informed and rational consensus free of coercion. And finally, in the case of the postwar welfare state, science was used as a substitute for appeals to the public, to justify centralized action in terms of universalist goals.

In this final guise, according to Ezrahi, science as a political resource and guiding metaphor for state intervention has also seen its demise. In a very interesting discussion he links the privatization of science, referring to its retreat from public forums and auspices, with a waning confidence in the possibility of rationally directed social intervention. Science, losing its attraction as a resource for formulating and judging state actions, has been supplanted by theater, by the conception of public actions as meaningful gestures in themselves, rather than as instrumentally directed toward future return. Ironically, Ezrahi believes that this generally maligned merging of statecraft with stagecraft may eventually prove to be, like science, an effective device for realizing liberal-democratic values.

Though Ezrahi considers science and technology as ideology, he means something very different from critical theorists who give ideology an antidemocratic, mystifying function. Though deployed as an ideological resource, as a way of justifying public action in a liberal democracy, science exerts a force of its own on the formulation of public actions, by subjecting this formulation to the exacting criteria of scientific validity. Science is a relatively benign ideology, one that facilitates the realization of collective goals and fosters accountability of public officials. It safeguards the public interest by allowing public actions to be alienated from the particular personalities, interests, and shortcomings of the actors. Its efficiency is therefore political rather than technical or technocratic.

Ezrahi thus challenges the technocratic conception by affirming the ideological role of science, while also challenging critical theory by suggesting that this ideology is relatively benign. Yet it is on this most contentious point that the book is potentially weakest. The "liberal-democratic problem of action," to which science and instrumental rationality respond, is conceived in the abstract, as a functional requisite at the level of social institutions. Had Ezrahi conducted his analysis at the level of practices of state personnel, one wonders if his view of science and technology as a machine for reconciling a mass polity with centralized public action would have been confirmed, or if, as Jürgen Habermas suggests, states have increasingly used science to shield social intervention from processes of collective will-formation, rather than as a way of realizing the public interest. This problem is compounded by the strict reliance on sources in social theory, philosophy, and literature, with a corresponding lack of direct evidence from the history of state intervention and the actions of state personnel.

Despite these reservations, this book deserves the attention of sociologists interested in the development of modern states. It challenges us to consider the role of science in a new light, not simply as research findings and policy recommendations on one hand, nor as a mystifying ideology on the other, but as an institutionalized logic for formulating and justifying public actions.

Driving Ambitions: A Social Analysis of the American Hot Rod Enthusiasm. By H. F. Moorhouse. New York: Manchester University Press, 1991. Pp. 231.

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One has only to read the first few pages of *Driving Ambitions* to realize that its title belies the fundamental sociological problems that it addresses. H. F. Moorhouse begins with a set of cogent observations about the subjective as well as structural importance of the automobile in modern society, the underrated significance of intense personal enthusiasms in the analysis of modern consciousness, and the remarkable neglect of both phenomena by social scientists, except for their occasional use as object lessons by critical theorists whose analysis, the author argues, is often facile.

Starting from these disquieting premises, Moorhouse produces a detailed analysis of hot-rodding. It is an impressive synthesis of materials drawn from magazines, correspondence, club notes, manufacturers' private memos, and other obscure artifacts, marshalled to recount 50 years of organized enthusiasm. With a freewheeling style that makes reading fun, Moorhouse identifies a complex set of dynamics between dragstrip organizers, club leaders, and local police; hot-rodders and manufacturers; magazine readers (represented in published letters) and editors. Although the consciousness characterizing grassroots hot-rodding is graphically and authentically captured through these data, the study could have been strengthened and enriched by adding interviews with participants, especially since even many of the pioneers are still living and accessible. Those who write to magazines are not entirely representative of grassroots participants, and published letters do not represent a random subset of those received.

Moorhouse effectively shows that no one-dimensional theory of social reproduction can account for the complex political, technical, and economic struggles that characterized hot-rodding through its half-century of existence. He documents *Hot Rod* magazine's early ascendancy as the mouthpiece of the hobby, and its incestuous link to the National Hot Rod Association. Controlled by the same few individuals (and even based at the same address for several years), both sought to energize, define, legitimate, and profit from grassroots enthusiasm. This effort at control has not been without resistance, however, as other magazines and more democratic sanctioning organizations emerged and continue to flourish. The notion that such an enterprise could represent a linear economic determinism is repeatedly shown to be inadequate; for example, efforts to make drag racing a "big business" via professionalization were opposed by the magazines, since this would lead to expensive race cars and constricted access to dragstrips, resulting in a decline in amateur participation (and hence, publishers feared, magazine readership). Thus, a Fortune 500 publishing company was able to define itself as championing the little guy.

Provocative discussions of such issues lead the reader to hope for a synthetic conclusion that will tie these strands together into a new theoretical account of such activities. So, it is a considerable disappointment that no such account is offered. Moreover, Moorhouse does not succeed in disproving the critical theorists' interpretations: that the hot-rodder is authentically impassioned and intensely energized does not mean that he may not also be, in Adorno's terms, "a discoverer of just those industrial products which are interested in being discovered by him" (p. 13). Frenetic activity at the level of everyday experience often serves to reproduce passively the larger system in which it is located; indeed, Moorhouse later presents data from manufacturers documenting their deliberate interest in the marketing potentials of hot-rodding.

The book suffers from other notable shortcomings. Although Moorhouse is rightly concerned to establish the importance of personal pursuits, he naively rejects the idea that the worklife of the protagonists also merits analysis. In his treatment of the last two decades, the focus on drag racing seems especially limiting: while dragstrip participation has declined (at least partly in response to professionalization), Moorhouse all but ignores the subcultures of street-rodding and "cruising," which have continued to thrive. The editing is unusually poor (misspellings, inconsistent indexing, and a line missing at a critical point in the argument); the author's spritely style often snowballs into run-on sentences. (Moorhouse is an English sociologist who has no interest in cars and does not even drive, so it may be appropriate that the dust jacket shows a picture [transposed] of a 1947 Plymouth, one of the all-time least popular cars on which to base a hot rod project.)

Imperfections notwithstanding, the book is an impressive achievement. It is an important contribution to understanding specialized modern obsessions, demonstrating how they both invite and resist cooptation and how they are reshaped by the institutions to which they give rise. As major social loci of impassioned activity, such pursuits require analysis and Moorhouse demonstrates that their dynamics are more complex than simplistic theoretical treatments have implied. I hope others will join him in further disentangling that complexity, even as analyses of comparable enthusiasms add to it.

Inflation of Symbols: Loss of Values in American Culture. By Orrin E. Klapp. New Brunswick, N.J.: Transaction, 1991. Pp. 199. $24.95.

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The central theme of this book is that various social processes have served as social magnifiers to enlarge, expand, and/or create an oversupply of various social goods and in so doing have served to "inflate" these goods and, as a consequence, to eventually undermine their value. After a brief introductory chapter entitled "What Is Symbolic Inflation," Klapp describes his five basic types of social magnifiers: chapter 2 focuses on "Overstatement," chapter 3 on "Crusades," chapter 4 on "Contagious Communication," chapter 5 on "Emotional Hitchhiking," and chapters 6–10 on various forms of inflation.

In chapter 2, overstatements are broken down into subclasses: hyperbole, hype, redundancy, euphemism, jargon, and abuse. Assorted examples, taken primarily from the mass media, range from the promotion of Elvis Presley to the abuse of words such as *awesome* and *liberal.*

While overstatement embodies the notion of inflation directly, the link in some of the other chapters seems to be more tenuous. The central issue of chapters 3–5 is extremism rather than inflation per se, though Klapp tries to relate the two issues.

While I share Klapp's concern with extremism, I am unclear as to the advantages gained by treating it as a form of inflation. Connections can be made between most theoretical notions, but this one strikes me as somewhat strained. I am also uncomfortable with an analysis which fudges important political and ideological differences as reflected in such statements as "Extremist groups such as CORE, SNCC, the Black Muslims, and the New York Black Panther Party . . . " (p. 27). I have similar reservations about the way in which crusades are characterized—"(1) militancy, (2) righteousness, (3) sense of uphill struggle, (4) image of evil, (5) unwillingness to compromise, and (6) implied evangelism" (p. 39)—given the historical record of how the many groups characterized as engaging in crusades, especially the civil right groups, actually behaved. The basic message of chapter 3 and those on contagious communication and emotional hitchhiking appears to be that false highs are bound to be followed by disappointments. Klapp's thesis that the core problem is one of oversupply has a certain prima facie attractiveness, but it really does not shed any new light on the process.

In chapters 6–10, Klapp attempts to utilize the concept of inflation in a more traditional manner, namely one in which an oversupply of some item serves to decrease its value. While Klapp begins with a brief account of the economic view of inflation, most of the text of these chapters presents a range of examples in which an increase in the supply of various social symbolic goods—standing applause, SAT scores, smiles, or university degrees—serves to undermine their value over time. While the reasons for an increase in supply vary from item to item, there is a sense in which Klapp feels that the underlying cause is a general tendency toward democratization and a consequent lowering of standards, though I do not know if he would accept this characterization. This is illustrated by his list of the seven factors which serve to undermine credentials: massification, elasticity of supply, slippage of academic and professional standards, faddism, counterfeiting, credential radicalism, and egalitarianism.

While I find the general thesis of this book intriguing, I fear that the analysis remains too superficial and ideological. Having spent a good deal of time studying a range of markets dealing in goods with both symbolic and economic value, I have been continually impressed by the way participants manage to reclassify goods to avoid the types of oversupply and subsequent inflation that Klapp assumes is inevitable. It is often the case that an increase in supply is made possible *because* of an item's loss of value. Often another item has replaced the original item as the symbolically valued item.

By focusing on the "loss of values in American culture" and by exploring the ways in which economic paradigms can be applied to sociological concerns, Klapp has fixed his attention on two important issues. I would suggest, however, that each of these issues, let alone their interaction, is significantly more complex than this book would indicate. Despite my misgivings, I must admit to being captivated by the book's central idea, which is much more than I can say about many books I have read. This leaves me somewhat confused. On the one hand, I feel that there is less substance here than I had expected. A case of symbolic inflation? On the other hand, I clearly obtained something of value, which leads me to wonder if, when it comes to symbolic values, perhaps inflation does not always entail loss.

Ritual Theory, Ritual Practice. By Catherine Bell. New York: Oxford University Press, 1992. Pp. x+270. $15.95.

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Catherine Bell's *Ritual Theory, Ritual Practice* is a work which seeks to redirect the study and conceptualization of ritual. As the author correctly points out, it is a work of metatheory very much in the spirit of Edmund

Leach's *Rethinking Anthropology* (London: Athlone Press, 1961) or Dan Sperber's *Rethinking Symbolism* (Cambridge: Cambridge University Press, 1974). The reevaluation of a familiar conceptual tool can only be welcome at a time when there is a renewed interest in the study of culture within sociology.

The argument of the book is divided into three main sections. The first of these contains a critique of existing treatments of ritual. At the core of the argument is the assertion that there is a common discourse underlying the study of ritual from Durkheim and Lévi-Strauss to Geertz, Victor Turner, Stanley Tambiah, and Robert Wuthnow. Bell argues that a set of universal dichotomies between theory and practice, thought and action, belief and rite permeate the theoretical, empirical, and textual "construction" of ritual. Drawing on Fredric Jameson's well-known study of Saussure in *The Prison House of Language* (Princeton, N.J.: Princeton University Press, 1972), Bell asserts that these largely unconscious textual and discursive practices set up a self-reinforcing circularity between subject, object, and methodology in the study of ritual. Ritual is defined as the enactment or dramatization of fundamental beliefs or symbolic codes and the methodology of analysis must uncover these hidden wellsprings. This linguistic practice has the underlying property—and herein lies the rub—of legitimating, privileging, and empowering the concerns of the theorist. In opening up certain avenues of enquiry and closing others this discourse provides only for the "domination of the theoretical subject" (p. 54).

In the remaining two sections of the book Bell offers a number of suggestions for reorienting ritual studies in a more critical direction. By elaborating a "practice theory" drawn from the work of Marx, Bourdieu, and Althusser, she attempts to by-pass the pitfalls that she considers have ensnared earlier theorists of ritual. Central to this actor-centered approach to ritual is the notion of "ritualization." Ritualization is situational and strategic action which aims to create ritual contexts and behaviors for purposes of social control and domination. In undertaking strategies of ritualization actors will typically attempt to contrast their actions with everyday, routine activity by means of repetition, formality, and fixity in patterned spatial and temporal environments. Drawing on the work of Foucault and others, Bell argues that ritualization enhances domination by operating on the human body and structuring the movements of the body. From this point she goes on to make the interesting and radical argument that "ritual activity is not the instrument of more basic purposes such as power, politics, or social control which exist before or outside of the rite" but rather that "ritual activities are themselves the very production and negotiation of power relations" (p. 196).

Cultural Theory, Cultural Practice throws down the gauntlet to cultural interpretations of ritual. It is a challenging and somewhat difficult book which repays close reading. Bell's erudition and command of the literature, especially in the field of anthropology, is most impressive. Her appeal for the centrality of dynamic individuals, strategies, and power relationships is powerful and will no doubt strike a chord among those similarly disaffected with prevailing trends in understanding ritual. The concept of ritualization is particularly useful and will no doubt become common coin.

On the negative side, it is not clear that her attack on the more hermeneutical perspective, and the alternative she offers, is completely successful. Bell argues that the dualisms dominant in the representation of ritual (thought/action, belief/rite, etc.) are the product of the unconscious conflation of levels of analysis and the treacherous slippage of language. The same data that she uses to demonstrate this point could easily be read another way. Namely, that "cultural" theorists of ritual are driven to define their objects and methodologies according to conscious, coherent, and internally consistent theoretical choices and logics. Moreover, her alternative approach to ritual replicates some of the flaws that she detects in the dominant discourse. First, although Bell attacks existing writing as being "encumbered by assumptions about thinking and acting" (p. 4), the alternative she offers seems to be similarly constrained by an a priori commitment to a relentlessly instrumental conception of social action. Second, while she attacks the dominant paradigm as being one in which "the concerns of the theorist take center stage" (p. 54), it is not at all clear that her revision does not similarly privilege the theoretical knowledge, competence, and interests of an analyst looking on with the ironic perspective of critical theory. Nowhere is this paradox clearer than in her discussion, drawn from Althusser, of the "blindness" of the actor involved in ritualization to the true function of ritual in maintaining the hegemonic social order. One is tempted to suggest that this contradiction can perhaps only be resolved with a movement from Marxian praxis theory to an ethnomethodological concern with situated and practical actions. In sum, although *Ritual Theory, Ritual Practice* does not perhaps deliver all that it promises, it is a useful counterweight and can be recommended as such.

European Cinemas, European Societies, 1939–1990. By Pierre Sorlin. New York: Routledge, 1991. Pp. 247. $59.95 (cloth); $18.95 (paper).

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Sorlin examines European films because they can "enlighten us about the building up of an image" (p. 47). His subject is films from Germany, England, Italy, and France from the years 1939–1990. Sorlin identifies similarities and contrasts between films in these countries, while omitting films from other European nations, because they constitute three-quarters of the total European film production over this period. Sorlin begins with the perspective that cinematic images are to be studied "in association with each other and in close relationship with the evolution of Europe during the past half-century" (p. 5). However, he does not end there. Sorlin considers cinema in relation to its commercial market.

Sorlin's in-depth analysis of the image making of three themes in European cinema—resistance, city life, and women—illustrates that films are best seen not as echoes of political life or reflections of social reality, but as "industrially produced" images to be "sold to audiences which buy them for their pleasure" (p. 9). Cinematic images are "independent of the present" (p. 12) although they may borrow from social reality. They represent only fictional constructions that are "artificially reproduced" (p. 12). This is so, Sorlin explains, because the world of cinematography constitutes a small community whose primary reference is cinema, not the society at large. In this view, "cinema has a timing of its own, a studio-timing partially independent from the social evolution" (p. 213).

Sorlin's interpretative view that incorporates the influencing role of the production market and cinema's attending or absent spectator allows us to find strikingly similar representations of social life in the three themes he examines. These representations correspond more to the image-making concept embedded in cinematography than to the social, economic, political, or cultural nuances of each country. From the outset, Sorlin alerts the reader that although he does not deal with Hollywood directly, it is "economically, technically, fictionally . . . the context of film-selling" (p. 10). Thus, in this conception of European cinema we see the films as belonging more to an image-making process intertwined with a small cinematography market than to their individual countries.

Sorlin examines European cinema's image making of resistance to Nazism and finds that, although it was a popular subject with audiences, European films reluctantly addressed it as a theme. Furthermore, regardless of "the diversity of their situations . . . the four countries succeeded in sharing a common depiction of the secret war" (p. 78). A mythic image of resistance was created on the big screen and to a large extent popularized "the memory of resistance in the Old World" (p. 79).

In his examination of city life, Sorlin argues, "the depiction of a town in films is the consequence of a deliberate choice" (p. 112). Although "obvious correspondences . . . between the films and their historical context" can be found, the author cautions against looking at films from this direction because it reveals very little about human interaction, sentiment, and experience. Instead, Sorlin examines the representation of social issues—family life, unemployment, love, and so forth—that is depicted in the midst of city life. City life is important to his analysis because it symbolizes "human landscapes likely to enclose and sometimes to influence the fate of individual characters" (pp. 134–35).

Sorlin examines the representation of women in European film and finds that until feminist critics entered film studies, semiotics and psychoanalytic methods were predominately those used to analyze the few female protagonists in European cinema. The ideal cinematic female character was a construction of the male gaze and was "unpredictable, whimsical, unreliable and extremely pretty" (p. 201). Sorlin finds that the female representation in films after the 1960s presented images of European women that did not correspond to their social reality but who embodied "a sexual equality the was far from being established at the time" (p. 202).

Sorlin concludes his richly interpretive focus on these three themes in European cinema by arguing against reflection theory precisely because "cinema is not society-proof but it filtrates and modifies what is observed in the street according to the modes of cinematic circles" (p. 212). The representation of resistance remained "a marginal subject for the European cinema" (p. 214); the depiction of city life "provide[d] only deceptive information about . . . what a town is," ultimately depicting towns as "unlimited areas or conglomerations of dwelling places" (p. 215); the representation of women remained a "fictional and visual treatment . . . [that] maintained a strict division between genders" and captured a "predominantly masculine point of view" (p. 215). According to Sorlin, although they are from different cultural, economic, and political nations, European films have much in common because of the intertwined relationship of cinematic production to Hollywood and European cinematography. Finally, films provide sociologists with insight into "vehicles [that] appeal to the imagination" and thus into "people's self definition" (p. 213).

Native and Newcomer: Making and Remaking a Japanese City. By Jennifer Robertson. Berkeley and Los Angeles: University of California Press, 1991. Pp. xvii+235. $29.95.

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Holy Cross College

Jennifer Robertson, a cultural anthropologist, has written an engaging study on the symbolic politics of place making. The ostensible subject of her ethnography is Kodaira, Japan, a suburban city of 150,000 in the Tokyo metropolitan area. Yet, her concern with Kodaira lies less with the community than with the ways that Kodaira citizens conceive of their community. Specifically, Robertson explores how Kodaira leaders and residents cultivate and appropriate traditional, rural images of community life to legitimate their suburban city as authentically Japanese. Through her analysis, Robertson proposes that city making, as a symbolic process, is driven by social conflict between Kodaira natives and newcomers.

Robertson's approach to this study of symbolic action is ethnographic, but it is ethnography of a special kind. As she remarks, she wants to offer "a literary portrait of Kodaira," one that examines the dominant trope that contemporary Japanese use to discuss, debate, and ultimately envision community life (p. 4). That trope, *furusato,* literally means "old village," but she suggests that it might better be translated as "home" or "native place." Like the idealized image of the small town in American culture, *furusato* is nostalgic. Its symbolic connotations reach back to a sentimental rural past, unspoiled by the modern urban present. Like the idealized American small town, *furusato* is also a popular utopia, a symbolic locale that can be used to imagine a better future (cf. J. Gusfield's *Community* [New York: Harper & Row, 1975]).

Robertson's interpretation of place making in Kodaira thus unfolds as a series of "readings" in which she analyzes the many ways that *furusato* informs public discourse and ritual about community identity. In chapters 1 and 2, she examines how *furusato* emerges in the 1970s as a significant cultural symbol in national and local political culture, and she documents how local political leaders actively engage in *furusato* making. Kodaira city hall, hoping to renew the spirit of the "old village" in the new city, supports historical restoration, living history, and the creation of an annual citizens' festival. Such cultural politics are directed toward the newcomers of Kodaira, the great majority of residents who are seen as lacking a sense of community and attachment to place.

In chapter 3, Robertson turns to historical analysis to document how recent local histories of Kodaira involve an active reconstruction of the city's past, providing a usable community identity for contemporary residents. In chapters 4 and 5, she analyzes religious and neighborhood associations, showing how local groups appropriate *furusato* to build communal solidarity in an ongoing process of community change and conflict. These later chapters, in particular, document the critical, and often contradictory, role of Kodaira natives in place making. Although these descendents of the 17th-century rural village constitute only 1% of the modern city, they are an essential voice in public debate over the meaning of Kodaira. On the one hand, natives—and their neighborhood landscape of farms, Shinto shrines, Buddhist temples, yard shrines, and festivals—authenticate Kodaira's claims to *furusato* as a community. On the other hand, natives cultivate their symbolic ties to *furusato* as a way of redressing their loss of status within a city dominated by newcomers. Thus, the restoration and celebration of "old village" ways also become a means of claiming privilege within Kodaira. Such cultural revanche by natives engenders conflict within Kodaira, separating natives from newcomers in associational memberships and other community activity.

Robertson documents both the social construction and use of a significant cultural metaphor and the power of that metaphor to shape human action and understanding in the local community. Her argument is well documented. She draws extensively on two years of fieldwork in Kodaira, and she makes effective use of observations of community ritual and the built environment, of informant interviews, and of documentary analysis of contemporary and historical texts. If the work has a significant flaw, it is perhaps that of the limits of disciplinary specialization. Although Robertson draws on some scholarship outside anthropology (for instance, Gerald Suttles), she makes relatively little use of a growing, multidisciplinary literature in sociology, cultural geography, architecture, and literary studies that addresses the symbolic dimensions of community identity and place. Nevertheless, *Native and Newcomer* is a strong and provocative work that should have significant appeal to sociologists, not only to those concerned with contemporary Japanese society but also to those interested in popular culture and ideology, urban and community sociology, and ethnographic approaches to cultural studies.

La disqualification sociale: Essai sur la nouvelle pauvreté. By Serge Paugam. Paris: Presses Universitaires de France, 1991. Pp. 254.

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Part of a small but growing European field of "poverty studies" that investigate new forms of social exclusion and marginalization, *La disqualification sociale* is an interpretive sociology of poverty in a midsized provincial city in western France. It uses data from the administrative files of municipal social agencies (covering about 1,200 recipients), in-depth interviews with 70 residents of a stigmatized "*cité*" (public housing project), and field observations to explore how social actors experience and actively negotiate the negative identity and condition of being "poor."

Drawing selectively on Simmel, as well as on Goffman's analysis of the management of stigma and labeling theory, Paugam first replaces the ambiguous, commonsensical notion of "poverty" by the analytical construct of the "recipient" (*assisté*), characterizing the poor relationally as "the set of persons whose social status is defined in part by the specialized institutions of social assistance which designate them as such" (p. 24). This leads him to distinguish three gradational categories: the "vulnerable" (*fragiles*) are the object of intermittent social support and resist being drawn into routine relations with welfare agencies, the "supported" (*assistés*) are regular beneficiaries of long-term aid who embark on a moral career culminating in the reframing of recipiency as a quasi occupation, the "marginals" (*marginaux*) fall through the various safety nets of state and city, by design or defect, and develop their own independent coping strategies.

The first half of the book analyzes how these three categories of the poor differentially interpret and manage their status. For the vulnerable (half of whom are employed), wage-labor remains the anchor of self-conception and welfare is seen as a temporary prop that they use either as a last resort or rationally. Those experiencing internalized vulnerability view public aid as a conspicuous sign of personal failure and despair of living in a degraded neighborhood with other welfare families. Their relations with social service providers are diffident and exacerbate their feeling of humiliation, so that they frequently refuse assistance even if they are entitled to it or until their situation becomes desperate. A younger group, with slightly better education, job skills, and self-confidence, experiences negotiated vulnerability, where assistance is sought in the manner of a consumer good to help (re)gain a foothold in the labor market. Regular recipients move from "postponed" to "installed" to "claimed" assistance, as their motivation to work decreases and dependency on social services increases. They learn to rationalize (in the twofold sense of economics and psychoanalysis) their reliance on aid and progressively take over the personality traits associated with their status. Their relations with social services providers evolve from distancing to appropriation and seduction to a conflictual tug-of-war characterized by distrust and manipulation. The marginals, having neither steady income nor social assistance, survive on the margins of the labor market and public insurance. Their marginality may be conjured (in the case of those who ineffectually struggle to re-enter the dominant institutions of society) or organized, when they work to construct a livable cultural frame within their given material constraints.

The second half of the book tackles the dynamics of identity construction and social relations within the town's stigmatized *cité,* a small housing project comprising 60 housing units where dependent households are concentrated. The neighborhood—nicknamed "Little Chicago," as are most such *cités* in France—is seen by residents and outsiders alike as as "wart" on the urban landscape. The physical degradation of buildings, the rapid turnover of families, and the inordinately high proportion of long-term welfare recipients in it all contribute to mark it as a place to shun. This stigma is perpetuated by the heterogeneity of the inhabitants and their "derogatory gossip," and further reinforced by the intensive intervention of social agencies which accentuates collective resentment and shame. To gain a measure of dignity, residents overstress their moral worth as individuals or highlight their achievements as parents; they also denounce the "faked poor" (those who undeservingly "profit" from welfare programs) in an attempt to reaffirm the legitimacy of their own status. Strategies of social distinction take three forms which all undermine local solidarity: mutual avoidance, reconstitution and elaboration of "infra-differences," and the diversion of public opprobium onto scapegoats such as notorious "problem families," foreigners, or single mothers. In short, residents of the *cité* form an impossible community, perpetually divided against itself, which cannot but refuse to acknowledge the collective nature of its fate.

Paugam's data are rich and well presented but somewhat undertheorized: notions of norms, roles, and "personality" are invoked rather loosely and no firm causal mechanism is offered to explain the striking correspondence demonstrated between the subjective experience and objective position of the various poor. One may also regret the one-sidedness of the portrait of welfare due to omission of the viewpoints and (counter)strategies of social workers. These criticisms notwithstanding, *La disqualification* is of considerable interest to American readers. It provides findings on the lifeworld and practices of a disadvantaged population that have direct parallels or relevance to the U.S. debate on poverty—for example, the centrality of employment for the formation of a "life project" which in turn determines a range of strategies, from welfare use to marriage and fertility. By focusing on the social construction of poverty, it avoids the sterile opposition of culture and structure and highlights the role of state institutions in "prefabricating" the phenomenon. But *La disqualification* is perhaps most instructive for its silences. Its conclusion links the emergence of "new poverty" to the ongoing economic crisis and to society-wide transformations of family models—no talk here of a special culture, a new group (a French "underclass"), or excessively generous welfare programs. On the contrary, Paugam endorses experimentation with forms of social assistance that seek to act upon the social milieu rather than on individuals and he highlights French local policies of "social solidarity" that have developed in tandem with the recently instituted national guaranteed minimum income. On both sides of the Atlantic, to fight postindustrial poverty clearly requires extending social rights and reaffirming the commitment of the state to the provision of full, active citizenship for all.

Lone Parenthood: An Economic Analysis. By John F. Ermisch. New York: Cambridge University Press, 1991. Pp. xv+194. $54.50.

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Rapid demographic and family change is ubiquitous in industrialized countries. In the past three decades, the number of one-parent families tripled in the United States and the British figure doubled. Parallel to many studies on single mothers in the United States and their poverty, welfare dependency, and employment, *Lone Parenthood: An Economic Analysis* by John F. Ermisch presents a systematic and comprehensive study of their British counterparts. This book provides U.S. readers with a useful source for comparison and welfare policy implications.

The author uses an economic approach to examine decision making regarding divorce, remarriage, childbearing outside marriage, and marriage of never-married single mothers in order to explore the entry into and exit from, as well as the duration of lone parenthood. He also uses an economic approach to the employment dynamics of divorced mothers in order to understand the determinants of entry into and exit from employment which is crucial for one-parent families to enhance living standards without welfare dependency. As the author points out, "the economic analysis . . . does not presume that economic goals are paramount in . . . decisions, nor does it assume that traditional economic variables dominate . . . decisions" (p. 41); he emphasizes the importance of the combination of various socioeconomic characteristics of women and welfare benefits in influencing decision making.

Ermisch begins, in chapter 2, by describing British demographic trends in comparison with other industrialized countries, focusing on both the similarities and differences in the growth of one-parent families. This is followed in chapter 3 by a formal demographic analysis of the dynamics of British lone parenthood with a simple Markov model. Using data from the 1980 *Women and Employment Survey (WES),* four transitions—marital dissolution, remarriage, illegitimate birth, and marriage of never-married mothers—were identified as important in the dynamics of lone parenthood.

The author then formally models the decision making involved in these transitions in chapter 4. After reviewing the economic theories of marriage and marital search, the author generates a series of testable hypotheses about the effect of related factors on the "gains" (or lack of gains) from a marriage, for example, the destabilizing effect of alternative gains outside marriage such as welfare benefits available to one-parent families.

In chapter 5, Ermisch empirically tests the hypotheses in the two transitions into lone parenthood: marital dissolution and illegitimate first birth. Drawing on the *WES,* the analysis shows that although higher welfare benefits do not encourage marital dissolution, they do significantly and moderately increase the likelihood that a woman has a child outside marriage.

In chapters 6 and 7, the author turns to examine lone mothers' employment, one of the most important decisions to cope with their poor economic circumstances. An economic framework of labor supply serves as a guideline for hypothesis testing about the effects on employment of potential wage, welfare benefits, and other nonlabor/nonwelfare income, with focus on the effect of taxability of welfare benefits and other income. Two types of survey, the *General Household Surveys* (*GHS* 1973–82) and *WES,* were used for static and dynamic analyses, respectively. The analyses largely confirmed the discouraging effect of welfare benefits due to their taxability and the encouraging effect of nonlabor/nonwelfare income for eligible divorced mothers because it was not taxable.

Chapter 8 provides an analysis of exit from lone parenthood based on the theoretical layout in chapter 4. It shows that welfare benefits do not directly prolong the length of lone parenthood. The author concludes the book by further examining the effect of various policies on lone mothers' employment.

The author did an excellent job in this comprehensive study of lone parenthood in Britain. To mention a few of many strengths of this study, I am particularly impressed by the author's effort to understand the dynamics of lone parenthood and the dynamics of lone mothers' employment with welfare benefits being one of the key determinants. While the former has been investigated before in Britain and other comparable countries, the latter was the first attempt in Britain, and among a few in comparable countries. From the dynamic analysis of lone mothers' employment, the contrasting effects of welfare income and nonlabor/nonwelfare income on eligible women imply that if the government enforces the child-support responsibility of the absent parent and aids lone-parent families with nonwelfare assistance such as child-care subsidies, lone mothers will be more likely to work. This finding joins the inconclusive findings from studies done in the United States and should stimulate further research seeking clear policy implications. Methodologically, the analyses were sophisticated and appropriate. Readers will certainly find many sound treatments of methodological issues such as heterogeneity and stationarity.

However, there are still some rough edges. For example, the differential effects of welfare benefits and nonlabor/nonwelfare income were not theoretically pinned down. Although it is reasonable to assume a discouraging effect of welfare benefits due to its 100% taxing on earned income, the nontaxability of nonlabor/nonwelfare income does not explain why it would increase the probability of employment for women who are eligible for welfare benefits but decrease it for women who are ineligible. The author does not take into account the income effect and substitution effect of nonlabor/nonwelfare income and argue for the dominant effect of one or the other.

Another limitation is the lack of analysis of dynamic employment for never-married mothers. Although divorced mothers comprise the majority of lone parents, never-married mothers (about 30% of lone mothers) deserve a careful study for two reasons: First, this group appears to be growing. Second, never-married mothers are the least disadvantaged in terms of economic well-being and marriage opportunities. Understanding their employment dynamics will provide important policy implications.

Nonetheless, these weaknesses do not outweigh the contribution of this book. *Lone Parenthood* will be an invaluable source for studies on poverty and single-mother families in fields such as sociology, economics, demography, and public-policy studies.

The Social Survey in Historical Perspective, 1880–1940. Edited by Martin Bulmer, Kevin Bales, and Kathryn Kish Sklar. New York: Cambridge University Press, 1991. Pp. xix+383. $64.50.

Tom W. Smith
NORC

There are two historical perspectives in this collection of papers on social surveys. First, we see how social surveys emerged in response to the social problems of the urban-industrial revolution of the 19th century and how they fit in with other efforts to study and ameliorate the blighting of industrial cities. Second, we learn in what ways social surveys were precursors to later social-scientific approaches such as community studies and national, full-probability samples. The overall place of social surveys is summarized in the introductory chapter, "The Social Survey in Historical Perspective," by Bulmer, Bales, and Sklar and in "The Social Survey in Social Perspective, 1830–1930" by Eileen Janes Yeo.

Later chapters describe and evaluate several of the major social surveys in England and America. Bales describes what is usually considered the first social survey, Charles Booth's study of the *Life and Labour of the People in London.* Sklar covers the Hull House studies of Chicago. Unfortunately she focuses more on Florence Kelley and the sexism that hampered Kelley and other Hull House women, such as Jane Addams and Julia Lathrop, than on the scientific and social contributions of their social surveys. Bulmer examines W. E. B. Du Bois's study of *The Philadelphia Negro.* While Du Bois's study of blacks in the Seventh Ward is a miniature compared to Booth's panoramic landscape of London, Bulmer rightly characterizes it as a neglected masterpiece. Finally, Steven R. Cohen in "The Pittsburgh Survey and a Social Survey Movement: A Road Not Taken" discusses one of the most extensive of the social survey efforts.

While most of these case studies consider how earlier social surveys influenced subsequent efforts, the development of the social survey was not merely sequential events, but part of a social movement to study and better society. This aspect is covered in Stephen P. Turner's discussion of the role of Columbia University in this movement and in Jennifer Platt's account of Anglo-American research collaboration before 1945.

A series of other chapters explore various measurement and substantive issues that run through the various social surveys. For example, E. P. Hennock compares how British social surveys struggled with the concept of poverty and how to operationalize it. Alain Desrosieres examines the "prehistory" of sampling and discusses how the ideas of representativeness and random selection replaced the notions of case studies and comprehensive coverage.

Ultimately social surveys died out as the collaboration of reformers and inquirers that was a hallmark of the social survey broke down. Reformers concentrated more on organizing the afflicted groups and direct action, while the inquirers grew into social scientists more interested in studying social conditions and developing theories to explain social processes. These developments are covered in chapters by Bulmer, Roger Davidson, and Seth Koven.

Overall these chapters present a thorough, scholarly treatment of the origins, achievements, and demise of social surveys in Britain and America. (Irmela Gorges ably discusses the social survey in Germany, but even so we really only learn about the Anglo-American situation.) We can learn much not only about the development of contemporary social science from this volume, but about how we can improve our current practices. For example, the social surveys combined closeness to the subject population, concrete information on the lives and problems of the people, and comprehensiveness in a way seldom seen today. In addition, their use of colored maps conveyed important information and revealed spatial relationships in a vivid manner rarely achieved today. This book allows us to learn both about our past and from it.

It's a Working Man's Town: Male Working-Class Culture in Northwestern Ontario. By Thomas W. Dunk. Montreal: McGill-Queens University Press, 1991. Pp. xii + 191. $34.95.

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This engaging and stimulating ethnography is about the culture of young, white, working-class males in the Thunder Bay district of northern Ontario. Dunk's data is based on a study of the leisure lives of 21 men, placed in the broader context of the occupational and ethnic structure of northern Ontario. His aim is to explain why, and to what degree, these men are integrated into the social structure of capitalism in Canada. In the process, he discusses many of the theories that examine the same questions for the working class in advanced capitalist economies.

The economy of northern Ontario, based on forestry, mining, transportation, and tourism, is often perceived as a hinterland of more economically dynamic southern Ontario, and in particular of the city of Toronto where white-collar jobs predominate. Compared with Toronto, the city of Thunder Bay has a distinctly "working man's" ambience.

Two accounts form the empirical heart of Dunk's study. One transmits the attitude of the white working class to "Indians." For these workers, as for many whites in northwestern Ontario, the most important racial and ethnic distinction nowadays is that between whites and Indians. Dunk suggests that white working-class antagonism toward Indians has several sources. One is a belief that, when it comes to the allocation of resources for subordinate groups, Indians are favored by the government in the south (Toronto and Ottawa) at the expense of white workers. This belief reflects the North-South divide in Ontario. Another source is the historical use by elites of a favorable image of Indians (the "noble savage"—moral, generous, clean, honest, and so on) to which is compared unfavorably the white working class (dirty, devious, narrow-minded, and so on). White workers, Dunk argues, react against these stereotypes by switching the groups to which they are applied.

Dunk's second empirical account details the importance of participant sport in the lives of the workers he studied, and in particular of a version of softball called "lob-ball." Despite its organization into leagues, an informal but important rule of lob-ball is that players must not be too competitive—having fun is paramount. For example, fielders and batters take cigarettes and beer onto the field. During one game that Dunk describes, a player made a long, running catch of a fly ball without removing his cigarette from his lips. From May until August, lob-ball is the passion of the workers Dunk studied. They play in a range of tournaments, some of which involve extensive travel. In some respects, Dunk argues, lob-ball is a way in which these men differentiate themselves from the dominant values they find at work. This includes the importance attached to having fun, to solidarity, and to group friendship. However, in other respects lob-ball is marked by the commodification of sport. Examples include the substantial sums players spend on equipment and the pervasive sponsorship of teams and tournaments by business.

Dunk concludes by distinguishing his own view of the role of culture in class domination from that of other writers. He criticizes the view that class is just one of several factors that inform working-class life. For Dunk, class position is the basis of everything else (as shown, for example, by his analysis of the causes of white working-class antagonism toward Indians). And he criticizes versions of Gramsci's theory of hegemony that imply that modern culture dulls workers' critical faculties vis-à-vis the class-biased nature of the modern state. Dunk's own view of culture, in this regard, is that workers use it to express class antagonisms, but in the process they get tangled up and distracted. Thus in lob-ball they try to avoid capitalist work relationships but are seduced by consumerism (expensive equipment, corporate sponsorship). In their attitudes toward the Indians, they resist domination from bureaucratic, white-collar Toronto, but end up embracing outright racism.

As is evident, this book offers much to discuss and debate. It is very well written, and makes an important contribution to debates over modern culture and to ethnography of the working class in Canada.

Pedagogy and the Struggle for Voice: Issues of Language, Power, and Schooling for Puerto Ricans. By Catherine E. Walsh. New York: Bergin and Garvey, 1990. Pp. xxvii + 167. $39.95 (cloth); $14.95 (paper).

Richard H. Brown
University of Maryland

This is a nice book on an interesting theme. Catherine Walsh brings considerable passion and experience to the question of why the perspectives of Puerto Rican students are ignored in the design of their learning environments and teaching practices. As persons on the receiving end of a relationship of domination, Puerto Rican youths understand how knowledge is infused with power. But their efforts to make sense of conflicting forces that shape their lives are rarely understood by their teachers, argues Walsh, and thus their "struggle for voice" is misinterpreted or ignored.

This view challenges the theories of cultural disadvantage or verbal deprivation that are often cited to explain low rates of educational attainment by minority children. Drawing on critical work by language theorists such as Mikhail Bakhtin, Basil Bernstein, and Pierre Bourdieu to illuminate historical and ethnographic data, Walsh argues that, on the contrary, students' conduct is an adaptation to conditions of cultural alienation and political oppression. Only by recognizing the validity and autonomy of their culture can pedagogues begin to encourage academic participation and learning by Puerto Rican children.

Walsh uses three principal rhetorical devices to make her case—the truth of history, the wisdom of the sages, and the authority of experience. Thus her chapters deal respectively with the history of American colonization in Puerto Rico and the suppression of its culture and language, the writings of major theorists of sociolinguistics and pedagogy, and the experiences she has had in working with Puerto Rican youths, including a small field survey.

The author focuses especially on the effect that colonial status has had historically on the political, socioeconomic, and psychological reality of the Puerto Rican people. Through the voices of Puerto Rican children and other Latino adolescents, she explores how the past and the present intersect in people's lives, inform pedagogy, and shape the conditions and struggles through which students come to know. Thus Walsh rejects generally accepted perspectives and practices among teachers and calls for new theories of pedagogy that respond to the complex needs of these students.

All this is interesting and properly critical of conventional approaches. Nonetheless, Walsh's work contains serious intellectual and political defects. First, little new is presented here. The discussion of Puerto Rican history seems to be taken almost entirely from secondary sources and contains nothing that a specialist would not already know nor anything that an ideologically sympathetic reader would not expect. Similarly, the discussions of pedagogic and sociolinguistic theory are nice summaries, but not original even as summary statements. The field reports also sound familiar—for example, Walsh finds that concepts like "respect" and "education" have quite different meanings for Latino and Anglo children. All this would be acceptable had the author integrated these diverse materials into a compelling new argument. But this is not accomplished.

Further, although Walsh laudably advocates a greater voice for Puerto Ricans and other dominated peoples, she does not engage the more difficult practical and political questions of how this is to be accomplished. True, as she says, bilingual teachers, more open classrooms, and the like would probably make a big difference. But there are also large obstacles to such innovations. One of the more obvious is that mass education seems to compel standardization and bureaucratization which subvert the more personal approach advocated by the author. Moreover, truly bilingual, process-oriented pedagogy tends to be highly labor intensive and, hence, much less cost-effective than the batch-processing approach which the author criticizes. Of course, in the longer run, or in the larger picture, the costs may be even greater if we continue the warped schooling that is inflicted upon so many of our children. But Walsh does not consider this longer run or larger picture, and this omission tends to undermine her recommendations.

Finally, the empowerment of minorities seems to presuppose the continuity of some more general civic discourse of which such a plurality of voices is a part. But what is or might be such a civic discourse in America today? This question, and that of the proper relations between the center and the margins, is crucial to any discussion of minority empowerment, but it is not noted by Walsh. This is a lacunae in much Left discourse generally, and it weakens the Left position not only intellectually but morally and politically: morally, because the value of gaining a voice is vitiated if there is no "public" of which that voice can be a part; politically, because we on the left will never win if we cannot do more than argue from and for the margins.

The Social Psychology of Minority Influence. By Gabriel Mugny and Juan A. Pérez. Translated by Vivian Waltz Lamongie. New York: Cambridge University Press, 1991. Pp. xii + 190. $64.50.

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This volume reviews and theorizes about a series of experiments on minority influence done by the volume's authors and others. Its goal is to present and partially evaluate their theory of both the process of individual conversion to a minority's position and the diffusion of social innovation stimulated by the actions of minorities. The theory involves mainly the effects of identifing the source of a communication as either an in-group or out-group member in interaction with variables such as presentation style, argument content (e.g., political or moral values stressed), and the public or private conditions of responding to the communication. The outcome variable is the immediate or delayed impact of the message on the target person's opinion.

The book's argument is that positions advocated by minorities influence the opinions of those in the majority by creating tension which results from the appearance of a counternormative position in the social environment. Responses to a position advocating social change are mediated through cognitive processes called "psychologization" (active resistance to the advocated position) or through "denial" (immediate apparent resistance to the position followed by change supposedly triggered by attention to the content of the message).

Despite the promise of the book's title, its stated goal of explaining social change, and the interesting labels attached to variables (e.g., minority, conversion, and so forth), it is unlikely that sociologists or sociologically trained social psychologists will find the book exciting. Its weaknesses relate to two issues: substance and method.

Neither the reported research nor the theory developed from it have much relation to the complex process of social change, to issues of minority/majority relations, or to intergroup interaction as studied in sociology. For example: the concept of social innovation is entirely restricted to a measure of *apparent* individual attitude change; "minority" refers to anything from persons advocating a rarely chosen alternative (including a preference for particular geometric shapes), mysterious groups who take pains to identify themselves as generic minorities (even as simply as, "our minority group," p. 173) in their communiqués, racial and ethnic groups, and even generic minority groups demanding immediate social change by invoking the sort of strident political rhetoric one finds in satires (at least) of campus or community disputes in Berkeley. Unfortunately, the "minority's" positions in the reported experiments too often reduce to slogans at about the level of 15-second spots in the final days of a presidential election campaign.

The research reported in this volume is perhaps most productively classified as a continuation of Hovland and Janis's studies of communication and persuasion with an overlay of topicality introduced by appropriately named concepts. A scrutiny of the operations used to define concepts in the reported experiments in relation to their common language meanings leads to the conclusion that nothing but a note of drama is being introduced. The seeming high drama of the research is attributable to the language used to describe it rather than to the substance of what was done.

The theory that is developed deals exclusively with individual attitude change in isolation from any possibility of counterargument, peer- or reference-group relations, self-interest, or any other consequences. Rather than attempt to experimentally evaluate a theory developed from studies of social change or conflict, the authors proceed on the assumption that their research includes all of the variables typically implicated in and therefore necessary to explain the phenomenon of interest. The assumption that all of the operating variables are in play in these experiments may not be warranted.

The book's principal methodological problem is that the pattern of experimental results are at least as well explained by classifying the research as yet another example of the situated-identity phenomenon. Alexander and Zucker and their associates experimentally demonstrated the powerful, if not overwhelming, effects of concerns about identity presentation and the importation of contaminating norms into supposedly sterile laboratory environments. Their devastating critique of naive experimentation highlighted the conflict between subjects' concerns regarding the maximization of positive identity through the impressions created by their choices in experiments and the researchers' naive desire that subjects attend only to the variables of experimental interest.

Experiments such as those reviewed in this book, in which what is measured is agreement with sources who are either heavy-handedly stigmatized or presented as socially approved cannot avoid situated-identity contamination problems. One of the book's principal findings, an unwillingness to display public agreement with stigmatized sources contrasted with a willingness to agree privately, is particularly susceptible to a situated-identity interpretation. Unfortunately, no attention to this alternative social psychological theory for the results obtained was considered in the volume.

Housing and Social Theory. By Jim Kemeny. New York: Routledge, 1992. Pp. xix + 192. $69.95.

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The purpose of *Housing and Social Theory* is to align housing research more closely with social science theories in order to enrich both housing studies and social theory. The dominant goal of the book is to "reintegrate housing into broader issues of social structure" by defining the field of housing in relation to other disciplines and by examining more closely the basic assumptions underlying housing research. Part 1, titled "Housing and Metatheory," addresses both these issues in its chapters: "The Disciplinary Basis of Housing Studies" and "The Epistemological Grounds of Housing Studies."

Part 2, "Bringing Theory Back In," deals with political science approaches to theorizing the state (chap. 3), theories of social change (chap. 4), and theories of welfare (chap. 5). Each chapter includes a review of the theoretical and housing literatures and the ways in which connections that have been missed could be made.

The third part of the book, "Toward a Divergence Thesis in Comparative Housing and Research," applies the ideas presented in the first two parts to the development of a divergence thesis built on earlier comparative work by Kemeny. Chapter 6 ("Ideology and Divergent Social Structures") and chapter 7 ("Divergent Social Structures and Residence") are general theoretical discussions, while chapter 8 ("The Political Construction of Collective Residence: The Case of Sweden") is a case study in theorizing housing.

The book ends with Part 4, "Toward a Theoretically Anchored Sociology of Housing," with a chapter on "Residence and Social Structure" and the conclusions. In these chapters Kemeny presents the outlines of a critical approach to housing based on C. Wright Mills's sociological imagination—questioning of the "taken for granted" elements of social structure and housing's embeddedness in that structure. Kemeny argues that housing constitutes such a fundamental dimension of social structure that the study of residence should be the central contribution of housing

studies to an understanding of society; he proposes
book “A Prolegomenon to a Sociology of Residen

Kemeny defines housing studies as the study of
economic, cultural, and other institutions and rel
tute the provision and utilization of dwellings”;
housing studies as “the focus for the social relati
indirectly involve the activities of planning, constru
use of shelter.” The study of “residence,” on the
the dwelling within its social context. Kemeny ide
key dimension in the social organization of modern
linking individual behavior with macrolevel societ

The book’s main contribution is to a sociology
dresses the relationships between households, dwe
social structure. Kemeny raises the important issu
proach to housing studies. For example, do coll
(such as exist in Sweden) actively shape a sociali
housing type a product of that ideology? A related
on urban form, and thus on social structure, of diffe
tenure. Kemeny suggests that “the single differenc
the predominance of one dwelling type over anoth
found consequences for the social organization of
fact alone could possibly constitute the basis for
gence between industrial societies” (p. 124).

While Kemeny’s book identifies an interesting
exploration of social and spatial institutions in the
theory—its steep price will probably make it mor
than for private libraries.

Urban Housing Segregation of Minorities in Wes
United States. Edited by Elizabeth D. Huttman, W
Saltman. Durham, N.C.: Duke University Press,
$59.95.

Nancy A. Denton
State University of New York at Albany

The major strength of this work lies in its fusion o
segregation on two continents, although the real co
ries of segregation for African-Americans in the
more recent segregation of immigrants and many
European countries is problematic at best. Clearly t
of African-Americans in the United States is much
in any European country. This point is noted by
disquieting that the European authors frequently di
tion indices decidedly in the moderate to low range.

that the conclusion that seems to emerge is that where there are few persons of black race, some other group must occupy the lowest rung or the "most despised" position. Reading about Western Europe also makes very clear the link between segregation and quality of housing, a link often not emphasized in U.S. studies. It also points out that more extensive government control of the housing market (in some of the European countries covered the government controls one-quarter to nearly one-half of the housing) in and of itself is not enough to insure housing integration. I must emphasize that these are my own conclusions because, despite introductions and conclusions by the editors, the pieces of the book remain rather disjointed, a danger in edited volumes that this one did not avoid.

Following a preface by Huttman, the book begins with a theoretical orientation chapter by Saltman which unfortunately is substantively narrow and methodologically dated. For example, her overview of the measurement of segregation ends with the index of dissimilarity. The book is organized into four sections: housing segregation in Western Europe, European policy, U.S. housing segregation, and U.S. policy. Huttman provides introductions to the Western Europe and U.S. sections, while Blauw and Huttman provide an overall conclusion and summary to the volume. The sections on Western European segregation and U.S. policy are better than the other two sections.

The range of European countries covered is impressive: the Netherlands (Blauw), Britain (Deborah Phillips and Valerie Karn), Sweden (Anna-Lisa Lindén and Göran Lindberg), West Germany (Jürgen Friedrichs and Hannes Alpheis), France (Maurice Blanc), and Switzerland (Michal Arend). The articles read like literature reviews for their respective countries and provide the reader with a good introduction to segregation there. I particularly liked the one on West Germany as it incorporates more measures of segregation, and I was struck by the inclusion of South Asians as blacks in the analysis of segregation in Britain.

The European policy section, introduced by Alan Murie, is more narrow, focusing on Dutch metropolitan areas (*Ger Mik*) and West Berlin (*Cihan Arin*), and ends with an overview by Huttman. Of these, I recommend the article on West Berlin. All the European pieces share a tendency to "explain away" segregation, however, and to emphasize the role of housing preferences while paying scant attention to issues of discrimination.

The geographical diversity in the U.S. segregation section is not as wide as that in the European survey: Gary, Indiana (Barry Johnston), Houston (Clyde McDaniel, Jr.), and Phoenix (Leonard Gordon and Albert Mayer) are the only specific metropolitan areas analyzed, though their respective chapters attempt to cover the entire Midwest, South, and Southwest regions. Ignoring most of the South and the Northeast seems a bit odd, and one wonders about the housing segregation in the really large cities with enormous numbers of minorities, particularly African-Americans, like New York, Chicago, and Los Angeles.

The U.S. policy section, introduced by Rose Helper, is much broader in scope. One of my favorite pieces is the one by Saltman, which when read after the case study of Cleveland by W. Dennis Keating, provides an excellent summary of the myriad forces attempting to deal with the issue of segregation in the U.S. The piece by John Goering and Modibo Coulibably, while perhaps more informative about data lacunae than actual policy, does a nice job of orienting the reader to the dilemmas of public housing segregation as it interacts with the broader housing market. Huttman and Terry Jones do a good job of discussing the problems of suburban segregation.

Given the extremely high price, datedness, and generality of the material, I cannot recommend this volume for individual purchasers. It does, however, warrant careful reading in a library, both for the entrée and introductions it provides to the wide-ranging areas covered, because some of the articles may prove useful to fill in gaps in course syllabi, and because the comparative framework raises issues not found elsewhere.

Inside the RUC: Routine Policing in a Divided Society. By John D. Brewer with Kathleen Magee. New York: Clarendon Press, 1991. Pp. 305. $48.00.

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John D. Brewer, with the assistance of Kathleen Magee, has written an excellent book on the sociology of policing in Northern Ireland. The term sociology is emphasized because the author focuses on the organizational structure, social behavior, and systemic aspects of the Royal Ulster Constabulary (RUC) as it polices a divided society. The sociology of policing is emphasized in another manner, for the book is actually an ethnographic study of the problems of routine policing in Northern Ireland.

Even though the book describes routine policing and its problems in an area called Easton (not its real name), an area of Belfast where political violence is absent, the author demonstrates how this policing both parallels and differs from that in other divided societies such as Lebanon, Israel, South Africa, and to some extent policing within Anglo-American societies or cultures with dominant-controlled hierarchies frequently referred to as "liberal democracies" (p. 155).

Although the title of the book may seem narrow, it is the subtitle—*Routine Policing in a Divided Society*—that actually describes the focus, thus making the book applicable to policing in any type of society. Brewer depicts how the police personnel in Easton are similar to police personnel in any other area, yet he integrates this description with a discussion of how the political and religious violence between the Protestant and Catholic sections of Belfast have affected the work of the police.

"[The] social cross-section gives Easton's police a variety of crime and law-and-order situations to confront, in conjunction with which there is the ever-present danger and threat from the IRA" (p. 34).

There are eight chapters and a conclusion in Brewer's book, which in turn can be grouped into three sections: (1) chapters 1–3 deal with routine policing, (2) chapters 4–6 focus on how societal divisions affect policing in Easton, and (3) chapters 7 and 8 describe how the police culture in the RUC parallels and differs from those in other police forces as a result of these divisions.

In the first section Brewer describes the mundane tasks that characterize routine policing even for the RUC. In this section it becomes evident that even the RUC is not constantly involved in crime fighting; in fact public service and community assistance are predominant. "Big crime" calls are rare and routine policing entails dealing with domestic disputes, drunken fights, loitering youths, and the like. At the same time gender problems or constraints are a concern for female officers in the RUC. While acknowledging the routine policing of the RUC, Brewer addresses the dislike for police paperwork, the emotional coldness or detachment in job performance, the strong desire to fight and detect crime as the police person's primary role, the "typification" (p. 67) as developed by RUC officers and police personnel in other societies, and the structuring of police discretion and occupational competency. Throughout this section and the others Brewer uses quotes from his fieldnotes to substantiate the ideas presented.

Having established that policing in Easton is parallel to policing in any society, in the second section the author reveals how policing in a divided society is different. "Societal divisions are such that neighborhood patrols are dangerous and have to take on the appearance of armed convoys, but it is the style of policing which comes nearest to breaching the divide between the police and the community" (p. 116). As Brewer describes several divisions (social and political conflicts, religion, sectarianism, and even the officer's personal biography and experiences) that could influence the officer's job performance, he demonstrates how frequently the officer's professionalism is tested. Even though Easton's police do not undertake high-profile trouble-related work, they too must make adjustments and are constantly aware of the danger and stress of the occupation. In this section Brewer addresses the perception of danger and stress as viewed by the RUC members and how it is normalized as within any policing position. He addresses the police personnel's concern for safety, their sensitivity to family matters, and the aberrant behavior that develops among officers as a result of mismanaged stress.

In the final section Brewer discusses the professional and impartial ethos of the RUC station in Easton, the occupational culture of the RUC, and the sociology of policing. Brewer presents the "cop culture" by identifying the commonalities found between the RUC organization and other police units. His ethnographic research becomes very useful here as he conveys the officer's perception of self and job. This final section can best be applied to policing within any society.

Brewer describes the policing of a society where paramilitary skills are essential for officers' survival, while at the same time neighborhood policing is the bureaucratically desired style. Brewer has done an outstanding job in covering the RUC; his use of an ethnographic approach gives credence to the book. There would be no problem in transferring the work or policing style described in Brewer's book to any other policing organization in any society.

The State and Women in the Economy: Lessons from Sex Discrimination in the Republic of Ireland. By Jean Larson Pyle. Albany: State University of New York Press, 1990. Pp. xviii+202. $47.50 (cloth); $15.95 (paper).

Kathryn Ward
Southern Illinois University

Economist Jean Pyle has written an interesting case study of Irish women workers in the manufacturing export sector during the years 1961–81. Over time Ireland has had the characteristics both of industrialized and of currently developing countries in terms of economic dependency and industrialization. During the 1960s Ireland was a leader in attracting foreign investment and export-oriented industries, such as clothing and electronics, which historically in other locations have been more likely to hire women workers. Yet, women's share of the Irish labor force remained low, in contrast to increases in countries such as Singapore.

Pyle explores how state policy interacting with other socioeconomic factors can shape women's employment opportunities in export-oriented production. This research has definite implications for the debates on the effects of transnational corporate investment on the empowerment of women workers. Foreign investment does not automatically provide more economic opportunities and empowerment for women workers.

Pyle explains this outcome through a careful consideration of prevailing theories of modernization, Marxism, and feminism and finds that they are theoretically lacking, unless we analyze how the state can shape investment, labor force, and family policies. Her evidence is aggregate-level comparative statistics on women's employment and demographic characteristics, Irish development documents, and legislation. First, she notes that, other things being equal, women's share of the labor force should have increased over time in Ireland and that many stymied women workers emigrated in search of jobs. Second, in careful detail, she traces how the Irish Development Authority (IDA) gave greater consideration to investment that would promote the employment of men over women. This action was undertaken in conjunction with the provisions of the Irish Constitution that held that women's primary roles were as wives and mothers rather than workers. The marriage bar and the prohibition of women's night work reinforced these provisions. The prohibition of divorce and abortion and the restriction of birth-control devices to married women and men solidified patriarchal control over women in the family.

Because of increasing pressure from the European Economic Community (EEC) to recognize the equal rights of women workers, Ireland gradually ended legal barriers to women's work, for example, the marriage bar. This action had immediate effects: between 1971 and 1974, married women's employment doubled. However, state constraints on women's marital and reproductive freedom prevailed. Women's labor-force participation and share of the labor force in Ireland remained the lowest in the EEC.

Pyle's major contribution is tracing how the state can interact with the family, firms, and the economy to shape women's economic roles through investment incentives, legislation, firms, and family and reproductive policies. This is important because many of our theories assume: (1) economic transition from agriculture to service and industry will involve the socioeconomic incorporation of women, (2) that state expansion and expenditures will lead to greater employment and empowerment of women, and (3) export-oriented investment and production lead to increases in women's employment and women's empowerment vis-à-vis men. These relationships are rarely automatic and often are temporary. Thus, researchers and policymakers who seek to improve women's access to economic resources need to closely scrutinize the role of the state.

Since Pyle uses aggregate level data to test her theoretical explanations, much work remains in delineating the ways that various governments shape women's access to socioeconomic resources through directives and incentives to firms and legislation. Her links need more specification. This future research can come through comparative case studies across regions and the world. Pyle's findings should be particularly relevant as Eastern European countries restructure their economies. Already, women workers report layoffs and changes in state policy that restrict their opportunities: emphasis on men's employment, cuts in child care in the eastern section of Germany, and moves to ban abortion in Poland.

This research is of interest to sociologists, economists, and political scientists in the areas of global economic restructuring, women in development, the state, and export-oriented production. This book is an excellent case study for graduate seminars in social change, development, and women in development.

Dangerous Passage: The Social Control of Sexuality in Women's Adolescence. By Constance A. Nathanson. Philadelphia: Temple University Press, 1991. Pp. xii+286. $34.95.

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Dangerous Passage is an important and provocative discussion of current responses to teenage pregnancy in the United States. Unlike some of the other observers of this phenomenon, Nathanson directly connects the issues of teenage pregnancy and childbearing to the larger—even more discomforting—issue of (female) teenagers' sexuality: "Adolescent pregnancy has become a generic term for the sexual unorthodoxy of young women. . . . She visibly symbolizes the breakdown of both moral and medical forms of social control" (pp. 163–64).

This book, written in the social problems/social constructionist tradition of Howard Becker, Joseph Gusfield, and others, argues, in brief, that the first successful "entrepreneurs" of teenage pregnancy were the professional family planners—researchers and public health specialists—associated with the Alan Guttmacher Institute in the late 1970s. This group succeeded in convincing Congress that pregnancy among teenagers was approaching "epidemic" levels (a finding which others have disputed to varying degrees) and that birth control services for teenagers should be vastly expanded. Such expansion has meant not only low-cost or free services at federally supported clinics, but also legislation that mandates confidentiality for minors receiving family-planning services.

With the election of Ronald Reagan in 1980, and the rise more generally of such related groups as the antiabortion movement, the New Right, and the "profamily" movement, moral conservatives began to make very forceful, and ultimately quite successful, counterclaims to the "ownership" of the teenage pregnancy issue. Gaining control of critical appointments within the relevant federal bureaucracies, this group dramatically recast the problem as one of female premarital sexuality, and accordingly, sought to reshape publicly supported services into abstinence counselling services (or, as critics have referred to them, "chastity centers").

Both of these earlier constructions—the sexually active teen in need of birth-control services and the sexually wayward teen in need of moral guidance—have been supplanted in the late 1980s, according to Nathanson, by a reframing of adolescent pregnancy in explicitly racial terms—the black teenage mother on welfare. Nathanson refers to this as a "neo-Moynihan" construction (p. 66), referring to the controversial work, two decades earlier, of Daniel Patrick Moynihan who first drew widespread attention to impoverished, black female-headed families. Under this construction, the predominant service requirement is education and training programs that would make these young mothers economically self-sufficient.

One could offer a slightly different interpretation of the present moment in teenage pregnancy politics than that presented by Nathanson. To be sure, the neo-Moynihan impulses that the author describes are very evident in our society at this time; the coercive uses suggested by politicians and legislators for the new contraceptive implant, Norplant, such as mandating this contraceptive for welfare recipients, and the various "welfare reforms" now current which call for financial penalties on teenage recipients bearing additional children, are eloquent testimony to Nathanson's point. Nevertheless, I would argue that moral conservatives' "ownership" of this problem is still very much with us; the overall number of birth-control facilities servicing teenagers (and older low-income women) is going down, and the remaining clinics exist in a highly vulnerable state because of such successful right-wing maneuvers as the "gag rule" (a Bush administrative ruling, upheld by the Supreme Court, which permits withdrawal of federal funding if clinics mention the option of abortion to their clients). In short, I argue that the present politics of teenage pregnancy are highly complex and contradictory, in which undeniable societal hostility to the procreative behavior of young black women coexists with a still-powerful social movement that would deny birth control (and, of course, abortion services) to all unmarried teenagers.

This is a minor quibble with a book that I feel is representative of the best of social problems scholarship. *Dangerous Passages* is meticulously researched, gracefully written, draws artfully on historical parallels, and in the tradition of this genre, shows the reader that a problem that has fundamentally *social* roots is nonetheless addressed—by all would-be "owners"—at the level of individual behavior.

Unfortunately, this book has one flaw that is also typical of much social constructionist writing. True to the genre, the book is written with great sympathy for female teenagers, and with great skepticism of the various adult entrepreneurs in this field, but the author remains above the fray. Nathanson skillfully exposes the political mileage to be gained from such phrases as "children having children," yet what does Nathanson herself think of pregnancy and childbearing at the age of 15? Is it "good" for the mother herself? For the child's long-term chances? If not, is it reasonable for public policy to attempt some response to teenage childbearing, even before long-term structural changes in the economy take place? Unfortunately, these are questions that Nathanson does not address. I hope that she will do so in subsequent work.

Driving a Bargain: Automobile Industrialization and Japanese Firms in Southeast Asia. By Richard F. Doner. Berkeley and Los Angeles: University of California Press, 1991. Pp. xv+371. $39.95.

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The automobile industry has a certain fascination for students of industrialization and social change. It typically is viewed as the centerpiece of economic growth for most developed market economies in the 20th century, as well as the industry around which the United States pioneered its spectacularly successful system of mass production from 1913, when Henry Ford first introduced the moving assembly line, until the 1960s. The significance of automobile manufacturing lay not only in its scale and global scope, but also in its immense spin-off effects through its links with numerous other industries.

Since the 1970s, the global pattern of motor vehicle assembly has entered a new era in which Japanese producers, led by Toyota, have revolutionized the way in which automobiles are designed and manufactured. Whereas mass production allowed manufacturers to make very large volumes of a limited range of standardized products for mass-market consumers, the new flexible system of "lean production" that emerged in Japan permitted auto companies to reduce costs per unit and dramatically improve quality while providing an even wider array of products.

Richard Doner's *Driving a Bargain* is an important book that pushes our understanding of Japan's automobile industry to a new level by focusing on its relatively recent shift to offshore production in Southeast Asia. Unlike U.S. car companies, which already had manufacturing plants in their major overseas markets by the 1950s, the Japanese automobile assemblers were still laying the bases for their international expansion in the mid-1980s. In Southeast Asia, where the Japanese production presence is most extensive, motor vehicle companies are moving beyond auto assembly to an emphasis on more integrated local manufacturing as well as growing exports of auto parts from these Asian production sites.

The heart of this book is Doner's thorough, firsthand analysis of automobile policies and Japanese production patterns in four Southeast Asian nations: Malaysia, Indonesia, the Philippines, and Thailand. Using an impressive array of personal interviews and secondary materials, the author dissects the policy objectives and accomplishments of each of these countries in four areas of auto-industry reform: the local content of automobile production, market rationalization efforts aimed at reducing the numbers of auto makes and models in order to achieve greater economies of scale, joint-venture requirements to assure an indigenous presence in automobile manufacturing, and export promotion. Although none of the four Southeast Asian countries achieved all of their initial goals, Doner's comparative ranking of auto-policy success reveals that "Thailand emerges as the region's best performer, the Philippines as the worst, and Malaysia and Indonesia are together in the middle" (p. 62).

The theoretical framework for *Driving a Bargain* is derived from two contrasting approaches to the bargaining process in Third World manufacturing industries: the structuralist (or dependency) approach, which argues that the substantial entry barriers to high-tech industries prevent an expansion of host country leverage, and the product-cycle approach, which highlights the potential for a gradual expansion of local leverage even in advanced manufacturing as developing countries utilize interfirm competition between transnational corporations to improve their bargaining position. The product-cycle perspective is less state-centric and more coalition-based than the structuralist view, and it leads Doner to propose what he calls a concertationist approach to development.

The notion of concertation as used in this book emphasizes negotiation and collaboration between public officials responsible for a particular sector (such as autos) and the organized, internally differentiated private actors in that sector. Differences in these functional coalitions between business groups and governments emerge as the most important factor explaining cross-national variation in bargaining performance within the automobile industry in Southeast Asia.

Driving a Bargain is an innovative study in several respects. It highlights the globalization of production that characterizes most modern industries during the past several decades. Furthermore, the author goes beyond standard indicators of globalization to show that the strategies of the Japanese automotive companies have led to a regional division of labor in auto parts supply in Asia. The four Southeast Asian economies are, in effect, ascending an auto-manufacturing ladder. The Japanese firms at the top, which provided the initial impetus to automotive development in South Korea and Taiwan, are now trying to integrate Thailand, Malaysia, Indonesia, and the Philippines into a regional production hierarchy controlled by Japanese motor vehicle assemblers to enhance Southeast Asia's potential as an automotive export base.

The most distinctive feature of this book is the high-quality comparative research carried out by the author to develop his arguments about bargaining and state-society relations. Doner conducted extensive field research in Japan and the four Southeast Asian nations in order to identify the coalitional bases of industrialization in the region. His analysis shows how countries with weak states can exploit opportunities for local industrial growth despite significant external obstacles.

While the strength of this study lies in its attention to coalitional patterns and bargaining dynamics at the societal level, the shifting patterns of competition in the international automobile industry are delineated in a rather sketchy fashion. The changing roles and distinctive global strategies of the U.S. and European auto companies clearly affect what the Japanese assemblers are doing in Southeast Asia and elsewhere, yet these receive little attention in the book. Furthermore, it would be interesting to know how the internationalization of Japan's automobile producers in

Southeast Asia is linked to the distinctive organization of Japan's domestic automotive industry, in contrast to its American and European counterparts. These are side issues to the author's main story, however. Overall, this is an excellent and path-breaking study that sets a new standard for comparative research on international development.

With Liberty and Justice for Whom? The Recent Evangelical Debate over Capitalism. By Craig M. Gay with a foreword by Peter L. Berger. Grand Rapids, Mich.: William B. Eerdmans Publishing, 1991. Pp. xii+276. $19.95.

Laurence R. Iannaccone
Santa Clara University

How do evangelical intellectuals view capitalism? Based on media reports of the past decade, one might expect their support for "conservative" economics to be just as strong and consistent as their support for "conservative" theology. Yet nothing could be further from the truth. Craig Gay's highly detailed study proves that evangelical scholars now occupy every point of the economic spectrum, from free market zealots to quasi-Marxist radicals.

Gay's book contains four chapters, framed by a useful introduction (which explains the nature of the study and provides valuable historical background) and an interesting epilogue (in which Gay critiques evangelical economic positions from a religious/biblical perspective). The first chapter reviews the writings of the "evangelical left," scholars who condemn capitalism in its totality, engaging in what amounts to a neo-Marxist critique of self-interest, private property, and wealth inequality. The second chapter turns to the "evangelical right," whose enthusiastic defense of capitalism leans heavily upon traditional neoclassical and libertarian thought. Chapter 3 describes the "evangelical center," a large and varied group of scholars who appreciate capitalism's productive efficiency but condemn its "excesses" and "failures." Over the past 20 years, the evangelical center has become increasingly supportive of mixed economies, governmental regulation, and state welfare programs. The collapse of Soviet-style communism appears to have halted this drift, but Gay predicts no end to the debate over "capitalist politics," "capitalist culture," and the role of the welfare state (p. 204).

How does one interpret the evangelical economic debate? Gay believes that "New Class theory" provides the most promising approach. According to this theory, contemporary Western societies pit the traditional "old" middle class (the producers of material goods and services) against a "new" middle class specializing in the production of symbolic knowledge. Whereas the interests of the old middle class coincide with those of business and the private sector, the New Class is a natural ally of the modern state and the principal supporter of its regulatory and redistributive apparatus. Evangelicalism, once restricted to the old middle (and lower) class, now contains an ever-increasing number of highly educated New Class members. Through a process of "cognitive bargaining," old and New Class evangelicals have arrived at opposing compromises with secular economic worldviews. The evangelical debate is thus just one manifestation of a broader, class struggle over the role of government.

There is no escaping Gay's fundamental insight, namely, that the economic views of evangelical intellectuals have become entangled with secular ideology and secular interests. On all sides, the statements (even those cast in biblical terms) mirror secular arguments too closely for theology to have been the critical consideration. On the other hand, it is not clear that New Class theory is needed to attain this insight or extend its implications. The New Class thesis compels us to frame the "debate" as a two-sided affair. But as Gay himself demonstrates, the parties to this debate do not fall into two discrete groups, nor are they even distributed bimodally. Rather, they are spread out in something like a normal distribution, with relatively small tails at the extremes and a very large and rather ambivalent cluster at the middle. This is not the stuff of class distinctions, let alone class warfare. The interpretation is further complicated by the fact that *all* the evangelicals in question are "intellectuals"—authors, professors, and theologians—and as such, stereotypical members of the New Class. Perhaps a more modest conclusion is in order: the range of economic opinion within American evangelicalism mirrors that of the larger society and has been shaped by standard, secular forces.

Whether or not one accepts Gay's New Class thesis, there is no denying the thoroughness of his scholarship and the importance of his basic findings. He has demolished widely held misconceptions concerning the "narrowness" of evangelicalism and the supposed link between theological and political-economic conservatism. He has also documented how the leaders of America's largest denominational family interpret and apply their scripture, theology, and tradition *selectively* so as to justify economic philosophies that originate outside their religious tradition. (A similar phenomenon has been observed within contemporary "Islamic economics," and one might predict comparable outcomes whenever members of a major religious tradition critique modern institutions in terms of ancient religious authorities.)

Craig Gay has produced a book that is exceptionally detailed, highly readable, and relevant to at least three different audiences: sociologists of religion, evangelical Christians, and social scientists interested in the interplay between religious and secular ideology. It is, on balance, a very impressive achievement.

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Supporting the argument that state policy and market conditions significantly limit and direct the applications of theory, directions for a new sociology of applied sociology are suggested.

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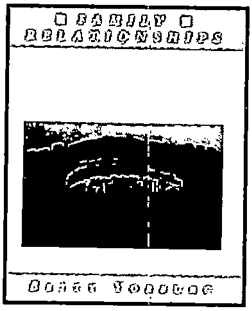

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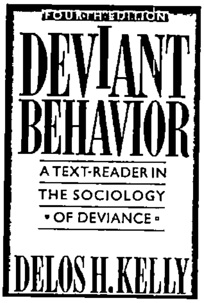
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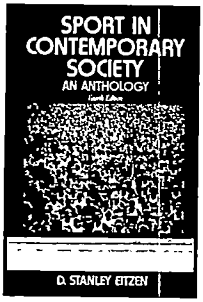
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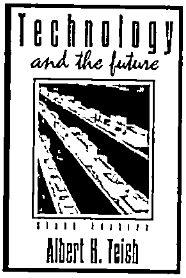
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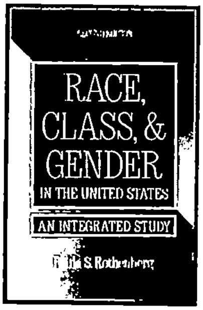
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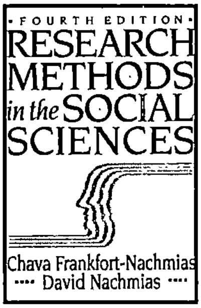

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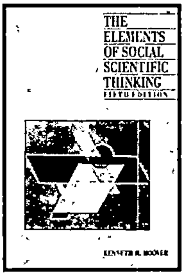

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(Rev. 7/92)

Was There a Hawthorne Effect?[1]

Stephen R. G. Jones
McMaster University

The "Hawthorne effect" has been an enduring legacy of the celebrated studies of workplace behavior conducted in the 1920s and 1930s at Western Electric's Hawthorne Plant. This article examines the empirical evidence for the existence of Hawthorne effects using the original data from the Hawthorne Relay Assembly Test Room. Allowing for a variety of other factors, the author assesses whether experimental changes, variously defined, had a common effect that could be regarded as a pure result of the experimentation. The main conclusion is that these data show slender or no evidence of a Hawthorne effect.

I. INTRODUCTION

An enduring legacy of the celebrated studies of workplace behavior conducted at the Hawthorne Plant of the Western Electric Company in the 1920s and 1930s is the so-called Hawthorne effect. Variously defined, the central idea is that behavior during the course of an experiment can be altered by a subject's awareness of participating in the experiment. Though not obviously more than an incidental and intermediate finding for the early researchers, the Hawthorne effect has come to occupy a central role in the methodology of experiments and continues to have widespread influence in social science and management textbooks and research and especially in research in the psychology of education. It is somewhat surprising that, although efforts have been made to identify Hawthorne effects in various areas of field experimental research, there has been no systematic study of the evidence from the Hawthorne Plant itself. My object in this article is to undertake such a study, recognizing the mixed experimental and field nature of the Hawthorne research.

I first review the widespread and largely uncritical acceptance of the

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[1] This research was supported in part by a grant from the Labour Studies Programme at McMaster University. I would like to thank participants in the McMaster Labour Lunch for comments on an earlier version of this work. I would also like to thank two anonymous referees for helpful suggestions. Correspondence may be addressed to Stephen R. G. Jones, Department of Economics, McMaster University, Hamilton, Ontario, Canada L8S 4M4.

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idea of a Hawthorne effect and then examine the evidence from the Hawthorne studies themselves, with allowance for a wide range of direct experimental and incidental variables, for the role of replacement workers during the five years of study, and for potential interdependence of the workers' output levels. Whether the Hawthorne effect is defined in a narrow or a broad sense, the conclusion is the same: the original Hawthorne studies contain little clear evidence of a Hawthorne effect.

II. INFLUENCE OF THE HAWTHORNE EFFECT

Writing in the widely influential Festinger and Katz (1953) volume, John French noted that a potential merit of field experiments over laboratory experiments is that the former can avoid or minimize artificiality and thereby overcome the problem of generalizing results from the laboratory to real-life situations. French continued:

> That this is not always the case, however, is well illustrated in the famous Hawthorne experiment. From a methodological point of view, the most interesting finding was what we might call the "Hawthorne effect." In order to manipulate more precisely the physical factors affecting production, the experimenters had set up a special experimental room for a small group of girls who were wiring relays. This wiring was separated from the rest of the factory, and the girls working in it received special attention from both outside experimenters and the management of the plant. Careful studies of this wiring group showed *marked increases in production which were related only to the special social position and social treatment they received.* [1953, pp. 100–101; emphasis added]

However, French gave no reference for these careful studies and only cited the original Roethlisberger and Dickson (1939) account—that does not contain any statistical analysis beyond bivariate correlations—so that the exact foundation for his statement is unclear.

Notwithstanding this, there can be little doubt that the Hawthorne effect has entered into the literature as a key fact to be reckoned with in many practical contexts. William Whyte, for example, writes of the Hawthorne studies, "As experiment followed experiment . . . it became abundantly clear that physical changes were not the key. As in the earlier experiment, output did shoot ahead where conditions were changed, but so did output shoot ahead where no changes had been made. . . . The researchers came to the conclusion that output shot up in both groups because in both groups the workers' participation had been solicited and this involvement, clearly, was more important than physical perquisites. The workers were a social system; the system was informal but what it really determined was the worker's attitude toward his job" (1956, p. 34). Similarly, Ruch and Zimbardo write, "*No matter what the research-*

ers did, productivity went up. Even when work conditions were made worse than they were originally, the women worked harder and more efficiently" (1971, p. 372; emphasis added).

More recently, Blalock and Blalock similarly report that, "*Each time a change was made, worker productivity increased,* leaving the impression that each change had a progressive effect. As a final check, the experimenters returned to the original unfavorable conditions of poor lighting, no rest pauses, and no incentive system. Seemingly perversely, productivity continued to rise" (1982, p. 72; emphasis added). Some qualification is provided by Elmes, Kantowitz, and Roediger: "With few exceptions, no matter what changes were made—whether there were many or few rest periods, whether the work day was made shorter or longer, et cetera—the women tended to produce more and more telephone relays. . . . The workers knew that the experimenters expected the changes in working conditions to affect them, so they did" (1985, p. 225). As these sources reveal, the received wisdom is that there were Hawthorne effects at the Hawthorne Plant.[2]

In several areas of active research, the Hawthorne effect has come to be a significant preoccupation for many scholars. In education research in particular, since Desmond Cook's (1962) classic work and the subsequent contribution of Bracht and Glass (1968), there have been many studies that attempt to deal with Hawthorne effects. In their recent survey of 86 such studies, Adair, Sharpe, and Huynh (1989*a*) give a mixed overall assessment of this work: their metanalysis gives no grounds for a Hawthorne versus no-treatment control difference.[3] Yet the view that the original Hawthorne studies provide a firm foundation for the idea of a Hawthorne effect remains firmly entrenched in the literature. As Adair recently wrote, "The investigators began by changing the method of determining wages. During the experiment the investigators also manipulated, on different occasions and sometimes concurrently, the length and timing of rest periods, the length of the work week, the length of the work day, and whether or not the company provided lunch and/or beverage. *Productivity seemed to increase regardless of the manipulation introduced* (1984, p. 336; emphasis added).[4]

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[2] In response to a referee, who fears that past errors may be reinforced through unwitting repetition, I should point out the major inaccuracies in the preceding quotations. The best single reference for careful corrections to these and other accounts is Gillespie's (1991) recent book *Manufacturing Knowledge,* in which a detailed archival study corrects many common misconceptions about events at Hawthorne, in addition to presenting a fascinating argument about the stabilization of conflicting interpretations in the process of "manufacturing knowledge." The women in the relay assembly room were not actually "wiring" relays, as French said (Gillespie 1991, chap. 2). Whyte's claim that physical factors were not the key is in part at odds with the evidence (see Gillespie [1991, pp. 38–48] on the illumination tests and Jones [1990] on physical effects on productivity in the relay room itself). Ruch and Zimbardo's claim that productivity went up no matter what the researchers did is incorrect; also incorrect are Blalock and Blalock's similar statement and the qualified description given by Elmes et al. (Gillespie 1991, table 4, p. 57).

[3] See also Adair, Sharpe, and Huynh (1989*b*) for further discussion of placebo, Hawthorne, and other controls in experimental research.

Finally, I must mention the wider influence of the Hawthorne experiments and the received wisdom of Hawthorne effects. In a leading case of popular business writing from the 1980s, for example, Peters and Waterman write, "For us, the very important message of the research . . . is that it is *attention to employees,* not work conditions per se, that has the dominant impact on productivity. (Many of our best companies, one friend observed, seem to reduce management to merely creating 'an endless stream of Hawthorne effects')" (1982, pp. 5–6).

III. THE HAWTHORNE EXPERIMENTS

A. The Hawthorne Studies and the Hawthorne Effect

The Hawthorne experiments were conducted at the Hawthorne Plant of the Western Electric Company in the late 1920s and early 1930s and involved a variety of different studies of workplace behavior. The illumination experiments, which initially sought to establish a physiological relationship between intensity of illumination and workplace efficiency, predated the main Hawthorne studies themselves and showed that, in some instances, workers could maintain efficiency even under very low intensity of light, a finding that the researchers viewed as quite anomalous.[5] Indeed, Roethlisberger and Dickson (1939, p. 17) even mention a sequence of experiments in which an electrician pretended to alter lighting intensity—simply replacing bulbs by others of equal power—after which the women involved commented explicitly on their preference for the old or new illumination intensity. Overall, they concluded, these experiments "failed to answer the specific question of the relation between illumination and efficiency," but, nonetheless, "they provided great stimulus for more research in the field of human relations" (Roethlisberger and Dickson 1939, p. 18).[6]

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[4] As with the earlier textbook quotations, Adair's claim that output increased no matter what the experimental change is incorrect (see, e.g., Gillespie 1991, table 4, p. 57).

[5] Roethlisberger and Dickson (1939, p. 17) cite the case of two "capable and willing operators" who maintained their productive efficiency even when the amount of light was cut to 0.06 of a footcandle, "an amount of light approximately equal to that on an ordinary moonlight night."

[6] Gillespie (1991, pp. 38–48) discusses the industrial illumination tests in some detail and shows that, following Roethlisberger and Dickson's (1939) account, the standard

For quantitative research, a more valuable aspect of the Hawthorne studies was the Relay Assembly Test Room studies, in which five women worked in a technologically independent way producing electrical relays.[7] This study covered 270 weeks, from April 1927 to June 1932, and involved 24 different "experimental periods" of varying length in which working conditions were changed, sometimes by conscious design of the researchers and sometimes out of practical expediency in the face of the declining 1930s economy. Table 1 details the timing of these periods and lists the principal changes made in each.

There is little direct evidence of a Hawthorne effect in the original research. Perhaps the clearest statement of what constitutes the effect was made by Roethlisberger and Dickson in reviewing the changes that occurred in the first seven periods of the Relay Assembly Test Room experiment: "There were those changes introduced by the investigators in the form of experimental conditions; these were well noted and recorded. There was another type of change, however, of which the investigators were not so consciously aware. This was manifested in two ways; first, in a gradual change in social interrelations among the operators themselves . . . ; secondly, in a change in the relation between the operators and their supervisors. . . . *From [the] attempt to set the proper conditions for the experiment, there arose indirectly a change in human relations which came to be of great significance in the next stage of the experiment*" (1939, pp. 58–59; emphasis added). Together with the primarily anecdotal evidence from the illumination studies, these observations, more than anything else in the major account of the Hawthorne

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interpretation of the early lighting studies tends to "exaggerate the initial ignorance of the experimenters and misrepresent the process of discovery" (p. 47).

[7] There were actually eight women in the study overall, counting three "replacement" workers, who appear here, as elsewhere, labeled 1A, 2A, and 5A. In fact, workers 1 and 2 were replacements, since 1A and 2A started out in the relay assembly room group but were removed near the start of period 8. It should be pointed out that there is considerable disagreement in the literature over the appropriate interpretation of this replacement of 1A and 2A by 1 and 2. Franke and Kaul (1978), e.g., used it to construct an indicator of "managerial discipline," though such a view was subsequently disputed by responses to their work (Wardwell 1979, pp. 859–60); Schlaifer 1980, pp. 998–99, 1004–5). In the original log for January 25, 1928, the day the replacement occurred, Hibarger, a Hawthorne piece-rate analyst who was the supervisor/experimenter in the Relay Assembly Test Room, stressed that animosity had built up between workers 1A and 2A and some of the other women in the group, though other evidence suggests that these workers' decision to restrict output was a more important factor leading to their replacement (Gillespie 1991, pp. 61–63). The weeks when they "replaced" workers 1 and 2, and the two occasions when worker 5A replaced worker 5, are detailed in table 1 above.

TABLE 1

Main Experimental Changes in Hawthorne Relay Assembly Test Room

Period	Weeks	Change
1	1–3	None; still in main plant
2	4–7	Move to test room; ↑ in VRest (from 0)
3	8–15	Small group incentive introduced, VRest ↑
4	16–20	Sched stop, Sched time (2 × 5 mins.); VRest ↓
5	21–24	Sched time ↑ (2 × 10 mins.), VRest ↕
6	25–28	Sched stop ↑, Sched time ↑ (6 × 5 mins.), VRest ↓
7	29–39	VRest ↑, Sched stop ↓, Sched time ↓ (15 and 10 mins.)
8	40–46	Raw materials problems, VRest ↓; 1 and 2 replace 1A and 2A
9	47–50	VRest ↓ (mostly), shorter day (1/2 hour less, 4:00 stop)
10	51–62	VRest ↑, return to full working day (4:30 stop)
11	63–71	VRest ↕, five-day week (from five and one-half days)
12	72–83	Days/week ↑, Sched stop and Sched time ↓ (to 0), VRest ↑
13	84–114	Sched stop and Sched time ↑ (15 and 10 mins.), VRest ↓
14	115–23	VRest ↓ (mostly); 5A replaces 5 in week 120
15	124–54	VRest ↓ (slightly)
16	155–58	Raw materials problems end, VRest ↕, seating change
17	159–83	Days/week ↓, VRest ↕; 5 returns to replace 5A
18	184–98	Days/week ↓, VRest ↑ (slightly)
19	199–210	VRest ↕, seating change (back)
20	211–38	VRest ↓
21	239–41	Days/week ↓, VRest ↓
22	242–50	VRest ↓
23	251–53	VRest ↓ (to 0), days/week ↓
24	254–70	Sched stop and Sched time ↓ (to 0), days/week ↓; 5A replaces 5

Note.—Changes relative to regular department include a smaller room, more uniform lighting, fans for use in summer, one layout operator for five women rather than for six or seven, a new chute mechanism, fewer relay types to assemble (in general), a new repairs procedure, a test room observer who "took over some of the supervisory functions" (Roethlisberger and Dickson 1939, p. 39), periodic physical examinations, and the freedom to talk more freely while working.

Legend.—An upward arrow (↑) = increase; a downward arrow (↓) = decrease; a dual-direction arrow (↕) = increases and decreases within the same time period; VRest = voluntary rest time; Sched stop = scheduled rest stops; Sched time = scheduled rest time; Days/week = working days per week.

experiments, seem consistent with the interpretation of the Hawthorne effect proposed by French.[8]

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[8] The only additional evidence regarding Hawthorne effects in the early accounts is given in Whitehead's (1938) meticulous study. In his discussion of Graph H-42 (in vol. 2) that charts weekly group output together with a number of the main experimental variables over the five year study period, Whitehead asks, "Is there any clear indication that this behavior is very markedly influenced by the changes in experimental periods?" (vol. 1, p. 41). His answer is that, except for period 12, when rest pauses were eliminated and group output over the period as a whole fell "about 4% below its trend at that time, . . . changes in output rate do not correspond in time with changes in experimental periods for the most part" (vol. 1, p. 42). He is, however,

B. Interpretation of the Hawthorne Effect for Empirical Investigation

There are three potential views of the usefulness of the original Hawthorne data for studying the Hawthorne effect. The most restrictive of these holds that the general experimental effect is the key. Particular factors relating to the isolation of the group from the rest of the plant, especially the "special attention" accorded to its members—which may be a euphemism for less authoritarian but closer supervision—are critical, in this view, and since there was only one such test room, with no control study, the Hawthorne studies yield essentially one observation. On this reading, the original data can at best be suggestive but can never resolve the issue of Hawthorne effects.

A less restrictive view, and one that is clearly consistent with the standard interpretation placed on the Hawthorne data by many of the authors cited above, is that the Hawthorne effect is related to the explicit changes made at the start of experimental periods, changes that were, of course, known to the workers concerned. In this light, one can look for a common effect on output associated with *any* such experimental period changes, making allowance for other effects from any experimental or environmental variables.

Finally, the broadest interpretation is that the Hawthorne effect might reasonably be related to *any* changes in experimental conditions, not just to the major changes made at the beginning of each experimental period. Thus, one might also expect a change induced by a Hawthorne effect to result from a within-experimental-period change in working conditions. As with the second view, the research strategy that arises from the broad interpretation is to look for some common effect at times when any experimental variable changes, controlling for the experimental variables themselves. This article investigates the consequences of the second and third of these approaches, which are termed the "narrow" and the "broad" definition of an experimental change, respectively.

C. Data Description and Output Levels around Periods of Experimental Change

My investigation begins with simple data description and then moves to more involved statistical models of output determination in the Relay Assembly Test Room. Summary statistics on all the data used in this

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quick to qualify this finding, noting that "a negative finding is never very convincing; it is always possible that some phenomenon remains undiscovered through a defect in the investigation" (vol. 1, p. 43). Whether my largely negative findings, which corroborate Whitehead's graphical analysis but are at odds with the established reading of Hawthorne effects, are convincing is, of course, for the reader to assess.

study are given in table 2.[9] The output variables for the eight women studied in the Relay Assembly Test Room, including the three replacements, are measured as the mean number of (standardized) relays produced per hour worked, averaged over a weekly measurement interval. Repair time and voluntary rest time are measured in minutes per week as is the general (non-person-specific) scheduled rest time. Small group pay is a dummy variable reflecting the introduction of a group piece rate based on the output of the five workers under study as opposed to pay based on the average output of the plant as a whole: this change was made in week 8 (see table 1 above). Other experimental variables with changes detailed in table 1 and summarized in table 2, were working days per week (hereafter days/week), the use of replacement workers, a change in the seating plan at the workbench, and the number of scheduled rest stops.[10] A change in any of these variables in a week will count as a *broad* experimental change, whereas a *narrow* change is limited to those groups of simultaneous changes listed in table 1. Overall, of the 270 weeks, 24 weeks had a narrow experimental change, while 88 weeks had a broad change.

The first set of results comes from a comparison of individual mean output levels overall—in the week preceding an experimental change, in the week of the change itself, and in subsequent weeks. This is an "unconditional" interpretation of the Hawthorne effect, as suggested by several of the authorities cited above. These figures are given in table 3,[11] and since different workers were present for differing periods and since some periods are dropped to avoid double counting, the sample size upon which the mean is based is given in each case.[12] For each worker, the results display little evidence of a simple Hawthorne effect. Relative to the week preceding the change, mean output falls slightly in the week

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[9] The data set employed in this study is based on pioneering work by Franke and Kaul (1978) and Franke (1979, 1980), which I have extended using data from Whitehead (1938) (see Jones 1990). It is available on request to readers who supply a DOS-formatted diskette.

[10] I have given further information about the Relay Assembly Test Room and the variables constructed for this research elsewhere (Jones 1990).

[11] A minor complication in this calculation is that, especially for the broad definition of an experimental change, a given week might be "double counted" as being, say, two weeks after one change *and* one week before the next change, which would tend to make interpretation of the output levels in such a week difficult. I have accordingly omitted all such double counting from the averages recorded in table 3 and from the graphs of output changes in figs. 1 and 2. In doing this, priority is given first to the week preceding the change, then to the week of the change, and so on.

[12] The sample size rises moving from the week preceding a change to the week of a change simply because I count the first week of the relay room study as a "change" and there is no output data for the preceding week.

TABLE 2

SUMMARY STATISTICS FOR ALL VARIABLES

Variable*	N	Mean	SD	Minimum	Maximum
Output 1	218	69.5	4.5	60.1	80.8
Output 2	216	73.0	5.0	59.9	82.4
Output 3	256	63.2	4.6	43.7	72.9
Output 4	256	67.2	7.1	48.1	82.4
Output 5	204	59.8	5.7	47.7	69.8
Output 1A	39	51.6	3.1	44.4	56.4
Output 2A	38	52.0	3.2	45.6	56.8
Output 5A	52	55.7	5.4	43.4	64.2
Days/week	257	5.0	.59	3.0	5.5
Unemployment rate	270	8.8	6.6	3.2	23.6
Repair time 1	231	27.7	15.0	0	59.9
Repair time 2	231	22.5	11.1	0	42.7
Repair time 3	270	19.6	11.8	0	40.7
Repair time 4	270	11.6	7.9	0	30
Repair time 5	214	18.4	12.6	0	69
Repair time 1A	39	20.5	11.6	0	38
Repair time 2A	39	18.9	12.1	0	37.4
Repair time 5A	56	40.0	27.6	0	69
Voluntary rest 1	231	4.5	3.3	0	15
Voluntary rest 2	231	6.7	4.1	0	20.6
Voluntary rest 3	270	6.1	3.3	0	13
Voluntary rest 4	270	7.4	4.1	0	16.5
Voluntary rest 5	214	5.8	2.0	0	8.9
Voluntary rest 1A	39	9.2	4.6	0	13.7
Voluntary rest 2A	39	8.2	5.6	0	17
Voluntary rest 5A	56	3.3	2.1	0	6.4
Raw material problems	270	.15	.36	0	1
Small-group pay	270	.97	.16	0	1
No repair time reports	270	.14	.35	0	1
No voluntary rest reports	270	.09	.28	0	1
No scheduled rest reports	270	.06	.24	0	1
Scheduled rest stops	270	1.7	.91	0	6
Scheduled rest time	270	20.6	9.4	0	30
1A/2A replacement	270	.86	.35	0	1
5A replacement	270	.21	.41	0	1
Seating change	270	.16	.37	0	1
No medical reports	270	.42	.49	0	1
Worker 1 ill	270	.07	.26	0	1
Worker 2 ill	270	.06	.23	0	1
Worker 3 ill	270	.05	.22	0	1
Worker 4 ill	270	.05	.21	0	1
Worker 5 ill	270	.02	.15	0	1
Heat wave	270	.04	.21	0	1
Cold wave	270	.03	.18	0	1
Narrow experimental change	270	.09	.29	0	1
Broad experimental change	270	.33	.47	0	1

* Numbers following variables represent workers.

TABLE 3

Mean Output Levels in Weeks around Experimental Changes

	*q*1	*q*2	*q*3	*q*4	*q*5	*q*1A	*q*2A	*q*5A
Overall	69.5	73.0	63.2	67.2	59.8	51.6	52.0	55.7
N	218	216	256	256	204	39	38	52
Narrow definition (Δ):								
Δ(−1)	70.3	72.8	62.1	64.8	58.7	51.4	52.0	57.3
N	16	16	23	23	19	7	7	3
Δ(0)	69.6	71.3	61.3	63.9	58.4	52.3	53.7	56.1
N	17	16	24	24	21	7	6	3
Δ(1)	68.9	71.6	61.5	64.0	58.0	51.2	50.8	56.8
N	17	16	24	24	20	7	7	4
Δ(2)	67.4	71.1	61.0	63.5	57.1	50.9	51.1	58.5
N	14	14	19	19	17	6	6	3
Δ(3)	68.6	71.7	62.1	64.7	59.1	51.9	52.6	53.3
N	12	12	15	15	13	3	3	2
Δ(4)	68.3	71.8	62.7	66.6	59.7	51.1	53.2	56.6
N	12	12	14	14	12	2	2	2
Broad definition (Δ*):								
Δ*(−1)	69.8	73.3	62.4	66.6	59.0	51.0	51.7	57.0
N	63	63	81	80	63	18	18	17
Δ*(0)	69.2	72.4	62.7	65.9	59.1	52.1	52.2	56.3
N	34	33	44	44	35	10	9	9
Δ*(1)	69.5	72.9	63.3	66.3	60.2	51.7	52.0	53.8
N	27	26	33	33	28	6	6	5
Δ*(2)	69.1	72.6	63.3	66.4	59.1	52.9	53.0	51.9
N	19	19	23	24	20	5	5	4
Δ*(3)	70.4	73.9	64.4	69.1	61.6	...	...	55.7
N	14	14	14	14	11			3
Δ*(4)	69.0	72.4	64.3	68.3	61.1	...	...	52.8
N	11	11	11	11	9			2

Note:—Here, Δ(*n*) denotes the observation *n* weeks after a narrow experimental change; Δ*(*n*) is used analogously for the broad definition. Also, *N* gives the sample size for each mean. Ellipses indicate that no observations were available for this worker for this week. Double counting of weeks is avoided; see text for explanation.

of the change for each of the core group of workers on the narrow definition, but the pattern is mixed for the changes within the broad category. In both cases, however, the changes are numerically very small. For subsequent weeks, there is similarly no clear pattern, and the movement is once again slight. At conventional significance levels, one certainly could not reject the hypothesis that output had the same mean in each of the weeks surrounding the experimental change.

More generally, the pattern of output levels at *all* periods after an experimental change (and before the next such change) is graphed for the narrow and broad cases in figures 1 and 2, respectively. As can be seen

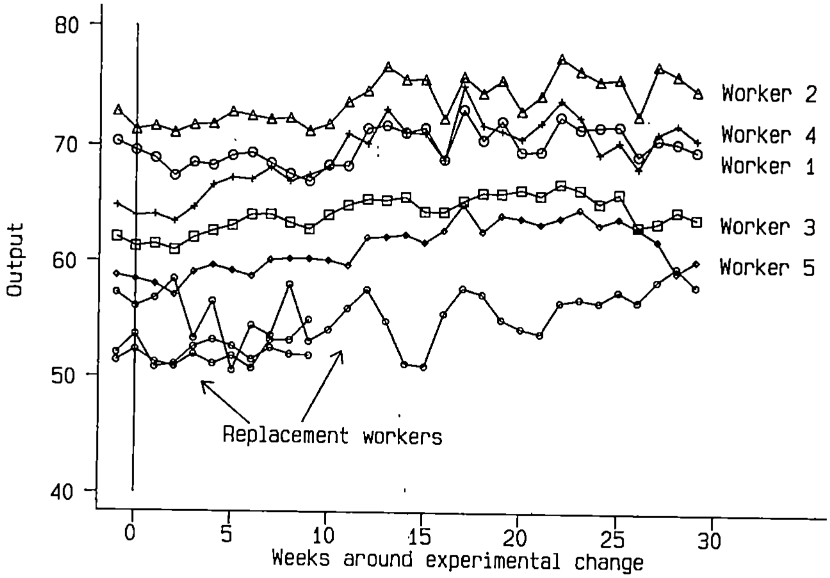

FIG. 1.—Mean output levels (narrow definition of experimental change)

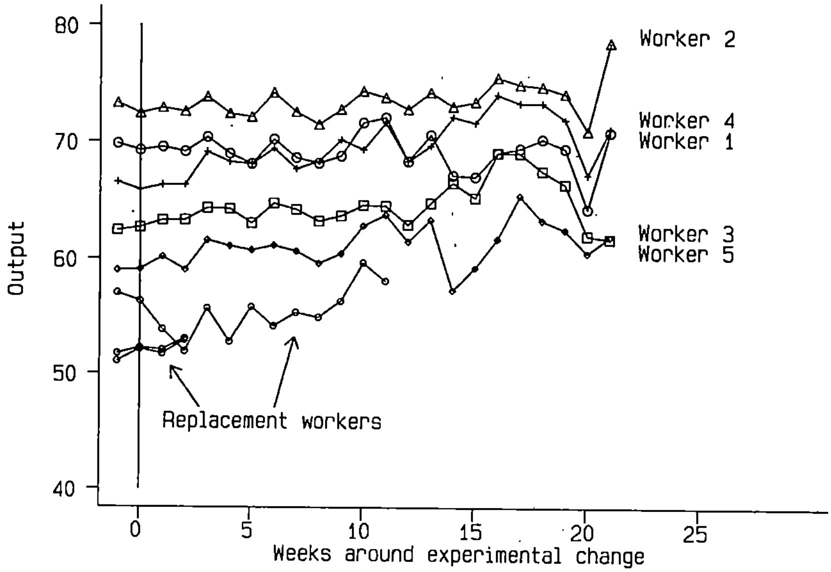

FIG. 2.—Mean output levels (broad definition of experimental change)

from figure 1, although there is considerable diversity in the mean output levels of the workers involved, especially including the three replacement workers who had particularly low average outputs, there is a clear trend only for worker 4, and that trend begins only in the fifth week after a narrow experimental change. For the broad definition in figure 2, worker 4 again has the strongest upward pattern in her mean output levels although, as with figure 1, in no case is there a significant regularity in these mean output data.

D. Models of Worker Behavior and the Hawthorne Effect

One potential problem with the results discussed above is that the experimental changes themselves, which are likely to have direct effects, may obscure any Hawthorne effects that might be present. To control for this, I next present the results of estimating the determinants of individual output levels, controlling for the direct effects of experimental changes.[13]

Two sets of specifications, or models, are employed. The first treats each worker's output as independent of the others, although potentially influenced by a set of common variables, and amounts to the estimation of

$$q_i = X_i b + Z c + u_i, \tag{1}$$

where X_i contains the person-specific variables from table 2, Z represents the variables common to all the workers, and u_i is a person-specific error term. An alternative approach, or model 2, allows for a sluggish response to experimental changes and a potential interdependence in the levels of output of the members of the working group by postulating

$$q_i = \lambda q_i(-1) + X_i b + Z c + \alpha \mathbf{q}_{-\mathbf{i}} + u_i, \tag{2}$$

where $q_i(-1)$ is the own lagged value of output and $\mathbf{q}_{-\mathbf{i}}$ is a vector of contemporaneous output variables for the other members of the working group. This second model follows one I developed earlier (Jones 1990).

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[13] It should be noted that, while simple theoretical regression techniques were known by the time of the Hawthorne studies (Stigler 1986), instrumental variable methods (as are used for some of the models below) came to fruition after the Second World War. Also, it should be stressed that such techniques as were known at the time of the Hawthorne studies did not have wide currency among empirical researchers. Computing limitations were also undoubtedly severe, so that it is reasonable to conclude that the present statistical methods were not readily available at the time of the Hawthorne studies themselves. Nonetheless, there is clear value for present-day researchers to use such multivariate techniques in a reanalysis of the original data, essentially since such techniques permit the researcher simultaneously to control for a large number of factors that might otherwise be confounded with the Hawthorne effect.

In model 1, a single equation estimate is appropriate, while for model 2 the endogeneity of other workers' concurrent output levels means that an instrumental variable estimate is preferred: the instruments I use are lagged output levels and person-specific variables, such as voluntary rest or repair time. Finally, in view of the role of replacement workers in the Relay Assembly Test Room, I employed a variant of model 1 in which the left-hand-side variable was the output of worker 1A *or* worker 1 (and similarly for workers 2A and 2, 5A and 5), with all of the explanatory variables being interacted with the dummy variable representing the particular replacement in question. I refer to this as the "interacted model."

To assess the presence of a Hawthorne effect in the context of these models, equations (1) and (2) were estimated for each worker and included a dummy variable with the value of 1 in each week of experimental change (and 0 when no change occurred). Sample sizes varied for model 1 and for the interacted model depending on the length of time the worker spent in the group, although model 2 was always estimated on the 159 weeks when all five core group members were present and had been present in the preceding week. The estimated coefficients on these dummy variables are given in table 4.[14] For model 1, there is little evidence of a common (Hawthorne) effect in either the narrow or broad case; the pattern of signs is mixed with the only significant coefficients suggesting a negative Hawthorne effect, controlling for the other variables. For the interacted model, which only matters for the three workers who were replaced at some point in the five-year study, the results are equally checkered. Similarly, when allowance is made for partial adjustment and potential interdependence of the worker's output levels, the dummy variables representing an experimental change are uniformly insignificant. As with the unconditional results of table 3, there seems to be essentially no evidence of a Hawthorne effect.

E. Patterns of Residuals

The final diagnostic check I employ is to examine the pattern of residuals from equations (1) and (2) when estimated excluding the experimental change dummy variables. The presence of a marked pattern of such residuals in periods following a change could suggest some type of Hawthorne effect, perhaps of a form too subtle to be adequately captured by a single contemporaneous dummy variable. Accordingly, three of the specifications in table 4 (I am excluding the interacted specification) were reestimated and the residuals plotted. Figures 3 and 4 illustrate these results in which, for each number of weeks after an experimental change,

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[14] Full results for these estimated equations are available on request from the author.

TABLE 4

ESTIMATES OF EXPERIMENTAL CHANGE EFFECTS

	$q1$	$q2$	$q3$	$q4$	$q5$
Narrow Definition:					
OLS specification 1	.93	−.74	−.09	−.72	.46
	(.73)	(.83)	(.49)	(.77)	(.58)
OLS interacted model...	1.00$^+$	−.30	...	...	.37
	(.60)	(.73)			(.61)
OLS specification 2	.52	−.95$^+$	.18	.56	.21
	(.59)	(.53)	(.51)	(.77)	(.56)
IV specification 2.........	.51	−.89	.29	.51	.59
	(.63)	(.56)	(.58)	(.81)	(.63)
Broad definition:					
OLS specification 1	.14	−.94$^+$	−.09	−.32	−.91*
	(.47)	(.52)	(.34)	(.53)	(.40)
OLS interacted model...	.28	−.65	...	...	−.67
	(.42)	(.47)			(.42)
OLS specification 2	.43	−.49	.09	.19	−.50
	(.39)	(.35)	(.34)	(.51)	(.37)
IV specification 2.........	.37	−.40	.06	.14	−.36
	(.41)	(.37)	(.36)	(.52)	(.39)

NOTE.—Each entry gives the estimated coefficient on a dummy variable that takes the value "1" in each week of experimental change (0 otherwise) entered into eqq. (1) or (2). Control variables for specification 1 for worker i ($i = 1, \ldots, 5$) were repair time of i, voluntary rest time of i, a dummy variable indicating if worker i was ill (but worked in the week in question), days per week, a dummy for raw material problems, scheduled rest time, scheduled rest stops, a dummy for the seating change, dummy variables for a heat wave or a cold wave (as reported for the week by the *Chicago Tribune*), and dummy variables for the absence of data on scheduled rest stops and for the absence of medical reports. In addition, replacement worker dummies were included for 5/5A (for $i = 1, \ldots, 4$) and for 1/1A and 2/2A (for $i = 3,4,5$). Sample sizes for the OLS specification 1 were, respectively, 218, 216, 256, 256, and 204. For the interacted model, these controls were all interacted with the replacement dummy variable, as well as entering the equation themselves. Sample sizes were 257, 254, and 225, respectively. For specification 2, additional regressors were the lagged dependent variable and the contemporaneous output levels of the other four workers. In this case, all estimates are for the consistent core sample of 159 weeks as detailed in the text. Numbers in parentheses are SEs.

$^+P \geq .10$.

$^*P \geq .05$.

each person's residual is averaged.[15] In addition, for each such week, the average is computed across individuals (weighted by the number of such weeks that the individual was in the sample). The circles in these figures are the individual mean residuals while the triangles, joined by a line, are the (weighted) mean of these individual mean residuals.

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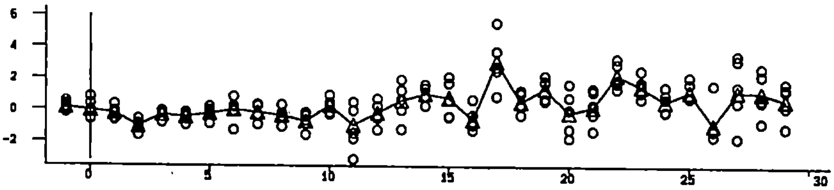
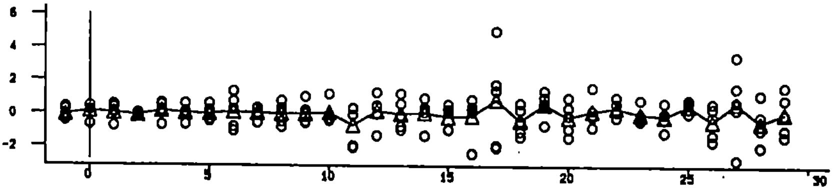
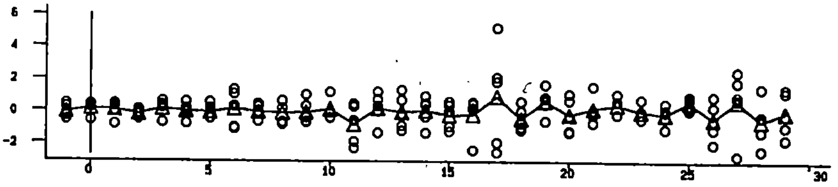
[15] These calculations avoid all double counting of periods in the same way as for table 3 and figs. 1 and 2. See n. 11 above. In addition, since the models are only estimated for workers 1–5, and not for the replacements, the associated residuals are graphed only for the five core members of the group.



a) OLS Specification 1



b) OLS Specification 2



c) IV Specification

FIG. 3.—Residuals in weeks around experimental change (narrow definition of experimental change).

Figure 3 shows the results for the three specifications viewed through the narrow definition of an experimental change. Under OLS specification 1, the pattern seems to be of slightly negative residuals shortly after a change; these residuals are offset by some larger positive effects beyond 15 weeks after the experimental change. Of course, these latter effects are based on very small samples (since most experimental changes were followed by another change within 15 weeks) and are not significant.[16] In addition, it seems hard to construct a convincing scenario in which a Hawthorne effect would take over three months to develop. For the two other specifications, the pattern of residuals is still flatter, providing no evidence of any systematic error that could suggest a Hawthorne effect.

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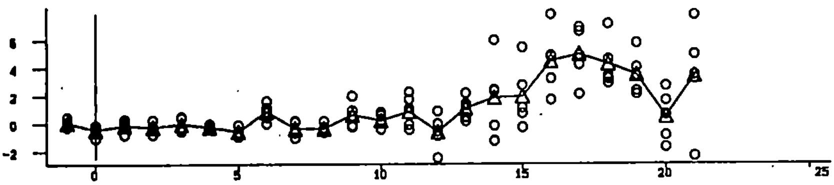
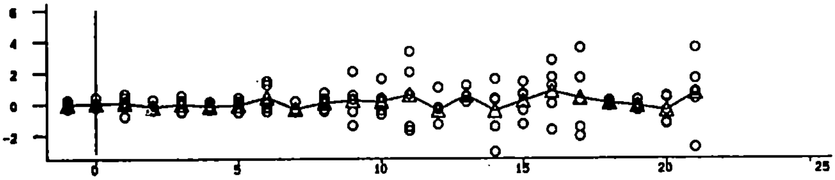
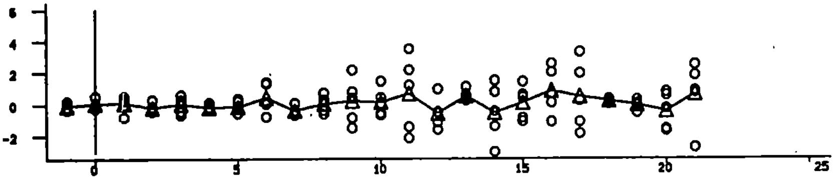
[16] For the narrow definition, there are four weeks when workers 1–4 record output levels 15 weeks after an experimental change; for worker 5, there are only three such weeks.



a) OLS Specification 1



b) OLS Specification 2



c) IV Specification

FIG. 4.—Residuals in weeks around experimental change (broad definition of experimental change).

The residuals in figure 4, which covers the three specifications for the broad definition of an experimental change, are very similar when averaged by week after the occurrence of an experimental change. Although the mean error is positive in OLS specification 1 from 15–20 weeks after a change, this is based on very small samples and is insignificant.[17] For the other two specifications, the point estimates seem to fluctuate very closely around zero, with no clear departure even many weeks after an experimental change when sample sizes become quite small.

IV. CONCLUSION

In this article, I have tested the evidence for a Hawthorne effect by examining the quantitative data on individual output levels collected over five years during the original Hawthorne studies. Contrary to the conventional wisdom in much research and teaching, I found essentially no evidence of Hawthorne effects, either unconditionally or with allowance for direct effects of the experimental variables themselves. My result appears to be robust across a wide variety of specifications, alternative samples, and two definitions of experimental change.

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[17] In fact, for the broad definition, there is only one week when workers 1–5 record output levels 15 weeks after an experimental change.

The one remaining interpretation of the Hawthorne effect that could survive my investigation is the first one I mentioned above, namely that the whole 270-week period of study was but one experiment, and that all of the various changes introduced at the start of the study and maintained throughout were one experimental change. Since we have no data on a control group, this interpretation means that there is, in essence, only one data point. Clearly, the original Hawthorne data are not adequate to the task of assessing this interpretation of what constitutes a Hawthorne effect. However, it is important to note that established interpretations of the Hawthorne studies (e.g., French 1953; Whyte 1956; Blalock and Blalock 1982; Adair 1984) never define the Hawthorne effect in this manner. In this context, I must conclude that there is slender or no evidence of a Hawthorne effect in the Hawthorne Relay Assembly Test Room. Finally, in light of these results, I must also conclude that the Hawthorne effect is largely a construction of subsequent interpreters of the Hawthorne experiments. A fruitful line of sociological enquiry, already initiated in part by Gillespie (1991), would explore the social and historical context of the reception of the Hawthorne experiments and the process whereby the Hawthorne effect has become enshrined as received wisdom in the social sciences.

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Individual, Collective, and Systems Rationality in Work Groups: Dilemmas and Market-Type Solutions[1]

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In work teams two situations often arise: first, one can observe only the output of the work group, not the contributions of each member; second, the output of each member depends not only on her own effort but also on those of co-workers as well. The problem that arises is how to construct reward schemes. Group piece-rate schemes are often suggested as a solution. However, these are susceptible to free-rider problems. A classic solution relies on a supervisor who keeps free riders in line. This article discusses another solution where the relationship between output and reward is highly nonlinear.

I. INTRODUCTION

In formal organizations, and particularly in work teams within organizations, two situations often arise. In the first, one can observe or measure the output of the work group but not the contributions of each member.

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In the second, the output of each member depends not only on individual effort but also on the efforts of the other workers.

The problem that arises in both situations is how to construct reward or incentive schemes. In the first case, one cannot tie individual rewards to individual outputs. In the second one may do so, but the tie between individual effort and total output is blurred by the interdependencies.

Group piece rates are suggested remedies in both situations (see, e.g., Balderston 1930, p. 10; Miller and Hamblin 1963; Dessler 1984, p. 377; ILO 1984, p. 15; Hills 1987, p. 370). However, it is well known that group piece-rate schemes are susceptible to free-rider problems, as pointed out in the sociological, psychological, managerial, economics, and industrial-engineering literature (Stinchcombe and Harris 1969; Granovetter and Tilly 1988; Hechter 1987, p. 136; Marriott 1949; Lawler 1981, p. 81; Dessler 1984, p. 377; Aft 1985, pp. 241, 247; Holmström 1982; Miyazaki 1984; ILO 1984, p. 16; Sellie 1982). Under a group piece-rate scheme each worker has an incentive not to work hard, since her contribution to the group output is of order $1/n$ (where n = group size). Increased effort is reflected in increased pay only to the order of $1/n$, since all other workers also benefit from the effort of a single worker, a problem that is exacerbated with the size of the group. A group piece-rate scheme is therefore likely to lead to low effort levels, low output levels, and hence low pay for all members of the work group. In the case of interdependencies when individual outputs are observable, even an individual piece-rate scheme is susceptible to free-rider problems. The mechanism is then that each worker knows that her own output depends not only on her own effort, but also on the efforts of her co-workers. She therefore has an incentive to take it easy and let the others work hard. But again, if everyone acts according to this logic, outputs, and hence rewards, will be low.

In both situations individually rational behavior leads to collective irrationality or poor systems functioning. It is individually rational not to work hard, irrespective of the actions of the other workers. But everyone would prefer a situation in which everyone works hard to one in which no one works hard. The situation belongs to the class of problems that has become known as the micro-macro problem (see Coleman 1986).

The classic solution to the problem of team interdependencies and to free-rider problems in work groups in general was adumbrated by Stinchcombe and Harris (1969), was later fully developed by Alchian and

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the free-rider problem are taken from a larger paper that has circulated since 1988 and is now available as a working paper (Petersen 1992*b*). Direct correspondence to Trond Petersen, Walter A. Haas School of Business, 350 Barrows Hall, University of California, Berkeley, California 94720.

Demsetz (1972), and was to some extent elaborated in Jacobs (1981). This solution substitutes a market-type relationship, such as the group piece-rate scheme, with an authority relationship in which a supervisor keeps free-riders in line (see also Williamson 1985, pp. 210, 244–46; Hechter 1987, pp. 136, 141).[2]

In this article, I discuss an alternative solution to the free-rider problem, a solution that retains the market-type character of the piece-rate scheme but in which the relationship between output and reward is highly nonlinear. It is shown that a so-called target-rate scheme, either individual or group based, in which pay is high if a production target is reached and low otherwise, can solve the free-rider problem. The solution was discussed earlier by Holmström (1982) in the case when only group outputs are observable, but he did not discuss the perhaps equally important case in which team interdependencies exist. In that latter case, both an individual target-rate scheme and a group target-rate can solve the free-rider problem. I also present empirical evidence to support this claim.[3]

Besides solving free-rider problems, a market-type relationship is preferable, on economic and ethical grounds, to an authority relationship. It saves the costs of remunerating a supervisor (a point already observed by Marx [(1867) 1967, p. 521]), although it incurs the costs of measuring output at the individual or group level (a cost that is usually incurred even in the absence of an output-related scheme). From an ethical viewpoint, a market-type relationship facilitates work-group autonomy, a feature workers might find desirable: workers, rather than the supervisor, decide the pace of the work (e.g., in the British mining industries, supervisors were almost eliminated when group incentives were introduced in 1977–78; see Heery 1984).

From a more empirical point of view, group incentive schemes are used in several industries in the United States and other countries, and about half of group incentive workers in the United States are paid according to target-rate schemes (see Sec. II below). Most well known are probably the target-rate schemes frequently used in the construction and building industries, where team interdependencies are important (see, e.g., Hills 1987, p. 352). A penalty, often called liquidated damages, is imposed if a project is not finished by a certain date (see, e.g., Stinchcombe 1985, p. 161). It is unknown whether such target-rate schemes were introduced to alleviate free-rider problems or for other reasons.[4]

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[2] Alchian and Demsetz (1972) take an extreme point of view, claiming that the need to alleviate free-rider problems under team production is the cause of authority relationships and even of capitalist employment relationships in general (for a critique, see Perrow [1986, chap. 7]).

[3] Underlying this discussion is also the broader issue of the relationship between output-related reward systems and productivity, an issue that is addressed elsewhere (Petersen 1992*c*).

The remainder of the article is organized in five sections. In Section II I present some descriptive material on the prevalence of target-rate schemes and of group incentive schemes in the United States. My primary objective is to illustrate the relevance of the problem. In Section III, I develop a formal model to compare the reward schemes, piece-rate and target-rate schemes, under two scenarios: in the first, only group outputs are observable; in the second, interdependencies exist between workers. I draw four conclusions and summarize them in propositions 1 and 2. In Section IV and Section V, I present the empirical evidence, based on establishment-level data on two industries in the United States. In Section VI, I present concluding remarks.

Before proceeding to these tasks, it may be useful to explicate the methodology to be followed. In Section III, the core theoretical part of the paper, I state the baseline model, in which all actors are assumed to be egoistic. There, I show that a target-rate scheme can overcome the free-rider problems of a piece-rate scheme. As is often the case, other factors are also at play, but these are not in focus here but are addressed in a companion article (Petersen 1992*a*), where the description of the social setting is made more complex and realistic. There, I bring in three additional institutions: social rewards, altruistic preferences, and moral commitment.[5] Throughout this conceptual analysis there is a conflict between the relevance and the applicability of the ideas. In Section III, I push, as far as my data allow, the latter aspect. The ideas presented are applicable to the quantitative data available. In the companion article, the relevance aspect dominates and the relationship of the ideas to the quantitative data presented in this paper is more tangential, relying instead on more ethnographic evidence.

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[4] The substantive setting of the current research is the factory, where target rates have clear relevance. See, e.g., the case study on Nucor Corp. (Lapkin 1975). However, target-rate schemes have relevance beyond the factory. Most interestingly, they are found in Chinese population policies. Each village has a maximum number of children that can be born, and each family within the village is enjoined to have no more than one child (see Greenhalgh 1986). If the village and each family within the village stays within the centrally specified limits, rewards ensue. Otherwise the sanctions are severe. Not only are the families that bear two or more children penalized, the entire village may be punished for the adversive actions of a few. In this manner China attempts to curtail free-rider problems in population growth. The enforcement of the target relies partly on the material incentives provided but also on the social and political sanctions imposed on offenders and would-be offenders. Indian fertility policies rely on similar target-rate schemes. In each village in India, a given number of sterilizations are to be performed (see Weisman 1988).

[5] The importance of social rewards for regulating behavior under various output-related payment systems has been extensively studied in the organizational and industrial sociology literature (see, e.g., Roy 1953; Whyte 1955; Burawoy 1979).

II. THE PREVALENCE OF GROUP INCENTIVE SCHEMES

In this section I place the issues of the paper in their broader empirical context, leaving the clarification and development of the concepts to the following section. For now, the objective is to document the prevalence of group-incentive schemes among incentive workers in the United States and the distribution of incentive workers by the type of incentives, piece rates, and target rates.[6]

Table 1 gives the distribution of incentive workers in the United States within each of 21 manufacturing industries among four types of incentives: individual piece and target rates and group piece and target rates. The industries are presented in descending order by the percentage of incentive workers paid by group incentives. The data pertain to production workers and were taken from the Industry Wage Surveys conducted by the Bureau of Labor Statistics (U.S. Department of Labor), to be discussed in greater detail in Section IV.

Table 1 shows that group-incentive schemes are quite common among incentive workers but that there are considerable interindustry differences. In the cotton, footwear, and clothing industries, historically the classic piece-rate industries, from 96% to 100% of the incentive workers are paid according to individual incentives, predominantly individual piece rates. The factories in these industries, particularly in clothing and footwear, are equipped with machines that are controlled by individual operators; hence, individual incentive schemes are feasible (see, e.g., Lokiec 1966, p. 314). In contrast, in the wood household furniture, corrugated and solid fiber boxes, and meatpacking industries, 60%–70% of the incentive workers are paid according to group incentives. In these industries, individual outputs are harder to identify and team production is more widespread. By comparing columns 1 and 2 and columns 3 and 4 in table 1, we see further that target-rate schemes are more common among group-incentive workers than among individual-incentive workers. In 9 of the 18 industries using group incentives, group target rates are more common than group piece rates, whereas in only 5 of the 21 industries using individual incentives, individual target rates are more common than individual piece rates. This makes sense. Free-rider problems are most severe among group-incentive workers and group target rates may alleviate these. Observed practices reflect what one would

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[6] Similar statistics are available for Britain, Norway, and Sweden (see Petersen 1992*a*, tables 1 and 2).

TABLE 1

DISTRIBUTION (%) OF PRODUCTION WORKERS PAID BY INCENTIVES BY TYPE OF INCENTIVE SCHEME IN A SELECTION OF MANUFACTURING INDUSTRIES IN THE UNITED STATES, 1973–80

	TYPE OF INCENTIVE SCHEME					
	Individual		Group			
INDUSTRY	Piece Rate	Target Rate	Piece Rate	Target Rate	Sum	% PAID BY INCENTIVES
Meatpacking	0	30	20	50	100	10
Corrugated and solid fiber boxes	12	31	12	46	101	25
Clay sewer pipes	39	4	17	39	99	24
Wood household furniture	25	17	25	33	100	17
Candy and other confectionary	36	9	9	46	100	11
Brick and structural clay	44	6	33	17	100	18
Other pressed or blown glass	14	38	29	19	100	21
Ceramic wall and floor tile	31	22	25	22	100	32
Glass containers	9	55	0	36	100	12
Iron and steel foundries	50	15	20	15	100	21
Clay refractories	52	14	24	10	100	20
Motor vehicles parts	43	25	14	18	100	27
Nonferrous foundries	33	44	5	18	100	18
Textile dyeing and finishing	50	30	10	10	100	10
Leather tanning and finishing	65	16	16	2	99	44
Wool yarn and handwoven fabrics	56	28	8	8	100	25
Cotton and man-made fiber	93	3	0	4	100	30
Men's and boys' shirts	98	1	1	0	100	78
Men's suits and coats	99	1	0	0	100	75
Footwear	97	3	0	0	100	73
Women's hosiery	100	0	0	0	100	63

NOTE.—The numbers for the 21 industries are computed from U.S. Department of Labor (1980*a*, table 23, p. 69; 1977*c*, table 15, p. 44; 1977*f*, table 26, p. 35; 1981*c*, table 32, p. 68; 1977*b*, table 12, p. 21; 1982*c*, table 19, p. 41; 1981*b*, table 21, p. 26; 1977*f*, table 26, p. 35; 1981*b*, table 13, p. 20; 1981*a*, table 29, p. 48; 1982*c*, table 19, p. 41; 1976*a*, table 22, p. 48; 1977*e*, table 20, p. 25; 1977*g*, table 28, p. 57; 1975*a*, table 21, p. 26; 1982*d*, table 46, p. 64; 1982*d*, table 28, p. 47; 1982*a*, table 28, p. 47; 1980*b*, table 23, p. 64; 1982*b*, table 27, p. 62; 1977*d*, table 12, p. 17).

expect were these establishments concerned about free-rider problems, because on the average free-rider problems are more pervasive among group-incentive workers than among individual-incentive workers.

The last column of table 1 gives the percentages of workers paid by any type of incentive, which range from a low of 10% in meatpacking to a high of 78% in men's and boys' shirts. By comparing the last column with the sum of columns 1 and 2 we see that as the percentage paid on incentives increases, the percentage of those workers paid on individual incentives also increases, attesting indirectly to the conjecture, often found in the literature, that when individual outputs are hard to measure and assess, other forms of control may take the place of output assessment (see Hechter 1987, chap. 7). The correlation between the percentage of incentive workers in an industry and the percentage of those workers being paid by individual piece or target rates is .74.

The materials presented in table 1 attest to the relevance of concepts and theories addressing the operation of group-incentive schemes, which were shown to be important in the United States, and the operation of target-rate schemes as opposed to traditional piece-rate schemes.[7]

III. INDIVIDUAL AND COLLECTIVE RATIONALITY: PIECE VERSUS TARGET RATES

In this section I show that a target-rate scheme may overcome the free-rider problems of a piece-rate scheme in the two situations described at the beginning of the paper: when only group outputs are observable and when there are team interdependencies. The two situations are discussed separately, starting with the latter. I follow a four-step procedure to first define the production technology and the preferences of workers. Then, I derive the collectively rational solution. Third, I characterize individually rational behavior under a piece-rate scheme and under a target-rate scheme. Finally, I compare the individually and collectively rational solutions, establishing whether the reward schemes can align the two interests, individual and collective.

A. Interdependencies between Workers

The interdependencies considered take the following form: the output of a worker is identifiable and measurable, but it depends not only on her own effort but also on the efforts of the other workers. What worker i

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[7] For non-Western societies, such a theory may be even more relevant. In China, group incentives have been in almost universal use (see Walder 1986, chap. 3; and for Japan see Dore [1973, chap. 3]).

produces depends on how hard she works, but it also depends on how hard her co-workers work, and vice versa. In Thompson's terminology (1967, pp. 54–55), the interdependence is reciprocal. Such interdependence is found in research groups, on sports teams (Keidel 1984), and in a variety of jobs in the manufacturing industries (Stieber 1959, pp. 29, 216–17; Dore 1973, p. 113; Kornblum 1974, pp. 37–43; Frank 1985, pp. 58, 76; Gartman 1986, pp. 176, 217; Hills 1987, pp. 352, 370; Montgomery 1987, pp. 12, 29, 44; Jones 1990).

To formalize the concept of reciprocal interdependence, let x_i and a_i denote the output and effort levels of worker i. Let $\mathbf{a}_{-\mathbf{i}}$ denote the effort levels of all the other workers, where $\mathbf{a}_{-\mathbf{i}}$ is a vector of dimension $n - 1$ and n is the size of the work group. Worker i produces output x_i according to a function

$$x_i = f(a_i, \mathbf{a}_{-\mathbf{i}}), \tag{1a}$$

for each i, where

$$f_1 \equiv \partial x_i / \partial a_i > 0, \qquad f_{11} \equiv \partial^2 x_i / \partial a_i^2 < 0, \tag{1b}$$

$$f_2 \equiv \partial x_i / \partial a_j > 0, \qquad f_{22} \equiv \partial^2 x_i / \partial a_j^2 < 0, \quad \text{for } j \neq i, \tag{1c}$$

$$f_{12} \equiv \partial^2 x_i / \partial a_i \partial a_j > 0, \quad \text{for } j \neq i, \tag{1d}$$

$$f_1 > f_2 \qquad \text{when } a_i = a \text{ for all } i. \tag{1e}$$

The first derivatives of $f(\cdot)$ formalize the idea that output x_i depends not only on a_i but also on $\mathbf{a}_{-\mathbf{i}}$, that is, on how hard the other workers work. The second derivatives rule out some forms of increasing returns to scale with respect to effort. The last condition, $f_1 > f_2$, says that worker i has more control over her own output than any single other worker has; without this condition the output of worker i could not be meaningfully observed and measured.[8]

The worker's preferences are over the effort expended and the wage earned, as follows:

$$U(a_i, w_i) = P(w_i) - V(a_i),$$

with (2)

$$P' > 0, P'' < 0, V' > 0, V'' > 0,$$

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[8] Some further restrictions may need to be imposed on the technology in order to obtain a well-defined solution to the problem. These restrictions are not investigated here.

where w_i is the wage earned and P' and P'' denote the first and second derivatives of P with respect to w_i (and similarly for V with respect to a_i). Equation (2) says that preferences increase with the wage at a decreasing rate and that preferences decrease with effort at an increasing rate. The more money one already has, the less desirable an extra dollar becomes, and the harder one already works, the harder it becomes to expend even more effort, all other things constant.

Before proceeding to the analysis proper I shall give an account of the behavioral concept underlying the analysis here. First, the model does not distinguish between individuals of different types: workers are identical in productive capabilities and in type of position occupied. In this article, I vary only the reward schemes faced. Elsewhere, I also vary preferences and moral commitments (Petersen 1992*a*, Sec. IV). Since the model assumes that workers are identical, all workers facing the same reward rule will behave in the same way, and choose the same actions.[9] Second, in choosing her actions each worker will, because of the interdependencies, take into account what everyone else does. The choice of each depends on the choice of all, because the reward of each depends on the choice of all. Third, if each worker, after having chosen her action and having observed the actions of everyone else, has no incentive to change her action, then all the workers are in mutual equilibrium, known as a Nash equilibrium for noncooperative games (Moulin 1982, chap. 3). I study behavior and compare outcomes that satisfy the Nash equilibrium solution. A further discussion of how this equilibrium might come about is given in Appendix A.

We can now introduce the reward scheme. An individual piece-rate scheme takes the form

$$\begin{aligned} w_i &= \beta(x_i - x_0) + w_0 && \text{if } x_i \geq x_0, \\ &= w_0 && \text{if } x_i \leq x_0, \end{aligned} \tag{3}$$

where x_0 is a quota that must be reached before the worker starts earning the piece rate, β is the piece rate (i.e., the pay per unit of output above x_0), and w_0 is the baseline wage or guaranteed minimum.[10]

A group piece-rate scheme takes the same form as (3), but the individual output x_i gets replaced with the average output per group member, namely

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[9] This symmetrical solution can be justified by strategic arguments; it satisfies the condition for Nash's bargaining solution (see Nash 1950). But it can also be justified by normative arguments, as in Hart (1955).

[10] Piece-rate schemes of this type are common and are described in Burawoy (1979, pp. 48–51), Edwards and Scullion (1982, pp. 181–82), and ILO (1984, p. 86).

$$\bar{x} = \frac{1}{n}\sum_{i=1}^{N} x_i. \tag{4}$$

A graphical illustration of the piece-rate scheme is given in figure 1a.

The condition for collective rationality can be derived by performing a simple thought experiment. Suppose the work group were able to decide and act as a unified body. If so, it would choose effort and hence output levels for each worker so that no one could be made better off without hurting someone else by changing effort and output levels. This amounts to finding the collectively rational solution. For both individual and group piece-rate schemes we derive the condition for collective rationality as[11]

$$\frac{V'}{P' \cdot [f_1 + (n-1)f_2]} = \beta \quad \text{for each } i, \tag{5a}$$

$$\frac{V'}{P'f_1} = \frac{\beta \cdot [f_1 + (n-1)f_2]}{f_1} > \beta \quad \text{for each } i, \tag{5b}$$

where (5*b*) is just (5*a*) rewritten.[12]

The condition takes into account the interdependencies between the workers, saying that each worker in deciding her effort level should consider the effects her own actions have not only on her own output but also on the outputs of the other workers. This can be seen from the

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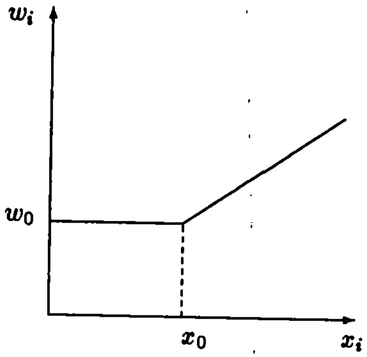
[11] Recall that $V' \equiv \partial V(a_i)/\partial a_i$, $P' \equiv \partial P(w_i)/\partial w_i$, $f_1 \equiv \partial x_i/\partial a_i$, $f_2 \equiv \partial x_i/\partial a_j$, and similarly for the second derivatives V'', P'', f_{11}, f_{12}, and f_{22}. See eqq. (1*b*)–(1*e*).

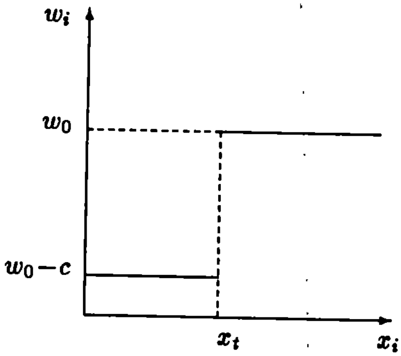
[12] Here, and in the individually rational solutions in (6) and in (10) below, I present two versions of the conditions. Since the second version is just the first rewritten, they amount to the same, but stating both helps the discussion of the results.

FIG. 1.—A graphic illustration of the material reward schemes in equations (3) and (8). Here, x_i (or alternatively $\bar{x}$) is the individual (or alternatively group) output and w_i is the individual wage paid as function of output; x_t is the target under both a target-rate scheme and a piece-rate scheme with a target; x_0 is the amount of output that has to be produced before starting to earn a piece rate, under both the linear piece-rate scheme with a guarantee and under the piece-rate scheme with a target; w_0 is the baseline salary under a linear piece-rate scheme with a guarantee, the wage received under a target-rate scheme when the target is reached, and the baseline salary received under piece-rate scheme with a target when the target is reached. The wage received under target-rate scheme when the target is not reached is shown by $w_0 - c$; it is also the baseline salary received under a piece-rate scheme with a target when the target is not reached.

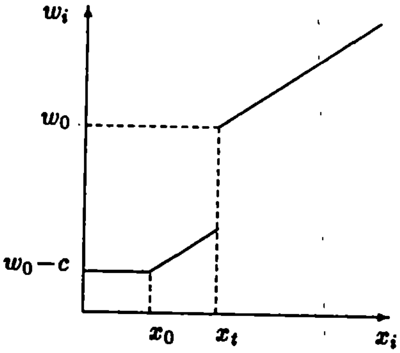
(a) Linear piece-rate scheme with a guarantee



(b) Target-rate scheme


w_i
w_0
$w_0 - c$
x_t
x_i

(c) Piece-rate scheme with a guarantee and a target


w_i
w_0
$w_0 - c$
x_0
x_t
x_i

left-hand side of (5*a*), where the impact of worker i's effort on the other workers' output is taken into account, both through the direct impact due to f_2 and through the number of workers n in the group. A proof of (5) is given in Appendix B.

Having defined what would be collectively rational, we can turn to the behavior of the work group. To understand its functioning we must analyze the behavior of its constituent members.

Each member, under the egoistic preferences assumed in this paper, is interested in maximizing her own preferences, taking the actions of the other workers as given. Under both individual and group piece-rate schemes her preferences over outcomes are as follows, ranked from most to least preferred:

A. I take it easy, everyone else works hard.
B. I work hard, everyone else works hard.
C. I take it easy, everyone else takes it easy.
D. I work hard, everyone else takes it easy.

In this formulation, each worker plays a strategic game against everyone else. In game-theoretic terminology, outcome A represents the free-rider solution, and outcome D represents the "sucker" solution (Elster 1979, p. 22).

Consider first the group piece-rate scheme. Each worker may think as follows: "It is in my best interest to take it easy, because if the others work hard I will reap the benefits of their labors, and if the others take it easy my own effort will affect the group output only negligibly, and hence I will exert myself to no avail." Taking it easy is thus a dominant strategy: no matter what the others do, I am best off taking it easy. If everyone else works hard, I will get outcome A, which is better than outcome B where I would also work hard. If everyone else takes it easy I am best off also taking it easy. In that case outcomes A and B are not accessible, and outcome C is preferred over outcome D. The central mechanism underlying this reasoning is that my impact on the output of the work group and hence on my wage is negligible, of order $1/n$, whereas I have to bear the entire cost of the effort of bringing about that impact on the output.

As is often the case, what each can achieve individually, all cannot achieve simultaneously (Hirsch 1976, p. 5). If everyone acts according to the logic in the preference ordering above, effort and hence output levels will be low and, by (3), little will be earned. Individually rational behavior leads to collective irrationality: outcome C is implemented, but everyone would have preferred outcome B. The workers face the famous Prisoner's Dilemma game. Outcome C is a so-called Nash equilibrium in dominant strategies (for definition of a dominant strategy, see Moulin [1982, pp. 14–15]).

In individual piece-rate schemes, the argument runs much the same. Each worker has an incentive to take it easy, because each, through the interdependencies, will reap benefits from the efforts of the other workers. The incentive to take it easy is weaker than it is under a group piece-rate scheme, but it is still present, and free-rider problems emerge.[13]

In formal terms, the argument is as follows. Under a group piece-rate scheme each worker chooses her effort level according to the condition

$$\frac{V'}{P' \cdot [f_1 + (n-1)f_2]} = \beta/n \quad \text{for each } i, \tag{6a}$$

$$\frac{V'}{P'f_1} = \frac{\beta \cdot [f_1/n + f_2(n-1)/n]}{f_1} < \beta \quad \text{for each } i, \tag{6b}$$

where (6*b*) is (6*a*) rewritten, while under an individual piece-rate scheme the condition for individual rationality is

$$\frac{V'}{P'f_1} = \beta \quad \text{for each } i. \tag{7}$$

Proofs of (6) and (7) are given in Appendix B.

By comparing the individually rational solutions (eqq. [6], [7]) to the condition for collective rationality (eq. [5]), we can conclude that less effort is expended in the former. Under the group or individual piece-rate scheme, outcome C is implemented, but outcome B is collectively rational. How the equations show this to be the case can be seen by several arguments.

Starting with the group piece-rate scheme, the left-hand sides of (6*a*) and (5*a*) contain the same expressions, whereas the right-hand side of (6*a*) is smaller than that of (5*a*), β/n versus β. Thus, in order for the equalities to hold, the left-hand side of (6*a*) must be smaller than that of (5*a*). This will only be the case when the effort level $\hat{a}_{GPR}$ in the group piece-rate scheme is smaller than the effort level $\hat{a}_{CR}$ in the collectively rational solution, because on the left-hand side of (6*a*) the numerator V' decreases when a_i decreases while the denominator $P' \cdot [f_1 + (n-1)f_2]$ increases when a_i decreases, due to the derivatives in (1*b*)–(1*e*). So, the ratio on the left-hand side of (6*a*) decreases as a_i decreases. Hence, when the individually rational effort level $\hat{a}_{GPR}$ under the group piece-rate scheme is smaller than the effort level $\hat{a}_{CR}$ in the collectively rational solution, the ratio on the left-hand side of (6*a*) is also smaller than that on the left-hand side of (5*a*), which is required in order for the equality in (6*a*) to hold. A formal proof of this is given in Appendix B. A perhaps more intuitive way to see this result, by comparing (5*a*) and (6*a*), is that the piece rate faced in the individually rationally solution is β/n, whereas in the collectively rational solution the workers are asked to adjust their behavior to a piece rate that is β. At a lower effective piece rate less effort is also likely to come forth.

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[13] In the case where f_2 is positive but small, it will, under the individual piece-rate scheme, be rational to work quite hard, but still not quite as hard as required in the collectively rational solution in B. So the rank ordering of outcomes is preserved, but taking it easy under outcomes A or C will mean working hard, though not as hard as in outcome B.

The proof in the case of (5) and (7), comparing collective rationality to individual rationality under an individual piece-rate scheme, is similar. It is deferred to Appendix B.

Further, by comparing (6*a*) and (7), we see that workers take it less easy under the individual piece-rate scheme than under the group piece-rate scheme. The mechanism is that the tie between individual effort and reward is blurred more under the group than under the individual piece-rate scheme. The proof is similar to that above and is deferred to Appendix B.

The results of this analysis can be summarized in proposition 1.

PROPOSITION 1. Under team interdependencies, group and individual piece-rate schemes are both subject to free-rider problems. Each worker has an incentive to take it easy, hoping the others will work hard. In addition, a group piece-rate scheme is more susceptible to free-rider problems than an individual piece-rate scheme.

I turn now to the individual and group target-rate schemes, in which a high wage is paid if a production target is reached and a low wage is paid otherwise. An individual target-rate scheme has this structure:

$$\begin{aligned} w_i &= \beta(x_i - x_0) + w_0 && \text{if } x_i \geq x_t, && \text{where } x_t > x_0, \\ &= \beta(x_i - x_0) + w_0 - c && \text{if } x_0 < x_i < x_t, && \text{where } c > 0, \\ &= w_0 - c && \text{if } x_i \leq x_0, \end{aligned} \tag{8}$$

where x_0 is a quota that must be reached before the piece rate is earned, x_t is the production target, and c is a penalty imposed if the target is not reached. If β equals zero, we have a pure target-rate scheme. The workers then face a two-tiered system: a high wage, w_0, is paid if the target is reached, and a low wage, $w_0 - c$, is paid otherwise. Figures 1b and 1c illustrate both cases, that is, in which β equals and differs from zero.[14]

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[14] Crozier (1964, p. 68) describes a target-rate scheme. Organizational psychologists refer to target-rate schemes as goal setting. In experiments they tend to perform better than continuous reward schemes (see Locke 1968; Guzzo and Katzell 1987, p. 114).

Under a group target-rate scheme, x_i is replaced with the average output per group member; namely $\bar{x}$ as defined in (4).

It is now always possible to choose the baseline wage, w_0, the penalty, c, and the target, x_t, so that the rank ordering of outcomes A and B in the Prisoner's Dilemma game is reversed, yielding the following preference ordering:

B. I work hard, everyone else works hard.
A. I take it easy, everyone else works hard.
C. I take it easy, everyone else takes it easy.
D. I work hard, everyone else takes it easy.

The mechanism behind the reversal in the ranking is this. Under a group target-rate scheme, each worker might reason as follows: "If the others work hard it is in my best interest also to work hard, otherwise we might not hit the group target, and I would rather work hard and hit the target than take it easy and miss it." The central point is that a very small drop in output from, say, x_t to $x_t - \epsilon$, may cause a very large drop in the wage; under a pure target-rate scheme, wages will drop from w_0 to $w_0 - c$. Each worker has an incentive not to be the cause of a drop in output below the target (see also van de Kragt, Orbell, and Dawes 1983; on step-function payoffs, see Hardin [1982, pp. 55–61]).

In an individual target-rate scheme, the argument is much the same: "If the others work hard it is in my best interest also to work hard, otherwise I might not hit my individual target, and I would rather work hard and hit the target than take it easy and miss it. Therefore, when everyone else is industrious it is also in my best interest to be industrious." Everyone working hard becomes a mutually self-enforcing outcome.

Conversely, "When the others take it easy, it is in my best interest also to take it easy." Under a group target-rate scheme, one hardworking member will not enable the group to reach the target. And each worker would rather take it easy and miss the target than be industrious and still miss it. Under an individual target-rate scheme, it will still be difficult for one hardworking member to reach the target if the others take it easy, because the interdependencies cause the output of that member to be low even though she works hard.

We can conclude, then, that under a target-rate scheme, individual and collective rationality may coincide. Choosing to work hard under a target-rate scheme, though, is not a dominant strategy as choosing to take it easy was under the piece-rate scheme. It is rational to work hard only when the others also work hard. Outcome B is one equilibrium point of the game in the Nash sense, outcome C is another. The former is collectively rational, the latter is collectively irrational.[15] Whether a worker will choose to work hard will depend on whether she expects and trusts other workers also to work hard. As experiments have shown, the level of trust may be crucial for eliciting cooperation in these types of games (see Tyszka and Grzelak 1976; Dawes, McTavish, and Shaklee 1977; Yamagishi and Sato 1986). I summarize this analysis in proposition 2.

PROPOSITION 2. Under team interdependencies, a group or an individual target-rate scheme may overcome the free-rider problem of a group or individual piece-rate scheme.

In concluding the preceding analysis, it may be instructive to compare what I will call the target-rate game of this section to two other similar and well-known games, the Assurance Game and the Chicken Game.

In the Assurance Game, extensively studied by Amartya Sen, the ranking of alternatives A–D is identical to the ranking found in the target-rate game above (see Sen 1974; Elster 1979, p. 22). There is a crucial difference between the two games, though. In the Assurance Game, the ranking of outcome B over outcome A occurred through a *change* in the actors' *preferences,* which originally were of the Prisoner's Dilemma type, in which outcome A is preferred over outcome B (see Sen 1974). The change may, for example, have been caused by concern for coworkers arising out of repeated interactions (see, e.g., Elster 1985, p. 362). In the target-rate game, the actors' preferences are unchanged. The reversal in the ranking of alternatives is produced by a *change* in the *structure of interaction,* namely, in the reward rules the work group faces, from a piece-rate scheme to a target-rate scheme.

In the present analysis, the collective action problem under the target-rate scheme is solved by everyone working hard. There are other solutions. For example, some fraction of the work group may shirk their duties and the remaining workers may work a bit harder to make up for them. The workers then face the so-called Chicken Game, in which each would prefer to be among the lazy (see Taylor 1987, chap. 2). But if the choice is between (*a*) taking it easy and missing the target and (*b*) working hard and hitting the target, each prefers the latter. The issue, for each worker, is whether there is a sufficient number (i.e., a critical mass) of workers who will work hard (Oliver, Marwell, and Teixeira 1985). Conceivably, bargaining among the workers as well as norms of fairness would then determine who gets to work hard and who gets to take it easy (for the importance of rules of fairness, see, e.g., Kerr [1983] and Stark [1990]). The current conceptual framework, in which all workers are assumed to be homogeneous, is ill-equipped to address these issues. Introducing heterogeneity in preferences and productive capabilities would probably allow one to predict the allocation of workers to the two groups, those who work hard and those who get to take it easy. Norms of fairness most likely also bear on the issue (see Elster 1989, chap. 5).

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[15] In fact, the target-rate game possesses an infinity of Nash equilibria, in which some workers are lazy and others work hard to make up for them.

B. Only Group Outputs Observable

The case in which there are no interdependencies between workers but only the group output can be observed, differs in only two ways from the case just considered.[16] First, there are no interdependencies to be taken into account; that is, the technology in (1) satisfies $f_2 = f_{12} = 0$. Second, individual incentive schemes are no longer feasible, since individual outputs are unobservable. We need therefore only consider the two group-incentive schemes.

It is immediately clear that the argument used to compare group piece-rate and target-rate schemes under the interdependent technology can be repeated. A group piece-rate scheme will be subject to free-rider problems, whereas a group target-rate scheme may overcome these. The arguments differ in one respect only. When interdependencies are absent, each worker should, in the collectively rational solution, act as if she faced an individual piece-rate scheme; whereas when interdependencies are present, she should take into account the positive effects of her own actions on the outputs of the other workers. The structure of interdependencies should be reflected in the choice rules.

The mathematical argument is as follows. Under collective rationality each worker should choose her effort level so that

$$\frac{V'}{P'f_1} = \beta \quad \text{for each } i, \tag{9}$$

which is identical to the individually rational choice rule under an individual piece-rate scheme (see eq. [7] above). When the workers face a group piece rate, the condition for individual rationality becomes

$$\frac{V'}{P'f_1/n} = \beta \quad \text{for each } i, \tag{10a}$$

$$\frac{V'}{P'f_1} = \beta/n \quad \text{for each } i, \tag{10b}$$

where (10*b*) is just (10*a*) rewritten.

Comparing (9) and (10) reveals that too little effort is expended under a group piece-rate scheme, since the denominator on the left-hand side of (10*a*) is divided by n: each worker attempts to get a free ride. The argument for why this is the case parallels exactly the argument made in comparing equations (5*a*) and (6*a*) above and need not be repeated here. It is given in Appendix B along with a formal proof of (9) and (10).

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[16] This case is discussed in Holmström (1982). See Sec. I above.

As a corollary, one can also show that the larger the group, the less effort it becomes individually rational to expend under the group piece-rate scheme, because the larger the group, the less effect one's effort has on the group output and hence on the wage.[17] The latter is formally seen by taking the second derivative of the reward rule (3) with respect to effort a_i and the group size n, after having replaced x_i with $\bar{x}$ from (4) and x_i in (4) with $f(a_i)$ yielding

$$\partial^2 w_i / \partial a_i \partial n = -f_1 \beta / n^2 < 0. \tag{11}$$

This says that the effect of increased effort on one's wage decreases with the group size (a corollary made famous by Olson [1965, pp. 28, 35]). The corollary will in general not hold when there are interdependencies; hence, the group size need then not impede collective action (a proof is given in Petersen [1987, app. 4B]; see also Oliver and Marwell 1988).

When a group target-rate scheme is used, in contrast, individual and collective rationality may coincide, the mechanism being the same as in the case of interdependencies.[18]

Again, the problem of collective irrationality may be overcome by an appropriate target-rate scheme. This scheme may operate as an alternative to the solution sketched by Stinchcombe and Harris (1969), in which a supervisor keeps free riders in line. The target-rate scheme may align individual and collective interests, thereby leading to systems rationality. Under a group piece-rate scheme, the two interests, individual and collective, diverge, and systems irrationality or malfunctioning ensues.

IV. DATA AND METHODS

I use data from two Industry Wage Surveys conducted by the U.S. Bureau of Labor Statistics, in the wood household furniture and the nonferrous foundries industries (see U.S. Department of Labor 1976*b*, 1977*e*).

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[17] For some, albeit limited, empirical evidence on this point, see Marriott (1949, 1951), Campbell (1952), Shimmin (1955), Buck (1957), Edwards and Heery (1985, p. 362, n. 10). See also the experimental literature (e.g., Harkins, Latane, and Williams 1980).

[18] One may further show that the size of the group under a target-rate scheme does not influence the decision to work hard or to be lazy, at least not in the formal statement of the problem. If the others work hard, it is in my best interest also to work hard, whereas if the others take it easy, it is in my best interest also to take it easy, irrespective of the group size (see also Oliver and Marwell 1988).

These industries have codes 2511 and 336 respectively, as defined in the *Standard Industrial Classification Manual* (see U.S. Executive Office of the President 1987). The survey of the wood household furniture industry was conducted in November 1974; the survey of the nonferrous foundries industry was conducted in May 1975. The populations for the surveys and the sampling from the populations are described in the surveys (U.S. Department of Labor 1976*b*, p. 51; 1977*e*, p. 39). Within each industry a sample of approximately 350 establishments was drawn. From establishment records, information was obtained both on establishment characteristics and on most of the production workers in the establishment.

Information on the establishment characteristics includes the following: size (i.e., number of employees); region and area within region; whether the establishment is located in a standard metropolitan statistical area (SMSA); union status and, if unionized, the name of the union organizing the majority of the production workers; production technology and major products; the number of employees remunerated by each of 10 different payment schemes; and provision of fringe benefits. For each employee surveyed, information was obtained on sex, occupation (an industry-specific code), method of wage payment (incentives or time rated), and hourly earnings. No information is available on age, experience, or education. However, the occupational classification is unusually detailed, corresponding to nine digits in the *Dictionary of Occupational Titles* (see U.S. Department of Labor 1977*a*). Within an occupation there is probably little variation in productivity-related characteristics such as education. Hence, the omitted variable bias resulting from not controlling for education is likely to be small.[19]

The wage data pertain to straight-time hourly wages, excluding premium pay for overtime and work on weekends, holidays, and late shifts. Thus, we do not conflate pay earned on regular hours with pay earned on overtime and irregular hours. Nonproduction bonuses, such as year-end bonuses, are excluded. The wages of workers paid according to incentives were usually obtained as average hourly wages over a period of two to four weeks (see U.S. Department of Labor 1976*b*, p. 52; 1977*e*, p. 40).

Unfortunately, the data on the incentive schemes are less complete than desirable. For each worker, we know whether he or she is paid by the hour or by results, but for the latter, we do not always know the type of incentive scheme used. However, we know the distribution of an establishment's employees among the different types of incentive schemes; that is, we know the number of workers paid according to individual piece rates, individual target rates, and so on.

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[19] In the case of salespersons in department stores, some limited evidence exists that supports such a claim. Petersen (1989, table 2) shows that human capital variables have almost no effects on the wage earned once one controls for a set of detailed occupational dummy variables.

Therefore, I selected the subsample of establishments in which only one type of output-related payment scheme was in use. For those establishments, we know the incentive scheme of each worker. We can therefore compare the effects of different incentive schemes across establishments, but can make no intraestablishment comparisons.

I stress that establishments were selected on the basis of an independent variable, the payment scheme. Thus, there should be no problem of observer sample-selection biases (as discussed in Manski and McFadden [1981, p. 10]).

The main method used in the statistical analysis is linear regression analysis of the logarithm of the hourly wage rate on establishment and individual characteristics. I chose the semilogarithmic form of the wage equation for its ease of interpretation; a coefficient is interpretable (roughly) as the percentage change in the mean of the dependent variable resulting from a unit increase in the associated independent variable. Note that we need therefore not consider the general wage levels prevailing in 1974 and 1975 to make social sense of the results (for an extended discussion, see Petersen [1989, Sec. III]).

A. Specification of Hypotheses for the Empirical Analysis

The analysis in Section III and the two propositions presented there are about productivity under different types of payment schemes. Productivity is not measured in the data sets used here, as it rarely is. However, we have a measure of another variable, wages, that may be correlated with productivity. In the following I justify my assumption that wages and productivity are correlated.

Consider wages at the establishment or work-group level. For workers paid by results, wages are clearly a function of productivity. The more that is produced, the more is earned. Under output-related payment schemes, high-productivity workers on the average earn more than low-productivity workers within the same establishment.

The question now becomes, Does this relationship between productivity and wages, which holds at the establishment level, also hold across establishments? Or, phrased this way, Do high-productivity workers in one establishment earn more than low-productivity workers in another establishment? This relationship need only hold for the incentive-rated workers across establishments within the same industry. We need not assume a similar relationship across industries nor, for time-rated workers, across establishments within an industry. In Britain, there is strong evidence of such a relationship. Ball and Skoech (1981) studied interplant differences in wages and productivity in 15 British industries and reported a very strong relationship between the two in almost every industry. Less research has been conducted on this topic in the United States, but the evidence that exists confirms such a relationship (see Levine 1992).

The assumption of a relationship between productivity and wages for incentive-rated workers across establishments is crucial to the empirical analysis that follows. Although the required assumption is hard to *verify,* it is not unrealistic. The statistical tests reported are performed separately for each of two industries, wood household furniture and nonferrous foundries. The industries are narrowly defined; they correspond to four (2511) and three (336) digits in the *Standard Industrial Classification Manual* (see U.S. Executive Office of the President 1987, pp. 114, 180). Thus, there should be considerable homogeneity between establishments within the same industry.

Furthermore, it would be peculiar indeed if, in the empirical analysis, where we control for as many as 64 variables, we were to find the relationship between the payment scheme and wages that follows from propositions 1 and 2 if there were no relationship between productivity and wages across establishments among incentive-rated workers. The hypotheses are quite specific and their rationales are clear.

Assume now that there is a relationship between wages and productivity among incentive-rated workers across establishments within the same narrowly defined industry. Propositions 1 and 2, taken together, suggest the following two hypotheses for the empirical analysis.

HYPOTHESIS 1. Group target-rate workers will on the average earn more per hour than group piece-rate workers, since the latter are subject to free-rider problems.

HYPOTHESIS 2. Under team interdependencies, individual target-rate workers will on the average earn more per hour than individual piece-rate workers, since the latter are subject to free-rider problems.

Note that hypothesis 1 is correct even if team interdependencies are not present, whereas hypothesis 2 holds only in their presence. With respect to hypothesis 2, we do not know for which workers or establishments in the data that team interdependencies are present. One should therefore expect that the wage differences between group target- and piece-rate workers are larger than those between individual target- and piece-rate workers, because free-rider problems are present for all group piece-rate workers whereas they are present for only a subset of the individual piece-rate workers, those subject to team interdependencies.

One might also speculate whether team interdependencies are more important in the wood household furniture than in the nonferrous foundries industry. The reason being that the latter industry relies more on machine processes (see U.S. Department of Labor 1977*e*, p. 1) than the former. If such differences in interdependencies are present, one should expect greater differences in wages between target- and piece-rate workers in the wood household furniture than in the nonferrous foundries industry.

V. EMPIRICAL ANALYSIS

A. Univariate Analysis

Descriptive statistics for the two industries—wood household furniture and nonferrous foundries—are presented in table 2. The table gives the average hourly wages of incentive- and time-rated workers by the type of incentive scheme used in their establishments: individual piece rates, individual target rates, group piece rates, and group target rates. Part A, pertaining to the wood household furniture industry, shows that group target-rate workers on the average earn 50% more per hour than group piece-rate workers and that individual target-rate workers earn about 8% more per hour than individual piece-rate workers. Of the four groups of incentive-rated workers accounted for in part A of table 2, group piece-rate workers earn the lowest average wages. Of the nine groups of workers listed in part A, only two other groups earn lower average wages than group piece-rate workers.

Furthermore, it is important to note that establishments using group target-rate schemes are not necessarily high-paying establishments. Only the workers paid a group target rate earn high wages in those establishments, whereas the time-rated workers in the same establishments earn lower wages than any other group of workers in table 2. Conversely, establishments using group piece-rate schemes are not necessarily low-paying establishments. Only the workers paid on group piece rates earn low wages in those establishments, whereas the time-rated workers in the same establishments earn higher wages than any other group of time-rated workers in the table. Therefore, it appears that the effect of being paid a group target rate is not primarily the effect of working in establishments that, on the average, pay high wages. It appears to be the effect of being remunerated by a group target rate. These considerations do not apply when we compare establishments using individual piece-rate schemes and individual target-rate schemes.

Part B of table 2, pertaining to the nonferrous foundries industry, reveals a similar pattern. Group target-rate workers, on the average, earn more per hour than group piece-rate workers, but only about 14% more. Individual target-rate workers also earn more per hour than individual piece-rate workers, about 6% more. The effects of the target-rate

TABLE 2

Means and SDs of Hourly Wages by Incentive Status of Workers and Type of Incentive Scheme in the Establishment Production Workers of Two Industries

	Incentive Workers			Time-rated Workers		
	Hourly Wages			Hourly Wages		
Incentive Scheme in Establishment	Mean	SD	N*	Mean	SD	N*
A. Wood household furniture industry:†						
No incentive scheme				2.91	.64	25,082 (222)
Individual piece rate	3.33	.92	2,623 (36)	2.63	.50	1,493 (35)
Individual target rate	3.58	.90	1,185 (13)	2.93	.45	259 (11)
Group piece rate	2.74	.42	183 (6)	2.99	.27	207 (4)
Group target rate	4.05	1.66	1,582 (22)	2.52	.23	362 (2)
B. Nonferrous foundries industry:†						
No incentive scheme				4.69	1.32	9,665 (237)
Individual piece rate	4.82	1.16	1,108 (54)	4.17	.90	1,770 (54)
Individual target rate	5.12	1.07	1,205 (38)	4.70	.89	1,729 (38)
Group piece rate	3.93	.34	168 (3)	4.32	.57	224 (2)
Group target rate	4.47	.44	150 (6)	4.56	.75	213 (4)

Note.—Figures for part A were computed using the *Industry Wage Survey* (U.S. Department of Labor 1976*b*); figures for part B were also computed from this series (U.S. Department of Labor 1977*e*). In both cases, the results pertain to production workers.

* N refers to the number of workers and (in parentheses) the number of establishments.

† Ratios of mean wages for wood household furniture: $\overline{w}_{ITR}/\overline{w}_{IPR} = 1.075$; $\overline{w}_{GTR}/\overline{w}_{GPR} = 1.478$. For nonferrous foundries, the ratios of mean wages are: $\overline{w}_{ITR}/\overline{w}_{IPR} = 1.062$; $\overline{w}_{GTR}/\overline{w}_{GPR} = 1.137$. These figures are computed as the ratio of the mean hourly wages of individual target-rate workers ($\overline{w}_{ITR}$) and individual piece-rate workers ($\overline{w}_{IPR}$) and the ratio of the mean hourly wages of group target-rate workers ($\overline{w}_{GTR}$) and group piece-rate workers ($\overline{w}_{GPR}$).

schemes are weaker than in the wood household furniture industry, but they are in the predicted directions. Group piece-rate workers on the average earn less than any other group of workers in the table, even less than those paid by the hour. In terms of earnings, being paid by the hour is better than being paid by a group piece rate.

In the nonferrous foundries industry, in contrast to the wood household furniture industry, there is an aggregate-level correlation between working in an establishment using group target rates and the general level of pay in the establishment, as time-rated workers also earn high wages in those establishments. For group incentive schemes, there still appears to be an independent effect of working under a group target-rate scheme as opposed to a group piece-rate scheme. The difference in hourly pay between the two schemes is about 14%. The difference in hourly pay between time-rated workers working in establishments using group target rates and time-rated workers working in establishments using group piece rates is only 5%. Somewhat informally, we can therefore say that 5% of the pay difference between group target-rate workers and group piece-rate workers is due to the fact that the former work in high-paying establishments (i.e., the group target-rate workers would have received 5% more just because they work in those establishments). The remaining 9% of the pay difference is, according to this line of reasoning, due to the different payment schemes. The same considerations do not hold when we compare establishments using individual target-rate schemes and individual piece-rate schemes.

In conclusion, the data support the analyses of Section III and the hypotheses of Section IV. Under the interpretations developed there, free-rider problems are ubiquitous under group piece-rate schemes but may be overcome under a group target-rate scheme. Similarly, when there are interdependencies between workers but individual outputs are observable, individual target-rate schemes may overcome the free-rider problems of individual piece-rate schemes. The data, so far, support both theoretical conjectures: group target-rate workers, on the average, earn more than group piece-rate workers, and individual target-rate workers on the average earn more than individual piece-rate workers. The differences in pay between target- and piece-rate schemes are much larger for group than for individual schemes. This squares with the theoretical analysis. Group-based schemes are always subject to free-rider problems, whereas individual schemes are so only when team interdependencies are present. Some fraction of the workers employed under individual incentive schemes probably work in settings with no interdependencies. This fraction is not subject to free-rider problems. One should therefore expect that the average wage differences between individual piece- and target-rate schemes are smaller than the average wage differences between group piece- and target-rate schemes, because the former difference is computed partly on the basis of groups of workers not subject to free-rider problems.[20]

In the univariate analysis—and as we will see in the multivariate analysis—it appears that free-rider problems are more severe in the wood household furniture industry than in the nonferrous foundries industry. The wage difference between group piece-rate workers and group target-rate workers is much bigger in the former.

There is one piece of evidence, albeit circumstantial, that may explain this difference. Before submitting the evidence I state the theoretical principle that makes the evidence interesting. It is widely believed that the more cohesive a social group is, the more able it is to solve free-rider problems (Dion 1973) because social rewards and moral commitment have a higher probability of succeeding in cohesive groups and both may solve free-rider problems, as I have discussed elsewhere (Petersen 1992*a*, Sec. IV). It is also commonly believed that the cohesiveness of a group may be a decreasing function of the turnover rate of its members: the lower the turnover rate, the higher the cohesiveness (see, e.g., Thernstrom 1970).[21]

Now to the evidence. In the months of the surveys and the months preceding them in the two industries, the turnover rate was almost 100% higher in the wood household furniture industry than in the nonferrous foundries industry, respectively 7.8% and 4.1% per month in the months of the interviews. Furthermore, the voluntary quit rate was almost 400% higher in the wood household furniture industry than in the nonferrous foundries industry, respectively 3.8% and 1.0% per month in the months of the interviews (see *Employment and Earnings* [U.S. Department of Labor 1975*b*, p. 122; 1975*c*, p. 122; 1975*d*, p. 124; 1975*e*, p. 114]). Although these turnover rates cannot be tied to individual establishments, they nevertheless indicate that the wood household furniture industry may be less cohesive than the nonferrous foundries industry. This in turn may explain why the wood household furniture industry appears to have greater free-rider problems among group incentive workers.

This difference in effects of group incentive schemes might also be due to differences in the degree of team interdependencies between the two industries. In Section IV, I speculated on whether team interdependencies are less important in the nonferrous foundries than in the wood household industry, since the former industry to a larger degree depends on machine processes, which one might conjecture call for less teamwork.

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[20] Note also that in both industries, individual piece-rate workers on the average earn more than group piece-rate workers. This is consistent with the second part of proposition 1, which states that under team interdependencies group piece-rate schemes are more susceptible to free-rider problems than individual piece-rate schemes.

[21] This claim has a long history, starting, probably, with Tocqueville ([1835] 1969, p. 557) and Marx ([1852] 1979, p. 111).

B. Regression Analysis

The objective of the regression analysis is to assess whether the differences in average pay between the four payment schemes hold when control variables are introduced. The control variables are sex, occupation, establishment size, presence of a union, SMSA status, production technology (only in nonferrous foundries), major products (only in wood household furniture), and, of course, the central independent variable, the payment scheme. All variables are entered as sets of dummy variables: 63 dummy variables are used for the wood household furniture industry, and 64 are used for the nonferrous foundries industry.

The analysis focuses on the effects of the four output-related payment schemes. The other variables are not of substantive interest for the present purpose and are not reported in the tables. The theory developed speaks to the effects of the payment scheme, not to the effects of the other variables.[22]

In each industry, I include only workers who are paid according to one of the four incentive schemes: individual piece rates, individual target rates, group piece rates, and group target rates. I report three different specifications of the wage equation. The first specification excludes the dummy variables for occupation, technology, and major products. The second specification excludes the dummy variables for occupation but includes the variables for technology and major products. The third specification includes all the variables.

Table 3, part A, gives the results for the wood household furniture industry. In all three specifications of the wage equation we see that the group target-rate workers on the average earn about 18%–20% more per hour than the group piece-rate workers, confirming the predictions of hypothesis 1.[23] Individual target-rate and individual piece-rate workers earn more or less the same in all three specifications. The difference between the two latter groups becomes negligible—of about 0%–3% in hourly wages—when the additional variables in table 3 are controlled for, but it is still in the predicted direction. The proportions of explained variance are high, ranging from .44 to .64.

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[22] The full set of results can be found in Petersen (1992*b*, tables 6–7).

[23] The effect of 18% is computed as follows, as are the percentage differences reported below. The point estimates, for example, from col. 2 of table 3, for being paid on a group target-rate scheme and a group piece-rate scheme are $-.13$ and $-.30$, respectively. The relative difference between the two groups is therefore $\exp[-.13 - (-.30)] - 1 = .18$.

There is a good reason for giving most weight to the results in the first column of table 3 (part A), the results that are most consistent with the theory and that replicate the univariate differences in part A of table 2. The regressions are based on the wages of 5,573 workers (see table 3). I estimate 20, 32, and 64 effect parameters (including the constant) in the first, second, and third columns respectively. Formally, there is therefore no problem with the degrees of freedom. However, these 5,573 workers are employed in only 77 establishments. The group-incentive workers are employed in only 28 establishments (see table 2). There is much less variation in hourly wages within than between establishments. In the third column I end up fitting a regression equation with 64 parameters to the data on wages from 77 establishments. With limited intraestablishment variation in wages, I practically have as many explanations (i.e., 64) as points of data to explain (i.e., 77), yielding potentially quite unreliable results. So I face a problem of overfitting the data. Therefore, the results in the second and third columns are less reliable than those in the first. For that reason, we are well advised to put more confidence in the results from the first column. These square better with the theory and with the univariate differences in part A of table 2.

Table 3, part B, gives the corresponding results for the nonferrous foundries industry. As in the wood household furniture industry, in all three specifications of the wage equation, the group target-rate workers on average earn about 16%–17% more per hour than the group piece-rate workers. Again, the results are consistent with hypothesis 1 of Section IV. Also, as in the wood household furniture industry, the difference in hourly pay between individual target-rate and individual piece-rate workers is negligible. In column 1, the difference is in the predicted direction, but it is only three-quarters of a percent. In column 2 there is no statistically significant difference between the two groups, whereas in column 3 there is a statistically significant difference between the two groups: individual target-rate workers earn about 2% less per hour than individual piece-rate workers. As in the wood household furniture industry, the proportions of explained variance are high, ranging from .34 to .50.

Again, as in part A of table 3, we are well advised to put more confidence in the results from the first column of part B of table 3. These results are most consistent with the theory and replicate the pattern of univariate differences found earlier. Specifically, once the control variables in the first column are introduced, individual target-rate workers on the average do not earn less than individual piece-rate workers, whereas according to the second and third columns, they earn less.

TABLE 3

EFFECTS ON THE LOGARITHM OF THE HOURLY WAGE RATE OF INCENTIVE SCHEMES FOR WORKERS PAID AN INDIVIDUAL OR A GROUP RATE IN TWO INDUSTRIES

	COVARIATES					
	Fewest		More		All	
A. Wood household furniture industry:						
Payment scheme:[a]						
Individual target rate	.027	(.009)	$.000^{+}$	(.009)	$.007^{+}$	(.009)
Group piece rate	−.093	(.014)	−.304	(.013)	−.311	(.013)
Group target rate	.109	(.008)	−.133	(.009)	−.135	(.009)
R^2, σ	.435	.215	.618	.177	.642	.172
Ratios of estimated wages:[b]						
$\hat{w}_{ITR}/\hat{w}_{IPR}$	1.027		1.000^{++}		1.007^{++}	
$\hat{w}_{GTR}/\hat{w}_{GPR}$	1.224		1.186		1.192	
B. Nonferrous foundries industry:						
Payment scheme:[a]						
Individual target rate	$.007^{+}$	(.010)	$-.019^{+}$	(.010)	−.035	(.010)
Group piece rate	−.260	(.024)	−.265	(.023)	−.245	(.023)

Group target rate	−.108	(.018)	−.090	(.020)	−.092	(.020)
R^2, σ	.338	.189	.447	.172	.500	.165
Ratios of estimated wages:[b]						
$\hat{w}_{ITR}/\hat{w}_{IPR}$	1.007		.981^{++}		.966	
$\hat{w}_{GTR}/\hat{w}_{GPR}$	1.164		1.191		1.178	

NOTE.—In both industries, controls are included for sex, unionization, size of establishment, SMSA, area, and occupation. In the wood household furniture industry there are additional controls for primary and secondary products. In the nonferrous foundries industry there are additional controls for primary type of metal cast, primary and secondary method of casting, and jobbing foundry. Col. 3 includes all the variables (64 and 65 parameters included in the models in parts A and B). Col. 2 excludes the occupational dummy variables (32 and 31 parameters included in the models in parts A and B). In the wood household furniture industry, col. 1 excludes the occupational and the primary and secondary products dummy variables (20 parameters included in the models in part A). In the nonferrous foundries industry, col. 1 excludes the occupational, the primary type of metal cast, the primary and secondary method of casting, and the jobbing foundry dummy variables (18 parameters included in the models in part B). In both industries, each variable is entered as a dummy or set of dummy variables. Only the effects of the payment system variables are presented. The estimates are obtained by least squares, and the standard errors of the estimates are given in parentheses except in the entries for R^2, where σ indicates the estimate of the standard deviation of the error term. Numbers for part A are computed from the *Industry Wage Survey: Wood Household Furniture Industry* (see U.S. Department of Labor 1976*b*). N (workers) = 5,573; N (establishments) = 77. Numbers for part B are computed from the *Industry Wage Survey: Nonferrous Foundries* (U.S. Department of Labor 1977*e*). N (workers) = 2,631; N (establishments) = 101.

[a] Reference category is individual piece rate.

[b] This is computed as $\hat{w}_{ITR}/\hat{w}_{IPR} = \exp(\beta_{ITR})$ and $\hat{w}_{GTR}/\hat{w}_{GPR} = \exp(\beta_{GTR} - \beta_{GPR})$, where β_{IPR}, β_{GTR}, and β_{GPR} are the point estimates for being paid on an individual target rate, a group target rate, and a group piece rate respectively. The point estimates are those reported in this table.

[+] Not significantly different from zero at the .05 level (two-tailed test).

[++] Computed from coefficients that are not significantly different from zero.

It is instructive to conduct a formal test of the fits of the models as one goes from column 1 to 2 to 3 in panels A and B in table 3. In the present case, as the number of observations is quite large, the standard F-test is not suitable since, in large samples, it tends to reject the null hypothesis too often. Rather, a Bayesian posterior prediction criterion test will be used, a type of test Raftery (1986) made known to sociologists in the context of contingency tables.[24]

In part A, column 3 fits better than column 2, which again fits better than column 1. This is not greatly damaging to the part of the theory that led to hypothesis 2. I just do not find that individual target-rate workers on the average earn significantly more than individual piece-rate workers once all the controls in column 3 are introduced, since column 3 fits better than columns 1 and 2 and the effect of being paid on an individual target rate is not significantly different from zero in column 3. In part B, column 2 fits better than columns 1 and 3 in two pairwise tests. This is also not so damaging to the part of the theory that led to hypothesis 2. I just do not find that individual target-rate workers earn more than individual piece-rate workers once all the controls in column 2 are introduced, since column 2 fits better than columns 1 and 3 and the effect of being paid on an individual target rate is not statistically significant in column 2. These tests add strength to the arguments above. I take the arguments about the number of establishments and the number of estimated parameters to be the most decisive in favor of focusing on column 1 in parts A and B; they are based on substantive reasons rather than on statistical tests than can be mechanical.

We can sum up the results of the regression analysis as follows. The data support the theory's predictions about the operation of the two group-incentive schemes. Group target-rate workers on the average earn more than group piece-rate workers. The theory asserts that group piece-rate schemes are subject to free-rider problems, whereas group target-rate schemes may alleviate these, thereby leading to higher effort levels,

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[24] The test, often known as the Schwarz criterion (Schwarz 1978), generally takes this form. Let H_0 and H_1 be the null and the alternative hypotheses, where the model under the null is nested within the model under the alternative hypothesis. Let D be the data. The test looks at the ratio

$$B \equiv P(H_0 \text{ is true} \mid D)/P(H_1 \text{ is true} \mid D).$$

If $\ln B > 0$, one accepts H_0, otherwise one accepts H_1. In the context of a linear regression model with normally distributed errors, the test is this. Let $\hat{\sigma}_0^2$ and $\hat{\sigma}_1^2$ be the estimated residual variances under the null and the alternative hypothesis respectively, let k_0 and k_1 be the number of parameters in the two models, and let N be the sample size. Then

$$\ln B = (N/2)\ln(\hat{\sigma}_1^2/\hat{\sigma}_0^2) + [(k_1 - k_0)/2](\ln N - 1).$$

higher output, and hence higher wages. Substantial differences in hourly pay between the two groups of workers were found both in the wood household furniture industry and in the nonferrous foundries industry.

In the regression analysis, in contrast to the univariate analysis, the prediction (from hypothesis 2) about the difference in hourly pay between individual piece-rate schemes and individual target-rate schemes was not borne out by the data. The difference in hourly pay between the two groups of workers is negligible, once the variables in table 3 are controlled for. It is important to note that the prediction was obtained under the condition of team interdependencies between the workers. Our data cannot tell us as to the extent to which this condition is satisfied. Our confidence in the theoretical implications of the results speaking to hypothesis 2 is therefore less than our confidence in the implications of the results speaking to hypothesis 1. The theory's prediction about group incentives holds irrespective of whether or not team interdependencies are present.[25]

VI. CONCLUDING REMARKS

At the most general level, the central conceptual issue addressed in the paper is this: How can actors construct social institutions so as to align individual and collective interests, thereby ensuring efficient systems' functioning? In the specific substantive context considered here—that of work-groups—the central task of the institutions was to solve the free-rider problems that individually rational behavior generates. Two institutions were considered.[26]

The first institution considered relies on authority relationships, in which a supervisor keeps free riders in line (e.g., Stinchcombe and Harris 1969; Alchian and Demsetz 1972). The second type of institution discussed relies on market-type relationships. When only group outputs can be observed, group target-rate schemes were shown to provide a potential solution to the free-rider problems that plague group piece-rate schemes. When there are team interdependencies but individual outputs can be observed, individual target-rate schemes may alleviate the free-rider problems of individual piece-rate schemes.

In the empirical analysis, I compared several market-type relationships. In both univariate and multivariate analyses, I showed that group target-rate schemes on the average lead to higher wages than group piece-rate schemes. The data, therefore, as far as the operation of group incentive schemes goes, are consistent with the interpretation, developed in Section III, that group piece-rate schemes are subject to free-rider problems, whereas group target-rate schemes may overcome these. In the multivariate analysis, I found no discernible difference in average pay between individual target-rate workers and individual piece-rate workers. However, in the univariate analysis, I found that individual target-rate workers earned about 6% and 8% more in hourly pay than individual piece-rate workers, as predicted by the theory.

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[25] As in the univariate analyses, the regression analyses show that individual piece-rate workers on the average earn more than group piece-rate workers, in both industries. This is consistent with the second part of proposition 1. See n. 20 above.

[26] Elsewhere I compare three additional and work-group internal mechanisms to the two solutions above, namely social rewards, altruistic preferences, and moral commitment (Petersen 1992*a*).

In most work-group situations, the institutions considered operate side by side. Which institution, one may ask, provides the most efficient solution to the free-rider problem and which is preferable? From the viewpoint of workers, market-type relationships seem preferable to authority relationships. The former facilitate autonomy at the work-group level, a feature workers might find desirable. In addition, market-type relationships make remuneration of supervisors superfluous, which, on economic grounds, may be desirable.

APPENDIX A

Discussion of the Nash Equilibrium Concept

Throughout the analysis in Section III, I relied extensively on the so-called Nash equilibrium concept for noncooperative games: If each worker, having chosen her own action and having observed the actions of everyone else, has no incentive to change her action, then all the workers are in mutual equilibrium. Each person's choice is the best possible response to the actions of everyone else.

The Nash equilibrium concept suffers from a well-known drawback. For worker i to choose her Nash strategy, she must first know the actions of the other workers, but none of the latter can choose a Nash strategy before the action of worker i is known. An endless circularity ensues: no one can act before she knows what all the others have done. The concept has nevertheless proved useful, and two alternative justifications have been proposed. The first relies on a dynamic specification of the game, in which the Nash equilibrium is seen as a stationary point toward which successive moves converge, an avenue utilized by Przeworski (1985, pp. 185–200); for technical details see Moulin (1982, pp. 115–36).

In the second justification, the Nash equilibrium emerges as the solution to a cooperative game. The workers get together before choosing their effort levels and communicate their intentions. Through this preplay communication each can promise or commit herself to play the collectively rational Nash strategy, provided the others also do so. If everyone sticks to her promise, no one will have regrets when the game is over (under the target-rate scheme). For assurance-type games, experiments have demonstrated the importance of communication for eliciting cooperation (see, e.g., Dawes et al. 1977; van de Kragt et al. 1983; Bornstein and Rapoport 1988).

The justification of the Nash equilibrium in terms of preplay communication seems reasonable in the present context. Workers may get together, say, each morning at coffee break and agree upon the efforts to be expended by each member, thereby facilitating the collectively rational outcome. Before starting the workday, the workers face a coordination game, which can be resolved through such preplay communication. Working hard may become a convention, which it is individually rational to abide by, provided the others also do so (see Lewis 1969, chap. 1; Leibenstein 1987, chap. 7). The first justification, in terms of a dynamic adjustment process, seems less relevant for the current purpose. A trial-and-error process, in which each worker silently chooses her action, observes the actions of everyone else, then chooses a new action, and so on, until the process converges, appears artificial in a context in which direct cooperation through communication is possible.

APPENDIX B

Proof of Equations (5)–(7) and (9)–(10)

Define first

$$\bar{x}_{-i} = \frac{1}{n} \sum_{j \neq i} x_j, \tag{B1}$$

and the general reward rule

$$w_i = g(x_i; \bar{x}_{-i}), \tag{B2}$$

where $g_1 \equiv \partial w_i / \partial x_i > 0$ and $g_2 \equiv \partial w_i / \partial \bar{x}_{-i} > 0$. An individual piece-rate scheme obtains when $g_1 = \beta$ and $g_2 = 0$, which corresponds to equation (3). A group piece-rate scheme obtains when $g_1 = \beta / n$ and $g_2 = \beta$, which corresponds to equation (3) with the average group output $\bar{x}$ from (4) substituted for x_i.

The condition for collective rationality under team interdependencies, under either an individual or a group piece-rate scheme, obtains by solving the program

$$\max_{a_i, \mathbf{a}_{-i}} P\{g[f(a_i, \mathbf{a}_{-i}); (1/n) \sum_{j \neq i} f(a_j, \mathbf{a}_{-j})]\} - V(a_i), \tag{B3a}$$

subject to the $n - 1$ constraints

$$P\{g[f(a_j, \mathbf{a}_{-j}); (1/n)\sum_{k\neq j} f(a_k, \mathbf{a}_{-k})]\} - V(a_j) \geq U_j^* \quad \text{for each } j \neq i, \tag{B3b}$$

where the relationships in equations (1), $x_i = f(a_i, \mathbf{a}_{-i})$, and (B1) have been used when inserting the reward rule (B2) into the preferences of equation (2); that is, into $U(w_i, a_i) = P(w_i) - V(a_i)$. In (B3*b*), U_j^* is the minimum payoff the worker is willing to accept, the payoff she must at least receive in order to be willing to participate in the arrangement.

The Lagrangian for the problem is

$$\begin{aligned} L(a_i, \mathbf{a}_{-i}) = {} & P\{g[f(a_i, \mathbf{a}_{-i}); (1/n)\sum_{j\neq i} f(a_j, \mathbf{a}_{-j})]\} - V(a_i) \\ & - \sum_{j\neq i} \lambda_j \cdot P\{g[f(a_j, \mathbf{a}_{-j}); (1/n)\sum_{k\neq j} f(a_k, \mathbf{a}_{-k})]\} - V(a_j) - U_j^*. \end{aligned} \tag{B4}$$

Since we consider n symmetrical workers, we get $f_d(a_i, \mathbf{a}_{-i}) = f_d(a_j, \mathbf{a}_{-j})$ for $d = 1, 2$ (i.e., the derivatives f_1 and f_2) and all i, j, when $a_i = a$ for all i. The n first-order conditions for a maximum are

$$\begin{aligned} \partial L/\partial a_i = {} & P' \cdot [g_1 f_1 + g_2 f_2 (n-1)/n] - V' \\ & - \sum_{j\neq i} \lambda_j P' \cdot [g_1 f_2 + g_2 f_1/n + g_2 f_2 (n-2)/n] \\ = {} & 0, \end{aligned} \tag{B5a}$$

$$\begin{aligned} \partial L/\partial a_h = {} & P' \cdot [g_1 f_2 + g_2 f_1/n + g_2 f_2 (n-2)/n] \\ & - \lambda_h \cdot \{P' \cdot [g_1 f_1 + g_2 f_2 (n-2)/n] - V'\} \\ & - \sum_{j\neq i,h} \lambda_j P' \cdot [g_1 f_2 + g_2 f_1/n + g_2 f_2 (n-2)/n] \\ = {} & 0 \quad \text{for each } h \neq i. \end{aligned} \tag{B5b}$$

Subtracting (B5*b*) from (B5*a*) yields

$$\lambda_h = -1 \quad \text{for each } h \neq i. \tag{B6}$$

Then inserting (B6) into (B5*a*) yields, after some rearrangement, the condition for collective rationality as:

$$\begin{aligned} V'/P' = {} & g_1 f_1 + g_1 f_2 (n-1) + g_2 f_1 (n-1)/n \\ & + g_2 f_2 (n-1)^2/n \quad \text{for each } i. \end{aligned} \tag{B7a}$$

Under a group piece-rate scheme, $g_1 = \beta/n$ and $g_2 = \beta$, whereas under an individual piece-rate scheme, $g_1 = \beta$ and $g_2 = 0$. In both cases the condition for collective rationality in (B7*a*) reduces to

$$\frac{V'}{P' \cdot [f_1 + f_2(n-1)]} = \beta \quad \text{for each } i, \tag{B7b}$$

which proves equation (5).

When there are no interdependencies between workers—that is, $f_2 = f_{22} = 0$—the condition for collective rationality under an individual or group piece-rate scheme follows as a special case of (B7*a*) or (B7*b*):

$$\frac{V'}{P'f_1} = \beta \quad \text{for each } i, \tag{B7c}$$

which proves equation (9).

Note that the formulation in (B7) allows for mixed incentives, where part of the wage depends on the group output and another part on the individual contribution (e.g., as described in Lupton [1963, pp. 104–5]).

The condition for individual rationality under either a group piece-rate scheme or an individual piece-rate scheme obtains by solving the program

$$\max_{a_i} P\{g[f(a_i, \mathbf{a}_{-\mathbf{i}}); (1/n) \sum_{j \neq i} f(a_j, \mathbf{a}_{-\mathbf{j}})]\} - V(a_i), \tag{B8}$$

where worker i, in solving (B8), takes $\mathbf{a}_{-\mathbf{i}}$ as given.

The solution to the program yields the condition

$$\frac{V'}{P'} = g_1 f_1 + g_2 f_2 (n-1)/n \quad \text{for each } i. \tag{B9a}$$

Under a group piece-rate scheme—that is, when $g_1 = \beta/n$ and $g_2 = \beta$—(B9*a*) reduces to

$$\frac{V'}{P' \cdot [f_1 + (n-1)f_2]} = \beta/n \quad \text{for each } i, \tag{B9b}$$

while under an individual piece-rate scheme—that is, when $g_1 = \beta$ and $g_2 = 0$—(B9*a*) reduces to

$$\frac{V'}{P'f_1} = \beta \quad \text{for each } i. \tag{B9c}$$

Equations (B9*b*) and (B9*c*) prove equations (6) and (7), respectively.

The condition for individual rationality under a group piece-rate scheme in the absence of interdependencies follows as a special case of

(B9*a*), when $f_2 = 0$, $g_1 = \beta$, and $g_2 = 0$:

$$\frac{V'}{P'f_1/n} = \beta \quad \text{for each } i. \tag{B9d}$$

Equation (B9*d*) proves equation (10).

In the discussion of how the group piece-rate scheme under interdependencies called forth less effort than optimal, I relied on the fact that the ratio on the left-hand sides of (5*a*) and (6*a*) (i.e., $V'/P' \cdot [f_1 + (n-1)f_2]$) decreases when a_i decreases. Formally, this is seen by

$$\begin{aligned} \frac{\partial}{\partial a_i}\left\{\frac{V'}{P' \cdot [f_1 + (n-1)f_2]}\right\} &= \frac{1}{\{P' \cdot [f_1 + (n-1)f_2]\}^2} \\ &\quad \times \{V''P' \cdot [f_1 + (n-1)f_2] \\ &\quad - V' \cdot \{P'' \cdot [f_1 + (n-1)f_2] + P' \cdot [f_{11} + (n-1)f_{22}]\}\} > 0, \end{aligned} \tag{B10}$$

which shows that the ratio $(V'/P' \cdot [f_1 + (n-1)f_2])$ decreases as a_i decreases.

Similar relationships hold for individual piece-rate schemes under interdependencies and for group piece-rate schemes in the absence of interdependencies. In the case of an individual piece-rate scheme under interdependencies, the condition for individual rationality is given in (7), while that for collective rationality is given by (5*b*), as before. The left-hand sides of (7) and (5*b*) contain the same expressions, while the right-hand side of (7) is smaller than that of (5*b*), when $0 < f_2 < f_1$. Hence, for the equalities to hold, the left-hand side of (7) must be smaller than that of (5*b*). This occurs only when the effort-level $\hat{a}_{IPR}$ in the individually rational solution is smaller than that in the collectively rational solution. Formally, this can be seen by taking the derivative of the left-hand side of (7), with respect to a_i, which follows as a special case of (B10) when $f_2 = f_{22} = 0$:

$$\frac{\partial}{\partial a_i}\left(\frac{V'}{P'f_1}\right) = \frac{V''P'f_1 - V' \cdot (P''f_1 + P'f_{11})}{(P'f_1)^2} > 0, \tag{B11}$$

which shows that the ratio $V'/P'f_1$ decreases as a_i decreases.[27]

That less effort is expended under a group piece rate than an individual piece-rate scheme in the presence of interdependencies can be seen by first noting that the left-hand sides of (6*b*) and (7) contain the same expressions, while the right-hand side of (6*b*) is smaller than that of (7), when $0 < f_2 < f_1$. Hence, for the equalities to hold, the left-hand side of (6*b*) must be smaller than that of (7). This occurs only when the effort-level $\hat{a}_{GPR}$ under the group piece-rate scheme is smaller than the effort-level $\hat{a}_{IPR}$ under the individual piece-rate scheme. Formally, this can be seen by the fact that the derivative of the left-hand side of (6*b*) with respect to a_i is given by (B11).

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[27] Note that the inequality in (B11) also could have been used to prove the result that in the presence of interdependencies less effort is expended under the group piece-rate scheme than in the collectively rational solution. One would then have had to take one's point of departure in equations (5*b*) and (6*b*), both of which contain the ratio $V'/P'f_1$ on their left-hand sides.

Finally, in the case of a group piece-rate scheme in the absence of interdependencies, the individually rational solution is given by (10), while the collectively rational solution by (9). The left-hand sides of (9) and (10*b*) contain the same expressions. The right-hand side of (10*b*) is smaller than that of (9), β/n versus β. So, as above, for the equalities to hold, the left-hand side of (10*b*) must be smaller than that of (9). This occurs only when the effort-level $\hat{a}_{GPR}$ in the individually rational solution is smaller than the effort-level $\hat{a}_{CR}$ in the collectively rational solution. Formally, this can be seen by the fact that the left-hand side of (10*b*) decreases as a_i decreases. Since the left-hand sides of (10*b*) and (7) contain the same expressions, the proof of this property is given in (B11).

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Children's First Names: An Empirical Study of Social Taste[1]

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Data on births in New York State between 1973 and 1985 are used to analyze gender differences in naming patterns. In a relatively rigorous way, the authors infer how seemingly idiosyncratic expressions of tastes in names are in general affected by underlying cultural themes. For example, the results suggest that long-standing stereotyped role assignments still have a subtle but major effect on the naming process. Two major parental characteristics—education and race—modify these general patterns. In turn, taste differences in subpopulations reveal their general esthetics dispositions. The authors present these results as the first large-scale systematic comparison of educational differences in naming patterns in the United States.

Why do specific tastes take the form that they do? Obviously there is no simple answer to this question, if only because many idiosyncratic personal experiences affect individual choices. Nevertheless, in advanced industrialized societies, most expressions of taste and fashion are affected by formal organizations that have a stake in the outcome and are dedicated to influencing such tastes. For example, designers, manufacturers, and retailers all play important roles in influencing clothing fashions (Barber and Lobel 1953; Simmel 1957; Blumer 1969). Similarly, there are commercial organizations and/or social institutions that directly affect a wide variety of physical and conceptual products such as furniture, foods, hairstyles, music, recreation, housing, graphic arts, architecture,

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books, and the like. Even highbrow and "noncommercial" forms of art are affected by organizations dedicated, in part or whole, to molding acceptable outcomes or distributing the products (e.g., Becker 1982, chap. 4; Crane 1987), to say nothing of popular forms of art (see Peterson and Berger 1975).

Nevertheless, there is good reason for viewing tastes and fashion as more than merely the product of formal organizations—important as they may be. For example, the consistent long-term shifts in clothing patterns described in the classical studies by Kroeber (1919) and Richardson and Kroeber (1940) cannot be readily explained as the product of arbitrary decisions by fashion leaders or other organizations. Rather, there are almost certainly both underlying cultural attributes and taste mechanisms that lead to changes. Many organized efforts to affect tastes fail, and not simply because of organizational ineptitude. An excellent example is

> the effort in 1922 to check and reverse the trend toward shorter skirts which had started in 1919 to the dismay of clothing manufacturers. These manufacturers enlisted the cooperation of the heads of fashion houses, fashion magazines, fashion commentators, actresses, and acknowledged fashion leaders in an extensive, well organized and amply financed campaign to reverse the trend. The important oracles of fashion declared that long dresses were returning, models of long dresses were presented in numbers at the seasonal openings, actresses wore them on the stage, and manikins paraded them at the fashionable meeting places. Yet, despite this effective marshalling of all significant sources of prestige, the campaign was a marked failure; the trend toward shorter skirts, after a slight interruption, continued until 1929 when a rather abrupt change to long dresses took place. [Blumer 1969, p. 281]

Similarly, Lurie (1981, p. 12) describes the failure of the maxiskirt to succeed, despite a major campaign on its behalf. Her interpretation is that the style failed because it clashed with cultural themes of the period. Another example is the decline of dress hats for both men and women in recent decades, a trend that has persisted despite the efforts of an industry with a clear interest in maintaining what had been a long-standing practice (Lurie 1981, pp. 176–81; Schudson 1984, pp. 39–40; Tenner 1989, pp. 21–26). In general, Schudson (1984, pp. 36–43) shows that the failure of intensive advertising efforts to affect consumer tastes is a widespread and common phenomenon, with the Edsel perhaps one of the most famous examples.

Given that tastes often shift in broad and orderly ways and, moreover, that there are periods in which tastes are highly resistant to organized efforts to mold them, it is reasonable to assume that fashions reflect other underlying cultural conditions and the operation of mechanisms that are not simply determined by organizational efforts. Two nonorganizational influences on taste are widely recognized: the effect of material wealth (described by Veblen [1967, chap. 4] as "conspicuous consumption") and the display and use of esoteric knowledge among the highly educated (the "cultural capital" described by Bourdieu [1977, 1984]).[2] Indeed, at the very least, institutional influences must themselves take these factors into account. In the case of conspicuous consumption, for example, Davis (1958, p. 11) observes, "esthetic norms and pecuniary norms are highly correlated in this society, for it is remarkable how, despite constant change in taste, a material object considered to be in 'good taste' at a given moment is also hideously expensive."

Important as the concepts of conspicuous consumption and cultural capital are for helping us understand tastes, it is hard to believe they are the only ones. Blumer's comments are still largely true: "The origin, formation, and careers of collective taste constitute the huge problematic area in fashion. Major advancement in our knowledge of the fashion mechanism depends on the charting of this area" (cited in Blumer 1969, p. 284). These significant questions about the cultural underpinnings of tastes are difficult precisely because institutions and organizations also seek to affect tastes. As a consequence, it is hard to find areas that are not susceptible to organized efforts and it is difficult to determine if the results are due to purely cultural factors as contrasted with organizational efforts. This is precisely the goal of this article, to examine *not* the organizational influences or symbolic displays of wealth or breeding, but rather to determine if there are *intrinsic* functions to taste. For example, if subsets of a society differ in taste, it is one matter to determine the function of taste as a categorical marker, it is another matter to determine why tastes take the form that they do. Viewed solely as a categorical marker, preferences could be in the opposite direction and they would still serve as a marker of social categories. But this would not be the case if there are intrinsic reasons for the preferences to take the direction that they do above and beyond their marker function.

Sociologists in the United States rarely study first names despite their distinctive theoretical potential and despite the existence of excellent large-scale data sets as well as obvious opportunities for surveys and field studies (exceptions include Miller [1927], Rossi [1965], Taylor [1974], Lieberson [1984], and Alford [1988]).[3] Yet, the first names given to children provide a rare opportunity to study tastes in an exceptionally rigorous and systematic way that is relatively free of organized efforts to determine the outcome. Industries linked to children are not affected by the names given to them—only by whether a child is born or not. There are, of course, certain religious rules operating for Roman Catholics and Jews about the choice of names, but these still allow followers a variety of options (the former can choose from an enormous number of saints and may meet the obligation with an appropriate middle name, and the latter now tend to select a name that has the same first letter as that of a deceased ancestor). Hence, although institutional activities such as the mass media and popular culture can and do affect naming practices, such influences are an unintended by-product rather than an organized effort to direct and mold tastes. The naming activity is ultimately a social process, and the resulting pattern of name usage reflects the combined influence of the imagery associated with each name, the notions parents have about the future characteristics of their children, estimates of the response of others to the name, the awareness and knowledge of names through the mass media and other sources, parents' beliefs about what are appropriate children's names for persons of their status, and institutionalized norms and pressures. As the role of the extended family, religious rules, and other institutional pressures decline, choices are increasingly free to be driven by factors of taste, and the possibilities are substantial that the choices will also reflect differences in taste between subsets of the population.

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[2] As Lamont and Lareau (1988) point out, the term has actually been used in a variety of ways by Bourdieu himself as well as by others.

[3] By contrast, a review of social science research on personal names by Lawson (1984) discloses a rich literature on first names in psychology, anthropology, history, and psychiatry. In addition, there are some sociological and demographic studies of naming outside the United States.

The theoretical concern, in brief, hinges on a fact and a question. The fact is that names may demarcate subgroups of a society along such lines as gender, race, class, ethnicity. They can also signal shifts in assimilation and identification. However, aside from the demarcation function, are there any *internal* reasons for each subgroup to select those specific names? We can readily visualize a variety of ways that lead subpopulations to differ in their naming patterns without reference to the substance of taste. There are the obvious group differences in both their initial exposure to names in the mass media and popular culture and, in turn, communication networks that would affect their spread. For example, groups vary in their exposures to names in movies, television programs, daytime soaps, musicians, and popular stars. Likewise, given the existence of various forms of residential and social segregation (between races, and by social class within ethnic and racial subgroups), there will be different transmission networks. There is also the possibility, raised in earlier theoretical work on fashions in clothing, that one class will imitate another class, particularly when the latter is of higher status. Moreover, tastes can have historical roots so that intrinsic reasons exist for a group initially to use certain names or avoid others. However, each name has a life of its own that endures long after the original reasons have disappeared or are even known to members of a group (see Lieberson 1985, pp. 76–83). The taste for difference sometimes leads counterculture groups to pursue that which is unpopular to the broader society, particularly if it is shockingly different. Finally, group differences in taste are no doubt sometimes accidental; namely, one group develops an affinity for a certain taste without there being any intrinsic reason for doing so. When interaction networks run along group lines, such initial "chance" differences may spread out among one group but not another. (The "accidental" interpretation has a certain facile residual quality to it, but it is not to be ignored unless one takes a totally deterministic view of tastes.)

All of these notions are sound, or at least plausible, but they do make a critical assumption; namely, they ignore the intrinsic nature of the specific name chosen. From the perspective of marking one class or ethnic/racial group from another, for example, it would really not matter what specific name is chosen as long as it is different from another group. Yet all of this overlooks the fact that groups can differ on a wide range of underlying social conditions and experiences that in turn can affect tastes; these conditions could include, for example, education, skills, values, norms, styles, position in the social structure, and geographical location. This experiential base influences the ability of subgroups to appreciate, understand, or otherwise resonate to different stimuli. Put another way, there is exposure to many names, yet not all names become popular. Aside from the influence of the factors discussed above, are there internal reasons for some names catching on with one subgroup and other names with another? In this article, we will examine gender, class, and racial differences in names to determine if there is evidence to indicate that the *intrinsic content* of the names affects the choices.

This article has a rather straightforward goal in examining first names: we wish to open up the difficult question of nonorganizational influences on taste, moving the topic from a seemingly evasive and insurmountable issue to one that can receive relatively rigorous empirical study. To accomplish this end, we consider differences in the tastes *underlying* the names chosen for boys and girls and then examine how education and race, in turn, affect these general themes. In each case, we examine the observed patterns—which are themselves rarely studied—to infer underlying cultural processes that influence the formation of tastes in society. Our data base consists of a 6.25% random sample of all names given to white or black children born in New York State between 1973 and 1985—a total of 193,142 births.[4]

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GENDER DIFFERENCES AMONG WHITES

The analysis of gender differences in the naming process is inspired by Alice Rossi's (1965) pioneering study of the names given to children in white middle-class families living in and near Chicago. Rossi views names as an empirical index permitting investigation of "the inner core" (p. 500) of kin relations. She found gender differences in the sources of the names given to children; in particular, boys were far more likely to be named after kin than were girls, suggesting that "men are the symbolic carriers of the temporal continuity of the family" (p. 503). Moreover, boys' names were found to be "more traditional, less currently fashionable than girls' names" (p. 504). Given the many changes in gender relations (at least on a formal, institutional basis) that have occurred in the 30 years since Rossi's survey was conducted,[5] important empirical questions exist. Do gender differences in the naming process still persist? If so, can one infer any symbolic themes underlying these differences by examining the choices made by parents?

With the exception discussed above, the actual names given to children are not of primary concern in the Rossi study; rather, it focuses on gender differences in the sources of names, for example, relatives, friends, and so on. However, the birth certificate data we use here provide a large enough set of cases to permit us to look more deeply at the names themselves. We can ask if there are gender differences in the kind of names used for boys and girls. In turn, if there are differences in the nature of their names, are we able to infer certain esthetics underlying these differences? Keep in mind Rossi's point that names are a far more subtle indicator of gender relations than would be obtained through either an attitudinal survey or even an examination of practices in formal settings. Since the birth data we use include information on race and class, it is possible to widen the analysis of gender to include the combined influences of these factors as well.

The naming patterns for white boys and girls differ in some very simple and basic ways (the data for blacks are discussed below). First of all, there are differences between girls and boys in the concentration of their names. Between 1973 and 1985, in New York State, the 20 most common

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providing the first eight letters of the child's given name for all live births, the data also give each child's sex, race of mother, month of birth, year of birth, and maternal education. The actual number of usable names is somewhat smaller because in some cases children were unnamed on the birth certificate or information on the education of the mother was unavailable. Racial categories other than white and black are too small in number for analysis.

[5] Rossi's naming data are, on average, even older since they refer to names given during the 40 years preceding her survey (see Rossi 1965).

TABLE 1

LEADING NAMES, BY RACE, OF CHILDREN BORN IN NEW YORK STATE, 1973–85

Girls				Boys			
White	%	Black	%	White	%	Black	%
Jennifer	4.2	Nicole	1.6	Michael	5.7	Michael	2.2
Christine	3.1	Tiffany	1.4	Christopher	3.4	Jason	1.6
Jessica	2.3	Ebony	.9	John	2.7	Anthony	1.6
Melissa	2.0	Jennifer	.9	David	2.6	Christopher	1.5
Michelle	1.9	Latoya	.9	Matthew	2.6	James	1.5
Nicole	1.8	Monique	.8	Joseph	2.6	Robert	1.4
Sarah	1.7	Kimberly	.7	Brian	2.6	David	1.3
Lisa	1.3	Natasha	.7	Jason	2.6	Sean	1.3
Elizabeth	1.3	Christine	.7	Daniel	2.4	Kevin	1.0
Amy	1.3	Michelle	.7	Robert	2.3	William	1.0
Heather	1.2	Crystal	.7	James	2.1	John	1.0
Stephanie	1.2	Melissa	.7	Steven	2.0	Joseph	.9
Kimberly	1.1	Danielle	.6	Thomas	1.6	Steven	.8
Amanda	1.1	Erica	.6	Eric	1.6	Andre	.8
Kelly	1.1	Stephanie	.6	William	1.6	Eric	.8
Danielle	1.1	Jessica	.5	Anthony	1.4	Charles	.8
Catherine	1.0	Tamika	.5	Sean	1.4	Brian	.7
Rebecca	.9	Lisa	.4	Mark	1.4	Richard	.7
Laura	.9	Elizabeth	.4	Jeffrey	1.3	Duane	.7
Rachel	.9	Tanya	.4	Richard	1.3	Kenneth	.6
Total births:							
71,422		17,071		75,163		17,762	

NOTE.—Total births refers to a 6.25% sample of births with an actual first name reported on the birth certificate.

boys' names were given to 45% of the white boys in the survey; for the 20 most common names for white girls that figure is only 31%. (The 20 most popular names and the percentages of children receiving them are reported in table 1.)[6] The leading name during this period, Michael, was given to 5.7% of all male births, whereas the name Jennifer was given to 4.2% of newborn girls. This edge holds for all 20 leading names. For example, the 1.3% held by the nineteenth and twentieth most popular names for boys (Jeffrey and Richard), would put them in the top 10 for girls. There is also a small gender difference with respect to unique names—"unique" being defined as a name reported in the sample for only one child of the same sex and race during the entire 13-year period. Among whites, 4% of the boys and 6% of the girls have unique names. In some cases the names are unique only because of intentionally distinctive spelling, as is probably the case for the one "Elayne" or the single "Janiss"—although it is impossible to rule out spelling errors. In some cases the unique name may simply be a standard name for a particular immigrant ancestry; in other cases, the unique choices are indeed correctly spelled names that are part of the existing stock, but are simply unpopular at present. For example, only one white boy in the sample was named Seymour during this span. Sometimes, a unique name appears because parents have chosen a nickname or diminutive form of a standard name, e.g., Stevie. Our inspection of these names, however, leads us to the conclusion that very often these unique names are innovations created by the parents themselves and not merely a spelling variant of an existing first name. The significance of this attribute will become more apparent when we briefly consider black names, but it is important to note that whites are more likely to make up a name for girls than for boys.

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[6] As much as possible, spelling variants are combined, with the criterion being whether the pronunciation of the name is the same even if the spelling is different. The New York State data only gives the initial 8 letters of a first name. This causes no difficulty other than it prevents distinguishing Christine from Christina (we arbitrarily used the former).

The names chosen for boys and girls have sharply different etymological origins, despite the obvious fact that every linguistic or cultural group has both boys' and girls' names.[7] The most striking gender difference occurs in the use of French names (including one Norman name). French is the source of 11 of the 50 most common girls' names, but the source of only one of the 50 leading boys' names (Dennis). Leading girls' names are also far more likely to have no historical-name roots at all; six of the top girls' names are either diminutive or pet names that now are used as formal names (e.g., Lisa from Elizabeth, Carrie from Caroline), and an additional three cases were either invented (Vanessa), or derived from a plant or town (respectively, Heather and Kimberly). Among boys, by contrast, only one of the names is a diminutive (Frank, a pet form of Francis). (Both genders, by the way, have a small number of names that are derived from surnames, e.g., Lindsay.) There is also a slightly greater disposition to give girls Latin names (nine for girls as opposed to six for boys), but that is too small a difference to draw any conclusions. In turn, boys have several distinctive cultural origins for their names: 12 from Greek, 11 from Hebrew, and 6 from English—far in excess of the 4, 6, and 1, respectively, for girls. Note that these gender differences cannot be attributed to ethnic factors within the white population since obviously the ethnic distribution of parents living in the same area has to be identical for boys and girls.

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[7] Etymologies were obtained from Dunkling and Gosling (1983) for the 50 most common boys' and girls' names in New York for the period under consideration. Bear in mind that there is sometimes a certain arbitrariness in the allocation of a specific origin since some names have a long chain of changes. However, the same source is used for both boys' and girls' names. The first etymological origin is generally used in those cases where more than one is reported for a given name.

Do the leading boys' and girls' names differ in the rapidity of their changes over time? Our analysis is based on a list compiled from the top 20 names found in 1973 and/or 1985.[8] Cramer's *V* summarizes the level of association between leading names and year.[9] This *V* is a chi-square-based measure of association that scales (between 0 and 1.0) the observed chi-square value in terms of its possible range based on the *N* and the degrees of freedom. (Chi-square itself compares the observed distribution with what would be expected if the variables were independent, but its value is affected by variations in the size of *N*.) In effect, Cramer's *V* expresses the observed chi-square value as a percentage of the possible range of values given the total *N* and the number of columns and rows in the analysis. A value of zero would occur if the chi-square value was the minimum possible and hence there was no difference between periods or social classes, or race in the distribution of names. A value of 1.0 occurs if there is a maximum possible association between name and the cross-tabbed variable (in this case, year). For a discussion of *V* and related measures of association, see Bishop, Fienberg, and Holland (1975, pp. 385–93).

The results are very clear: there is less stability and more turnover in the top 20 names given to girls during the span under consideration; *V* is .40 and .24, respectively, for girls' and boys' names. These "standardized" chi-square values indicate that the most popular names for girls are more closely associated with year; that is, they change more between 1973 and 1985 than do boys' names. The disposition toward new names for girls shows up not only with respect to unique names but also, almost paradoxically, with respect to the leading names for girls. In general, girls' leading names are more likely to be of a novel or unconventional origin such that they were not part of the existing repertoire of standard first names. *American Given Names* (Stewart 1979) provides a useful listing of names judged to be part of the standard stock in the United States. The book was written in 1977 (p. 41) and its lists of names appear to be somewhat dated. As a consequence, we can take the presence of a name in this listing as meaning that the name would have been considered part of the conventional stock of names. (Whether popular or not at the time is a separate matter; e.g., in New York State over a 13-year span there were only three baby girls in the sample named Agatha—yet such a name is appropriately part of the standard name repertoire.)

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[8] This procedure is used throughout the study; e.g., when two classes are compared, a list is likewise generated that is based on the leading names in each group. (Again the magnitude of the statistic reflects equal weights for the two variables.)

[9] Product-moment correlations are inappropriate because of the shape of the distributions being correlated. Spearman rank order correlations avoid these parametric assumptions, but they ignore more of the information than does Cramer's *V*. However, ρ was also computed and is consistent with the conclusions reported here.

There is a gender difference in the disposition to use novel names. Among white boys, 45 of the leading 50 names are in the Stewart directory, with the comparable figure for white girls being only 35 (see App. tables A1 and A2). Likewise, longitudinal results tend to validate both this cross-sectional conclusion and the utility of the Stewart-based index. Consider those names in the top 20 for each gender at the end of the period (1985) that were not in the top 20 at the beginning of the period (1973). Among the seven names new to the top 20 list for white boys at the end of the period, all but one was in the Stewart book; by contrast, five of the 10 new names for white girls are not listed in Stewart.

The phonemic shape of boys' and girls' names overlap in many respects, as one might expect, but there are also some striking differences in the actual sounds of the names preferred for these groups. These occur in the initial sounds, endings, and elsewhere in the name. Consider, for example, gender differences in the endings. Among the 100 names most often given to white girls, 34 end in a "schwa-like" sound, as in Jessica or Sarah; by contrast a schwa ending is rare for boys, being found in only one of the 100 leading white boys' names (Joshua). A long "*ee*" sound (as in Amy and Stephanie) is also a more common ending in names given white girls, although the gap is far less extreme (28 vs. 10). By contrast, 87% of the leading names given to white males end in a consonant sound, more than double the rate occurring among females. Names ending with an "*n*" sound are an exception, however, being equally common for girls (as in Christine, Catherine, and Megan) and boys (in both cases about one-fourth of all names). These phonemic patterns at least in part reflect differences in gender-based endings found in many languages. Their true significance will be clearer when we consider class differences in tastes.

Interpretation

The different naming patterns for boys and girls can be explained in a relatively straightforward way by drawing on historically rooted conceptions of the roles of males and females in society. Rossi (1965) finds that males are more likely than females to be named after kin, suggesting that, "women play the more crucial role in family and kin activities, while men are the symbolic carriers of the temporal continuity of the family" (Rossi 1965, p. 503). Alford's (1987, chap. 6) investigation of naming in southeastern Oklahoma reports similar results, leading him to conclude that "parents are more likely to give kin names to boys than to girls, to symbolize and encourage their sons' roles as perpetuators and symbols of family continuity and prestige" (p. 132). Also, the naming of sons after fathers was once fairly common in parts of the United States (see Taylor 1974), whereas first name continuity between mother and daughter is rare.

From this perspective, the divergent tastes underlying naming patterns for boys and girls reflect a tendency to assign a lesser role to women, such that their names are relatively more decorative and thereby open to fashion. As a consequence, there is more turnover in girls' names, they are more likely to be made up, they are less conventional (in the sense of being found in Stewart's list), they are less imposing in the sense that a number of the new names are nothing more than adaptations of previously diminutive forms such as Lisa or Carrie, and their origins are more likely to be French, as opposed to earlier biblical or Greek sources. (One explanation for this last result is that images of the French in the United States are associated with such attributes as fashion, cuisine, style, and culture—matters that may lead parents to use such French names for their girls but avoid using them for boys.) The lesser concentration of girls' names may reflect a larger and more rapidly expanding pool of potential names since there is more "play" with girls' names and innovation is more often acceptable. In addition, there is a long-standing gender difference in the attractiveness of a common name; relatively speaking, novelty is more appealing for girls and traditional names more appealing for boys (see Allen et al. 1941). These gender differences are not simply a function of the greater propensity to use ancestral sources for the naming of boys. The reader will recall that names on the top 20 list for boys at the end of the period (but not at the beginning) were far more likely to have been in Stewart's directory, as compared to new leading girls' names. Thus, even when there is turnover, the new leading names for boys are more likely to reflect a taste for "old-fashioned" names. By contrast, newly popular girls' names are less likely to be derived from the existing repertoire of names since it is less critical that they be "traditional" in the sense of having roots in society.

A Brief Note about Black Names

An extensive analysis of black names merits a separate paper, but the data on black names in table 1 do help illuminate the gender issues in general, for whites as well. Table 1 shows a modest overlap between the 20 most common names given to white and black girls born in New York State between 1973 and 1985; the top six names for white girls are all among the top 20 for blacks. Jennifer, the name most commonly given to white girls, is in fourth position among blacks; Nicole, the name most commonly given to black girls, ranks sixth among whites. However, four of the top six black names do not even make the top 20 among whites. This asymmetry reflects the fact that blacks are acquainted with both the larger culture of the society and their own black subculture.[10] Names that are very attractive to whites are more likely to also have at least some attraction for blacks, whereas the converse does not hold—names may be highly important for blacks without influencing the larger, dominant white culture. As a consequence, there is considerable dissimilarity in the leading names, with nine of the top twenty being different in each group. Whites chose Sarah, Amy, Heather, Amanda, Kelly, Catherine, Rebecca, Laura, and Rachel: blacks favored Tiffany, Ebony, Latoya, Monique, Natasha, Crystal, Erica, Tamika, and Tanya. Cramer's *V* between the leading names for blacks and whites is .49. (This is a rather high value of *V*, compared to others encountered here, suggesting a relatively large effect on names, in this case the racial gap in the names given to girls.)

By contrast, the situation is substantially different for boys during this period. Fifteen of the top 20 names are the same for blacks and whites; the *V* between groups is .23. To be sure, even an index of .23 means that there still are, of course, many important differences. Kevin, Andre, Charles, Duane, and Kenneth are not among the top 20 names given white boys; Matthew, Daniel, Thomas, Mark, and Jeffrey are, by contrast, ranked lower among blacks. Moreover, names on both lists exhibit substantial differences in their relative importance; for example, Anthony is more common as a name for blacks than as a name for whites.

This gender pattern for black-white comparisons is harmonious with our interpretation of gender differences *within* the white population, namely, the existence of a generally more conservative view toward male names. As a consequence, even subcultural differences—here between blacks and whites—are sharper for girls than boys. In similar fashion, although blacks are far more likely than whites to have unique names, this disposition, as for whites, is stronger for girls. Among blacks, 19% of boys and 31% of girls have unique names during the entire 13-year period. The data in table 1 and the observations about both the associations with and the tendency toward unique names are sufficient to make it clear that a sharp cultural gap exists between blacks and whites in their naming patterns. We cannot delve into the reasons here, but clearly the gender gap remains even as a race effect operates.

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[10] The use of the term "white culture" refers to the aggregate of differences between the whites and other populations; it does not imply the absence of ethnic and regional subcultures within this aggregate.

THE INFLUENCE OF EDUCATION

As Gans (1974, pp. 70–71) observes, education is probably the most important influence of class on taste. Are the names given to children influenced by parental education? If socioeconomic position *does* influence tastes in names, this will provide a rare opportunity to determine how taste is structured when neither income, nor occupation, nor formal education constrain the choices—unlike most other settings where these factors have a direct bearing (Sobel 1981; Bourdieu 1977). There are studies of class influence on naming in other countries (see Frank [1977] and Besnard and Desplanques [1988] for class influences in Germany and France, respectively). By contrast, to our knowledge there are no systematic studies of how any major socioeconomic component influences naming patterns in the United States.[11]

Educational Differences

How similar are the names given to children by women with different educational levels?[12] Table 2 gives Cramer's *V* between white women classified by their educational attainment. Without exception, the distribution of leading names tends to vary by similarity of mother's educational background. In other words, there is a gradient in the matrix such that *V* increases when names are compared with women of progressively more distant levels of education. This pattern is found for both boys' and girls' names. The high school/college gap is not an exceptionally sharp divide when it comes to naming practices; for both boys and girls, the names given to children by mothers with a high school education have about the same level of Cramer's *V* with women who have 1–3 years of high school as with women who started college.

Mothers with only an elementary school education have somewhat

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[11] An exception is the suggestive ecological effort at examining class differences in naming through comparisons between Ann Arbor and Detroit (Evans 1983).

[12] There is no reason to assume either that women alone name the children (see, e.g., Alford 1987, pp. 126–29) or that education is the only SES characteristic affecting tastes. However, the New York data provide information only on the education of mothers. (In general, this is the case; birth certificate data are more likely to provide information on the mother than the father, and more likely to include information about education than for occupation or income. The father is not always known or registered and it is much easier for health providers to obtain usable education data compared with other SES characteristics.)

TABLE 2

CRAMER'S *V*, LEADING WHITE NAMES, BY EDUCATION OF MOTHER

Years of Schooling	0–8	9–11	12	13–15	16	17+
Elementary, 0–8	. . .	.23	.33	.48	.53	.54
High school, 9–11	.31		.15	.26	.30	.30
High school, 12	.32	.14		.11	.14	.16
College, 13–15	.44	.26	.11		.07	.11
College, 16	.50	.40	.23	.15		.09
Postcollege, 17+	.52	.42	.24	.18	.09	. . .

NOTE.—Information pertaining to boys' names appears above the diagonal; information for girls', below the diagonal. For all boys' values, $P \leqq .0001$; for all girls' values, $P \leqq .0085$.

distinctive naming preferences; *V* is relatively large between this group and mothers in other educational levels, even with women completing 1–3 years of high school. This pattern need not necessarily mean that minimally educated white women have a propensity for relatively distinctive naming patterns. The prominence of Carlos, Jose, Juan, Luis, Miguel, Carmen, Maria, and Rosa among their children's names raises the question of whether the unique results for the least-educated mothers is affected by a concentration of Hispanics in the category. Since Latinos cannot be analyzed separately with the New York State name data, we use an indirect test based on an examination of 1980 census data. For women in a major childbearing age (20–24) the number in New York State who are of Hispanic origin is probably at least one-third of white women with 8 or fewer years of schooling.[13] This means that the Hispanic ethnic factor is sharply affecting white comparisons involving the least-educated white women, but it is a less critical factor among mothers with higher levels of education. Accordingly, the least-educated class of whites is dropped in further comparisons.

We now examine whether each of the leading names—defined as those names that appear among the top 20 for at least one of the five remaining educational groups—vary in some systematic way with the educational level of the mothers. The detailed information for girls and boys are presented, respectively, in tables 3 and 4. There are a few girls' names

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[13] The number of Spanish origin women in this category is 76% of the total number of white women of comparable age and education. Since, for all Spanish origin females in New York, just under half are reported as white, a crude estimate of 37% indicates the magnitude of this problem. It drops sharply to 22.8% and 5.8%, respectively, for women with some high school or completing high school. Obviously, this does not take into account interaction effects or fertility differentials.

TABLE 3

Names Given to White Girls, by Education of Mother, New York State, 1973–85

Name	ND	% Receiving Name by No. of Years of School Completed by Mother				
		9–11	12	13–15	16	17+
Emily	.39	.27	.22	.76	1.17	1.43
Allison	.33	.11	.56	1.05	1.55	1.65
Lauren	.32	.13	.64	.99	1.49	2.07
Megan	.32	.12	.66	1.57	1.94	1.33
Catherine	.21	.56	.76	1.25	1.93	1.67
Elizabeth	.20	.79	.96	1.47	2.19	2.67
Sarah	.20	.82	1.52	2.23	2.91	3.32
Laura	.18	.47	.76	1.24	1.52	1.47
Erin	.17	.27	.76	1.22	1.38	.86
Rachel	.15	.55	.77	.98	1.55	1.49
Rebecca	.13	.67	.80	1.11	1.54	1.51
Amy	.01	1.13	1.29	1.46	1.45	1.57
Christine	−.02	2.29	3.48	3.44	3.28	2.73
Amanda	−.02	1.08	1.10	1.09	1.25	1.27
Stephanie	−.02	.99	1.25	1.27	1.28	1.07
Kelly	−.03	.67	1.33	1.42	.84	.72
Erica	−.04	.77	.76	.72	.81	.86
Heather	−.04	.97	1.40	1.49	1.23	.80
Danielle	−.04	.74	1.29	1.17	.77	.99
Kimberly	−.06	1.08	1.19	1.33	1.12	.68
Jessica	−.07	2.73	2.26	2.44	2.16	2.31
Jennifer	−.07	3.86	4.78	4.21	3.80	3.42
Jamie	−.07	.76	.89	.83	.74	.60
Stacy	−.08	.64	.92	.73	.61	.60
Lisa	−.09	1.10	1.58	1.13	.91	1.15
Nicole	−.10	1.56	2.17	2.06	1.17	1.07
Melissa	−.11	2.18	2.15	2.04	1.55	1.29
Michelle	−.11	1.67	2.28	2.06	1.06	1.09
Angela	−.27	.79	.69	.49	.19	.10
Maria	−.27	1.01	.65	.36	.36	.28
Tammy	−.45	.84	.37	.18	.06	.06
Crystal	−.47	1.36	.51	.32	.07	.00
Valid *N*	. . .	10,330	31,557	12,695	6,900	5,027

TABLE 4

NAMES GIVEN TO WHITE BOYS, BY EDUCATION OF MOTHER, NEW YORK STATE, 1973–85

		% RECEIVING NAME BY NO. OF YEARS OF SCHOOL COMPLETED BY MOTHER				
NAME	ND	9–11	12	13–15	16	17+
Andrew	.23	.53	1.05	1.69	2.13	2.73
Adam	.19	.52	.96	1.52	1.74	1.98
Matthew	.17	1.40	2.26	3.77	4.20	3.99
Jonathan	.16	.75	.90	1.45	1.95	1.70
Ryan	.14	.34	.95	1.54	1.29	1.07
Scott	.08	.70	1.06	1.28	1.58	1.24
Mark	.08	.81	1.38	1.59	1.46	2.06
Joshua	.07	.97	.98	1.32	1.28	1.91
Brian	.07	1.44	2.69	3.25	3.53	2.51
Daniel	.07	1.63	2.36	2.73	2.96	3.26
Jeffrey	.06	.88	1.28	1.67	1.40	1.76
David	.04	2.45	2.40	2.89	3.17	3.85
Eric	.04	1.32	1.58	1.87	2.06	1.89
Steven	−.02	1.66	2.15	2.03	2.20	2.00
Christopher	−.02	2.80	3.72	3.99	3.50	2.88
Michael	−.03	4.75	6.22	6.14	5.70	5.46
Sean	−.03	1.22	1.42	1.64	1.25	1.20
Thomas	−.04	1.34	1.88	1.63	1.56	1.57
John	−.05	2.34	2.93	2.56	2.97	2.17
James	−.07	2.13	2.23	2.18	1.97	1.72
William	−.09	1.65	1.70	1.46	1.36	1.46
Jason	−.12	3.02	2.85	2.39	2.07	2.11
Robert	−.12	2.71	2.58	2.27	1.83	1.67
Richard	−.13	1.42	1.45	1.16	.88	.94
Joseph	−.15	2.67	3.15	2.21	1.63	1.55
Anthony	−.25	1.95	1.78	1.04	.61	.51
Luis	−.54	1.11	.30	.13	.11	.04
Jose	−.58	1.71	.46	.16	.07	.04
Valid *N*	. . .	10,880	33,168	13,468	7,197	5,344

that display a perfect gradient with education, either rising or falling progressively throughout the span of educational levels. Among girls, for example, this is the case for Allison: a name given by virtually no women with a minimal education, but given to slightly more than 1.5% of the daughters of college graduates or postgraduates during the 13-year span under study. The same gradient occurs for both Lauren and Sarah. On the other hand, Carmen displays a perfect negative gradient with mother's education. Among boys, there are positive gradients with education for Adam, Andrew, and Daniel. Andrew, for example, increases progressively upward to 2.7% (mothers with postcollege schooling). Jose and Luis not only have negative gradients, but virtually no boys receive those names among those born to mothers with at least some college.

In order to provide a systematic comparison of each name's educational pattern, an index of net difference (ND) is determined for each name and reported in the first column of tables 3 and 4. This index compares the educational distribution of mothers of children with a given name with the educational distribution of other mothers whose children have a different name. For example, if every mother giving the name Emily to her daughter is compared with every mother giving her daughter a different name, there would be three subsets: in some cases Emily's mother would be more educated; in other cases the other mother would have a higher attainment level; and in some cases they would be of comparable education. The ND summarizes this gap by subtracting pairs in which there was some inequality. An ND of .39 for Emily means the following: if mothers giving their daughters the name Emily during the observed period were paired with all other mothers having daughters, the former would have higher levels of education 39 percentage points more often than the opposite inequality would occur (see Lieberson 1975).[14] The names are ordered in tables 3 and 4 from the highest positive ND value (linked most closely with higher levels of mothers' education) to the largest negative ND value (names relatively more concentrated among the children born to less educated mothers). This index will later provide the basic stratification measure for determining whether class differences in taste are linked to substantive features of the names themselves.

Black-White Comparisons

Given the influence of educational background on naming practices among whites, what happens when blacks and whites of similar educational attainment are compared? In general, black-white gaps remain as large or larger after they are matched by education. Because of the smaller number of cases and the lower educational distribution, we have divided the black population into three subsets based on mother's education: those with 9–11 years of school; high school graduates; and those with at least some college education.[15] Cramer's *V* between black-white

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[14] The index is scaled to take into account the distribution of remaining mothers and thereby make the maximum possible range ± 1.0 for each name.

[15] In order to maximize comparisons, data for whites were recomputed to also pool mothers with at least some college education. Because of the ethnic complications

TABLE 5

CRAMER'S *V*, THE INFLUENCE OF EDUCATION VERSUS RACE

	BLACK-WHITE COMPARISONS	
YEARS OF SCHOOLING	Boys	Girls
9–11	.35	.54
12	.25	.52
13+	.26	.48

	WITHIN-GROUP COMPARISONS			
	Blacks		Whites	
	Boys	Girls	Boys	Girls
9–11 versus 12	.15	.19	.15	.14
9–11 versus 13+	.27	.31	.27	.30
12 versus 13+	.14	.18	.15	.19

differences in the leading names for all boys is .23. For males, the largest gap is among those with less than a high school education ($V = .35$); for the other two educational groups, *V* is essentially no different from what is observed for comparisons that ignore education (table 5). Girls' names fit this pattern very nicely: the black-white gaps between mothers of comparable education are much larger than the *V* indexes for boys' names (repeating the stronger class influence on girls' names as opposed to boys' names, which was observed for whites); again, there is essentially no reduction in *V* when education is matched.

In the case of girls' names, naming preferences between black mothers with different educational levels are—with one slight exception—lower than any of the race differences between mothers of comparable education. In other words, the race effect is stronger than the class effect such that blacks with the lowest and highest of the three educational levels are more alike in their naming preferences than they are with whites of comparable education. This also holds for whites. The largest *V* between black subsets occurs when mothers with less than a high school education are compared with those with at least some college (.31). The same pattern occurs among whites (.30). It is noteworthy that this gap is still smaller than any of the black-white gaps when mothers of comparable education are compared (the lowest is .48). For tastes in girls' names,

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noted earlier within the 0–8 year category for whites, this category was dropped for blacks as well since a comparison with whites is not feasible.

the class effect is not as strong as the race effect. The pattern is less clear-cut for boys' names; the gaps between adjacent educational groups within a race are also smaller than any of the interracial gaps across mothers with a common level of education, but the lowest and highest educational levels differ within each race at roughly the same magnitude as is found in two of the three comparisons between blacks and whites with comparable education.

In short, although education is associated with naming tastes for both whites and blacks, the overall pattern of black-white differences in naming is not readily explained by educational factors. When schooling is taken into account, the racial gaps, by and large, remain substantial, and, strikingly in the case of girls' names, the class gaps within the black population are smaller than those between blacks and whites who are matched by formal attainment. Black-white differences in naming patterns cannot be explained by education.

THE NATURE OF EDUCATIONAL DIFFERENCES IN TASTE

The ND indexes presented above make it clear that educational subsets of the population differ in their tastes. But this raises a deeper and more difficult question than it answers: Why do the tastes take the specific form that they do? Are these merely idiosyncratic phenomena such that one name just *happens* to be favored by one educational group as opposed to another? Or is there reason to believe that the distribution of names between subpopulations is in part a reflection of some distinctive underlying set of dispositions? These issues are separate from—although related to—asking what the *motives* are for having different names. It is one matter, for example, to say that group *X* desires to set itself off from group *Y* in the names given to their children, but it is another task to determine why the tastes take their specific form. Why does one group prefer one name while another group tends to favor a different name? Starting with educational differences in specific names, we will seek to determine if they represent broad dispositions and propensities to favor certain types of names. In other words, is there a structure underlying class variations in name preferences?

We observed earlier that boys' and girls' names differed on three broad aggregate characteristics: concentration, uniqueness, and continuity over time. However, these factors provide very few clues to the dimensions underlying educational differences in name preference within each sex. Except for children born to women with less than a high school degree, for example, there is remarkably little difference between mothers in the concentration of names given to their children (table 6, col. 1). The daughters of high school graduates have the same concentration, for

TABLE 6

EDUCATIONAL DIFFERENCES IN AGGREGATE NAMING CHARACTERISTICS

Years of Education of Mother	% in Top 20	Index of Unique Names	Cramer's V, Leading Names, 1973–85
Mothers of girls:			
9–11	27.8	48.0	.56
12	33.4	36.7	.49
13–15	35.6	36.6	.43
16	36.3	35.2	.39
17+	35.6	34.9	
Mothers of boys:			
9–11	40.7	35.8	.33
12	48.0	25.7	.28
13–15	49.6	21.8	.29
16	49.6	23.2	.24
17+	48.8	17.8	

NOTE.—All values of Cramer's V are based on chi-square significance at .001.

example, as do the daughters of mothers with postcollege education. A similar pattern occurs for boys' names; the upper four educational groupings are virtually identical in the concentration of boys' names. There is likewise no difference between mothers in the disposition toward unique names for their daughters, again with the exception of the least educated (table 6, col. 2).[16] The boys' pattern is similar, except there appears to be more variation among educational groups, with the highly educated especially low in their use of unique names for their sons.

As for continuity in the leading names of each social class, college graduates have the greatest stability over time in the names given to either boys or girls (col. 3), and there is a tendency for V to increase with progressively lower levels of education. This means that turnover for both boys' and girls' names occurs more rapidly for the less educated.

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[16] The number of births varies by educational group (e.g., there are more mothers who stop at a college degree than there are those who go on to graduate education). Since the probability of a unique name being found in an education group is affected by the group's number of births (visualize the extreme case where one group has only one birth during the period), it is necessary to adjust for this factor. The influence of size is controlled by taking a random sample of a constant size (296) for both the sons and daughters born to each educational group. This eliminates the effect of size and describes the pattern as a function of each group's relative distribution. Although the number of unique names found this way cannot be taken as representative of the total population, its relative number is representative and provides the index values shown in col. 2 of table 6.

Observe, however, that there is a lower V for boys within each educational category. Nevertheless, the data show that the gender gaps for all three of these features operate more or less independently of social class—they are pervasive in the society.

CLASS TASTES: PHONEMIC, ETYMOLOGICAL, AND HISTORICAL UNDERPINNINGS

Girls

Are social class tastes affected by the sounds of names, their etymologies, and history? Because various characteristics of names are associated (French and Hebrew names, e.g., also differ in their phonemic structures), it is not easy to analyze these questions when the number of names is relatively small. Ragin (1987) proposes a Boolean "truth table" approach for precisely this problem, and we adopt here a modified form of it, avoiding the deterministic assumption that does not permit contradictory outcomes from the same set of conditions (the Boolean format is used here to provide a ready overview and summary of the relevant name characteristics being analyzed, indicating the relatively small N's and problems of collinearity). In the analysis that follows, the linguistic and social characteristics of names are summarized and then examined in terms of their ND indexes, that is, whether names with a given array of characteristics produce positive or negative educational gradients. In effect, the data in tables 3 and 4 are the raw data on which the analyses are based.

The six girls' names ending with an "*n*" type of sound (Allison, Catherine, Christine, Erin, Lauren, and Megan) illustrate both the procedure used to gauge the influence of educational background on tastes and a suggestive empirical result. Looking back at table 3, we see there is essentially no systematic difference between mothers in their disposition toward the use of Christine (ND $= -.02$), but in all of the five remaining names a favorable disposition toward the *n*-ending name varies directly with mothers' educational attainment (ND ranges from .17 to .33; the exception for Christine may reflect the fact that girls named Christina are also included [see n. 6 above]). The names differ on other characteristics as well: three are names with historical roots, but three are not listed in Stewart. Moreover, three of the names are of French origin. Perhaps these are factors affecting the upward gradient of these names with mother's education. The first two rows of table 7 summarize these features. A "1" means presence of the characteristic indicated in the column; a "0" indicates its absence. The first row lists those names ending in an *n* sound that also are relatively new names, are not of French

TABLE 7

BOOLEAN APPROACH TO CHARACTERISTICS OF GIRLS' NAMES AND ASSOCIATION WITH MOTHERS' EDUCATION

N	EE	A	L	C	S	B	F	ND+	ND−
1	0	0	0	0	1	0	0	3	0
1	0	0	0	0	0	0	1	2	1
0	1	0	0	0	1	0	0	0	5
0	1	0	0	0	0	0	1	1	1
0	1	0	0	0	0	0	0	1	0
0	0	1	0	0	0	1	0	2	0
0	0	1	0	0	0	0	0	1	7
0	0	0	1	0	1	0	1	0	2
0	0	0	1	0	1	0	0	0	1
0	0	0	1	0	0	1	0	1	0
0	0	0	1	0	0	0	1	0	1
0	0	0	0	1	0	1	0	1	0
0	0	0	0	1	0	0	0	0	2

N = Name ends in *n* sound.
EE = Name ends in *ee* sound.
A = Name ends in schwa-like sound.
L = Name ends in ell sound.
C = Name ends in other consonant sound.
S = Name not found in Stewart (1979).
B = Major biblical name.
F = French name.
ND+ = Number of names with positive ND index.
ND− = Number of names with negative ND index.

origin, and are not major names in the Bible. The last two columns of the table give the number of such names found and whether they are ND+ or ND−. In this case all three names are positive (meaning an upward gradient with education). The second row summarizes ND values for the three other *n* names which are in Stewart (1979) and are of French origin. Two of these are ND+ and one has a negative ND.[17]

Before turning to cross-tabulations and interpretation, consider the remaining zero-order patterns. There is also a clear educational gradient for the long *ee* ending; with less educated mothers tending to favor such names (six of the eight names). Likewise, most of the names ending in schwa are also favored more by mothers with lower educational levels (seven out of 10). Names ending in an "ell" sound or other consonant-like sounds also are more likely to have negative ND indexes, although the exceptions have distinctive patterns with respect to the other characteristics under consideration. A striking pattern is found for the 11 leading girls' names not listed in Stewart: eight are favored by less educated mothers, and the three exceptions are all for names that have an *n* ending. The four prominent biblical names are all ND+ (Rachel, Rebecca, and Sarah in the Old Testament, and Elizabeth, which appears in the New Testament).[18] French names have a mixed pattern—at least on this initial examination. The analysis that follows is consistent with the patterns summarized in the Boolean table, but obviously it is speculative on two grounds: the numbers are small and hence are subject to fluctuations. In any case, our interpretations are to be viewed as speculations or working hypotheses subject to further investigation.

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[17] The one exception is barely negative, but we are using conservative procedures here to estimate whether any taste patterns are associated with mother's education.

Class tastes for girls' names are affected by etymological and historical considerations; indeed, when these are taken into account the influence of education on some of the sound factors becomes clearer. Women with less education tend to favor names without long-standing historical roots (i.e., not included in Stewart [1979]) relatively more often than do highly educated mothers. It is illuminating to examine the specific patterns in table 7 for the 11 leading names not found in Stewart. Some of them have striking concentrations among the daughters of the least-educated mothers; Crystal (ND = −.47) is the seventh most popular name for the daughters of mothers with less than a high school education, thirty-ninth for high school graduates, and over fiftieth among women with at least some college. Likewise, Tammy (ND = −.45) is fifteenth in the least-educated group, but over fiftieth for all other mothers. For the 11 names not listed in Stewart (1979), the three exceptions with an upward educational gradient are names ending in an *n* sound: Megan, Lauren, and Erin. One can speculate that the higher educational groups in general resist names that appear too novel and lack historical roots. When there are shifts in the names preferred by such women, it is for previously neglected names that do have historical roots in the society. Such considerations are not as central among less educated women, for whom the fashionable nature of girls' names will have relatively more appeal. The deviation with respect to *n*-sounding names, which are generally more attractive to highly educated parents even if they are not historically rooted, is puzzling. We speculate that this taste differential exists because names ending in a consonant are less "frilly" or obviously feminine in their connotation (witness the differences observed earlier for all of the leading boys' and girls' names in the phonemic nature of their endings). Hence, highly educated mothers generally find names ending with an *n*-consonant sound more appealing for their daughters because they understate a glaring gender gap, yet clearly mark the child as female. For the very opposite reasons, these names hold less appeal for less educated mothers for whom "traditional" gender roles may have more appeal.

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[18] Based on the limited number of biblical persons regarded as "prominent" (see Reader's Digest Association [1967, pp. 609–12]. In the case of Elizabeth, the linkage with British royalty is also obvious and could be a more critical factor than the biblical one in affecting the differential appeal of the name. The enduring popularity of the movie star Elizabeth Taylor may also have contributed to the widespread use of this name.

Harmonious with this interpretation is the generally stronger disposition of the less educated toward "traditional" vowel endings for girls, that is, for names ending in an *ee* or a schwa sound. Overall, there is a clear and consistent pattern for the most common girls' suffix, those with a schwa ending. Inspection of the relevant panels in table 7 shows that only three of the 10 names are more favored by the highly educated. And, of these, two are major biblical names (Sarah and Rebecca). Laura is the only name with a positive educational gradient that is not accounted for. Once this is taken into account, the disposition of less educated mothers is striking. We are inclined to interpret this pattern as reflecting the propensity among the less educated to give their girls names that have a more distinctive feminine ending. However, one cannot draw such a strong conclusion about the *ee* ending because all five of the "new" *ee* names (i.e., not in Stewart [1979]) are favored by the less educated, whereas the pattern tips in the opposite direction for the remaining three *ee* names. So, if anything, it is class differences in the taste toward novel names that may be affecting the association with *ee* endings rather than some traditional linkage between such endings and girls' names. (Keep in mind that the gender difference in the use of *ee* endings is not as one-sided as the gender difference in names with schwa endings.)

Four of the five names ending with an ell sound have negative gradients (Crystal, Michelle, Nicole, and Danielle). The exception is easy to account for; it is Rachel, which is a major biblical name. Nevertheless, there are two possible interpretations of the four names with negative gradients. On the one hand, they may represent a class disposition toward the sound, or it may be a spurious relationship reflecting more fundamental factors—the remaining names are not in Stewart (1979) and/or are of French origin. Again, as mentioned above, it may be seen as more "feminine" to use such names and hence these names have a greater appeal for less educated parents. At this point, one cannot choose between these alternative interpretations. Not much can be said of the three names with other consonant sound endings: the one name with a positive gradient also is listed as a major biblical name.

Regarding the positive ND values for the biblical names, one could speculate that these biblical gradients reflect ethnic differences within the white population rather than purely taste differences by social class. If, let us say, there are a relatively important number of Jews in New York and they are concentrated among the highly educated women and if, in turn, they have a special disposition toward biblical names for their daughters, then perhaps this is the cause of the apparent gradient for the three Old Testament names. Although this is a plausible conjecture that cannot be ruled out entirely (New York data cross-tabulating the choices by ethnicity are not available), the evidence indicates that these gradients are not due to this factor. First, the names are also popular elsewhere during this period. Oregon is hardly known for its large Jewish population; yet in 1981 Sarah ranked first in the state, Rebecca was sixth, Rachel twelfth, and Elizabeth was below twentieth that year (although it was thirteenth by 1984). By contrast, in New York in 1981, Sarah was fourth among whites, Rebecca was seventeenth, and Rachel ranked thirteenth. In other words, all three Old Testament names scored better in Oregon than they did in New York State. By contrast, Elizabeth, which enjoys biblical significance from the New Testament rather than the Old, ranked more highly in New York (eighth) compared with Oregon (Oregon data reported in Oregon Center for Health Statistics [1986, p. 8]).[19] These results indicate that some of these biblical Old Testament names are at least as popular in states with small Jewish populations. This parsimonious conclusion—that the gradients in New York do represent class differences in dispositions toward biblical names that are not merely a function of ethnic variation by education—is particularly noteworthy since all three of these Old Testament names have phonemic endings that would, by themselves, either produce no upward gradient in usage by education or even a downward gradient.[20]

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[19] There are regional differences in naming patterns (Zelinsky 1970) and this affects the comparisons. In 1981 in New Mexico, Sarah was fifth most popular (vs. fourth in New York) and Rachel was eighteenth (vs. thirteenth in New York); on the other hand, Rebecca was stronger in New Mexico (fourteenth) and Elizabeth was weaker (twelfth) than in New York (New Mexico Health and Environment Dept. 1987, p. 2). Moreover, in the same year, Sarah ranked first in Vermont and Rebecca was seventh in that state (based on state data obtained from Ms. Joanna Smith).

[20] As a further test, the distribution of names in Wisconsin in 1979–81 was compared with New York data for the same years, with white mothers again classified by their level of education in each state. The test is whether the interstate correlations of leading names between comparably educated mothers is *relatively* weaker among the highly educated mothers. If so, this would be at least harmonious with the alternate hypothesis that the New York State results reflect an especially high concentration of Jews among such women and, in turn, their distinctive tastes. If Jewish women either are not that numerically important among highly educated New York women or do not have extraordinarily different tastes from other women of comparable educational levels, we would have no a priori reason to expect the New York–Wisconsin correlations for names given by highly educated mothers to be weaker than those for other mothers from the two states. Among girls' names, the interstate correlation is highest

TABLE 8

BOOLEAN APPROACH TO CHARACTERISTICS OF BOYS' NAMES AND ASSOCIATION WITH MOTHERS' EDUCATION

G	H	OT	NT	BT	S	ND+	ND−
1	0	0	1	0	0	1	1
1	0	0	0	0	0	0	3
0	1	0	1	0	0	1	2
0	1	1	0	0	0	5	0
0	1	0	0	1	0	0	1
0	0	0	1	0	0	1	1
0	0	0	0	0	1	1	3
0	0	0	0	0	0	4	4

G = Greek name.
H = Hebrew name.
OT = Old Testament name.
NT = New Testament name.
BT = Name found in both Testaments.
S = Name not found in Stewart (1979).
ND+ = No. of names with positive ND index.
ND− = No. of names with negative ND index.

Boys

Phonemic factors appear to have less influence on class dispositions toward boys' names. In the case of sound endings, for example, at best there are only minor differences between the classes—and they occur in no systematic way along class lines. Table 8 focuses on the etymological and historical influences. The sharpest of these are the five Hebrew names that are prominent in the Old Testament (based on the *Reader's Digest* criteria cited earlier): Adam, Jonathan, Joshua, Daniel, and David in varying degrees have upward gradients with mother's education. Prominence in the Old Testament appears to be the key factor; negative class gradients occur for three of the four Hebrew names that are prominent in the New Testament or in both Testaments (Thomas, John, Joseph). Matthew is a distinct exception.

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for mothers with 9–11 years of schooling (r = .82), but otherwise there are only minor differences between the remaining educational groups (r = .71 for high school graduates; .67 for those with 1–3 years of college; .63 for college graduates; .67 for postcollege women). Finally, we determined the outlier names in each interstate regression (standardized residuals of 2 or more), to see if these Old Testament names were unusually frequent in New York among the highly educated. This is probably the most critical test. The names under consideration were never exceptional outliers by this criterion. As a consequence, we can have a reasonable amount of confidence that the social class gradients for Old Testament names are not spurious.

Educational differences in tastes for boys' names are affected by several other features, albeit less sharply and/or less conclusively. Of the four leading names not in Stewart (1979), three are favored by the less educated; Ryan is an important exception. However, two are really conventional names that simply are not part of what had been the "standard" national repertoire of names. Rather they are Spanish names, Luis and Jose. Thus the association for boys may reflect an ethnic division associated with education. There are negative gradients with mother's education among four of the five leading names of Greek origin—the only exception, Andrew, may have been influenced by interest in Prince Andrew of England. At this point, no speculation is in order since three of the Greek names have very modest negative gradients. New Testament names have no clear linkage with social class since overall there are three with positive gradients and four with negative ones (five if Joseph is included). Finally, of the eight names with other historical origins, an equal number have gradients in each direction. At this point, we are unable to come up with any obvious way of subdividing these in a theoretically meaningful way.

As was the case for the SES gradient with Old Testament female names, the results for boys cannot be explained by variation in the Jewish component of the subsets of mothers. The Wisconsin–New York interstate correlations, for example, are actually highest among the most educated mothers (r is .65 and .66, respectively, for those graduating college and those with a postcollege education), compared with .47 among those with 1–3 years of high school, .29 for high school graduates, and .42 for some college. The analysis of outliers finds only Joshua appearing, and this is for those in the lowest educational class with Joshua *lower* than expected in New York when compared with Wisconsin.

Comment

Overall these results are intriguing. On the one hand, in the case of girls' names, we have uncovered various tastes underlying educational differences in name preferences, for example, class dispositions toward unconventional names or names ending in a hard *n* sound. In general, highly educated mothers are less likely to give their daughters names that are in the "traditional" or "conventional" female mold. It is as if they wish to have daughters who are taken more seriously and with less emphasis on names that are frilly. On the other hand, the patterns underlying class preferences for boys' names largely do not reflect these factors (about the only exception being the Old Testament factor). Granted that the class gaps are not as great for boys' names, does this mean that our underlying theory is partially correct, holding for differences in tastes only for girls' names? Another possibility is that class differences in their tastes in boys' names do reflect major thematic factors, but have simply not yet been measured with the above attributes. If the refusal of sexual status increases with education (an assertion made by Bourdieu [1984, p. 404]), then both boys' and girls' names should differ in some thematic fashion.

SEMANTIC DIFFERENCES AND CLASS

Since it is hard to believe that there are not some systematic differences underlying class references in boys' names, semantic connotations of names were examined to see if the class preferences in boys' (and girls') names vary along semantic dimensions. Starting with the pioneering research on semantics by Osgood, Suci, and Tannenbaum (1957), there is considerable evidence that first names vary widely in their connotations and images in the population (see the extensive literature listed in Lawson [1987*a*]). The issue is, Do the connotations of a name affect the likelihood of a given class using it for their children?

Lawson has reported data on the stereotypes associated with a large number of men's and women's names (1985*a*, 1985*b*, 1987*b*). These were obtained from a survey of college students (250 women and 250 men) at the State University College in Fredonia, New York. Each name is rated on six dimensions: good/bad, strong/weak, active/passive, sincere/insincere, intelligent/dumb, and calm/emotional. Many of the leading names used in this study are among those included in Lawson's work (25 of the leading boys' names and 13 of the leading girls' names).[21] Since the educational data are for mothers, our semantic analyses are restricted to the responses made by female subjects; however, male subjects usually give fairly similar responses to the names.

General Differences

The first part of table 9 allows us to compare the semantic connotations of leading names with the larger population of first names. The more popular boys' and girls' names for which we have semantic information are, in general, different from the entire set of names surveyed by Lawson. The average leading boys' name scores higher on five of the dimensions: good, strong, active, sincere, and intelligent. There is virtually no

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[21] In some cases, spelling variants of the same girls' name were rated separately in the Lawson study. All 18 are included in the averages, but only the most common form is used in the correlations reported below. Because of differences in N, tests of significance are not the same for the boys' and girls' results.

TABLE 9

SEMANTIC CONNOTATIONS AND POPULARITY OF NAME, BY EDUCATION OF MOTHER

	Semantic Connotations					
	Good	Strong	Active	Sincere	Intelligent	Calm
Mean scores:						
Leading boys' names	88	82	75	88	84	60
Other boys' names	77	65	57	75	70	59
Leading girls' names	84	50	44	81	71	58
Other girls' names	73	54	49	72	66	51
Spearman correlations:[a]						
Boys' names	.12	−.38*	−.19	.18	−.20	.09
Girls' names	.76***	.49*	.56*	.69**	.45⁺	−.07

[a] Between connotations of names and their association with education of mother.
* $P < .05$.
** $P < .01$.
*** $P < .001$.
⁺ $.05 < P < .10$.

difference between the more popular names and other names with respect to calmness. Overall, then, the most popular names for boys are not a random sample of the repertoire of boys' names, but are generally those that the population would judge as having more favorable connotations on most of these dimensions. The pattern is more complicated for girls' names. As above, popular names for girls score higher on the goodness scale, on intelligence and sincerity, but, if anything, they score lower on both strength and activity than do the less popular girls' names. In other words, there is a strong gender factor that affects whether the images associated with a given name will appear attractive or not.

Popular boys' names, compared with the leading girls' names, have very different semantic connotations; in particular, the former have images that are much stronger, more active, and more intelligent than those for popular girls' names. Although these gaps exist for the entire set of boys' and girls' names, the differences are far greater among leading names. For example, the strength gap is 32 points between the leading boys' and girls' names, whereas it is only 11 among those not exceptionally popular. The net effect of this selectivity in naming patterns within each sex is to increase the gender gap between leading names on these dimensions from that which would occur if a representative cross-section of boys' and girls' names were selected. Thus these gaps are not simply a function of general differences in the connotations of boys' and girls' names, such that the former are stronger, more active, and more intelligent. One possibility is that the connotation of names affects the likelihood of parents selecting them for their child, but that the selectivity is different for boys and girls. The reverse causal hypothesis is also plausible; namely, that popular names generate sharper gender stereotypes. Only longitudinal data will resolve this.

Given this general pattern, do the social classes differ in the semantic characteristics of their childrens' names? For each of the six semantic scales, part 2 of table 9 gives the Spearman rank-order correlation between the semantic connotation of each leading name and the frequency of its usage by mothers of different educational levels (as measured by the ND index for each name). Important class differences are found for several dimensions. Boys' names that connote strength are particularly common in families with less educated mothers ($\rho = -.38$). This means that boys' names with exceptional connotations of strength have relatively less occurrence when mothers are highly educated. The pattern runs in the opposite direction for girls' names, with connotations of strength having greater frequency among highly educated women ($\rho = .49$).

There are a number of linkages between mother's education and semantic connotations of girls' names. Positive correlations exist between the education of mothers and the propensity to pick daughters' names that have connotations of goodness ($\rho = .76$), strength ($\rho = .49$), activity ($\rho = .56$), sincerity ($\rho = .69$), and intelligence ($\rho = .45$). Keep in mind, this means that such semantic dimensions are relatively less common among names chosen by less educated women. In any case, these factors do not appear to have any bearing on social class differences in the selection of boys' names other than in the case of strength—which is more common among the sons of less educated mothers.

Comment

Names vary widely in their images or connotations. We are not surprised to find that popular boys' names have connotations that differ from those found among leading names for girls. Most important for the issue at hand, we find that the social classes diverge in the images associated with the names given to children. On four of the six semantic dimensions, the educational level of mothers is linked to the relative appeal of the semantic connotations of girls' names. For boys' names, by contrast, the image of strength increases the chance of the name being reported for the sons of less educated mothers.

It is not entirely clear-cut how to interpret the association between the educational level of mothers and the semantic connotations of the names favored in the households. There are two plausible speculations, each with radically different implications. The first involves the following causal chain: (1) names differ in their semantic connotations, (2) classes differ in the importance attached to various characteristics of boys and girls, (3) these in turn affect the names given to children, (4) an association therefore results between class and the connotations of names. The second chain is as follows: (1) classes differ in their images, (2) names are adopted differentially by each class due to various nonsemantic factors, (3) names begin to pick up the connotations of the classes that specially favor them, (4) an association therefore results between class and the connotations of names. Of course the two are not mutually exclusive, each may be operating on different names or even together; for example, a name's semantic connotations would attract classes differentially to use the name and, in turn, a feedback develops such that the society at large then associates the name with a class; therefore, the name picks up stereotypes about the class. The next step requires longitudinal information on naming for a longer span than is available here and, in addition, requires data on the images of names *before* and *after* they become linked with particular classes.

We should also note that the stereotypes and images reported here are limited to those studied by Lawson. So we cannot get at questions of whether there are other semantic dimensions (say, those dealing with "traditional feminine" forms such as physical attractiveness) that are correlated with class differences in a name's attractiveness. Similarly, there may well be other semantic features correlated with the frequency of names given to sons. Also, further research would have to deal with the question of whether data on semantic differentials derived from a survey of college students by Lawson is representative of perceptions by parents generally.

DIFFUSION OF NAMES ACROSS EDUCATIONAL GROUPS

A classic perspective on fashion is that lower strata copy the choices made by more prestigious classes either because they want to appear similar to the latter or because the latter's usage provides confidence in the choice (see, e.g., Simmel 1957; Barber and Lobel 1953). In either case, the argument runs, it is the class-linked quality of the item that drives the taste among other segments. At some point, the distinctiveness of the fashion for the initial wearers is lost, and the process recycles as new fashions are introduced to set off one class from another, which are again copied, ad nauseam. Other features besides class can operate; for example, the current imitation of American pop tastes in Europe and Asia occurs in part simply because the tastes *are* American.

It is an empirical question as to how accurate a representation this is of the fashion world, but such a model, if it applied to the diffusion of first names, would suggest a process that runs counter to notions of intrinsic appeals of the names to one class as opposed to another. The nature of the fashions is secondary, from this perspective, to the symbolic class association they hold. This perspective has added importance because it could provide an alternative perspective to class differences in tastes, suggesting there are not intrinsic differences so much as lags in what is popular for each class. The relevance for the semantic issues raised above is obvious.

Although the span of time covered by the New York State data is too brief to permit an intensive analysis, particularly in which the maintenance of class differences in tastes is considered (the total number of name shifts should be much greater), we can at least consider whether there is evidence of a class diffusion process, in particular whether less educated mothers adopted the names first used by more highly educated mothers.[22] To our knowledge, this diffusion issue is rarely studied with actual temporal data for virtually any kind of tastes. The results are clear-cut in one sense, but they also raise an alternative issue not hitherto considered.

We determined every name that was ranked in the top 20 for at least one educational group in 1985 (the terminal year) but was not similarly ranked in any class in 1973 (the initial year). This is our set of newly popular names. For each name, we determined in what year—if ever—the name first made that group's top 20. By this procedure, it is possible to determine an adoption order for any name that made the top-20 in more than one educational category. Justin, for example, first makes the top 20 in 1977 in two categories: mothers with some college education and mothers with postcollege education. Two years later, the name ranks in the top 20 for women with a college degree and for women with 9–11 years of schooling. A year later, Justin makes the top 20 for high school graduates. We can say that the popularity of the name for women with either some college or postcollege precedes that of the three other educational groups. For the two groups whose preference first appears in 1979, we can say that the name precedes one other group and follows two earlier ones. We can also determine that the name's popularity with women in the high school category follows four other groups and of course does not precede any group.

The pattern found through this scoring procedure is remarkably clear (see table 10), all the more so when one considers the small *N*'s sometimes necessary to make the top 20. Girls' names often first appear among the top 20 of the highly educated and then a few years later among daughters born to less educated mothers. The opposite diffusion pattern, from less

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[22] Only 12 girls' and 8 boys' names meet the criteria indicated, and in three cases no diffusion was observed at all.

TABLE 10

ADOPTION SEQUENCES FOR NEWLY POPULAR NAMES, BY EDUCATION OF MOTHER

	No. of Cases			
	Girls' Names		Boys' Names	
Years of Education of Mother	Earlier	Later	Earlier	Later
9–11	6	11	1	7
12	3	15	2	10
13–15	10	9	8	5
16	13	4	10	2
17+	12	5	9	6

to more educated mothers, is far less common. The boys' pattern is similar, albeit slightly less orderly. Again, among less educated parents, new names are far more likely to appear in the top 20 after usage by other groups. The opposite sequence is less common for boys' names. The main difference from the girls' pattern is really for boys born to women with a postcollege education. Although in naming their sons they are more likely to precede rather than follow other groups, it is by a far narrower margin than that observed for girl's names.

Overall, although the number of cases is small, a remarkably orderly pattern emerges that appears to support a class-based diffusion model for names.[23] However, it is not entirely clear that this is the case, or at least as pervasive as quick analysis of the data in table 10 might appear to support. In the case of Justin, recall that the adoption process spans only four years. The name first makes the top 20 in 1977; by 1980 it has made the top 20 for all five groups. Now by the standards of, say, clothing this would be reasonable to understand through copying from a single initial model. But for clothing there is a set of mechanisms for such a diffusion: print media devoted to fashions, stores that promote new fashions, television and movies to carry forward fashion messages. However, it is not entirely clear how rapidly one class would learn of the names adopted by another class. In some cases, the explosion is so rapid that we speculate that it is really differential rates of adoption from a single source rather than a chain of diffusion per se based on an appeal driven by the impression that higher-strata persons are using this name for their children. Ashley, for example, first becomes a top-20 name in 1983 for two classes (some college and postcollege), then for two more a year later

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[23] A detailed analysis of the relative frequency of preceding vs. following for each specific combination is not justified by the small numbers.

(high school graduates, college graduates) and makes the top 20 in 1985 for the 9–11 group. There is imitation or copying, to be sure, but it may come from a single stimulus that subsets of the population are differentially exposed to, say, the use of a name inspired by an upscale woman's clothing chain that is more popular with one subset than another.

In brief, while the results are not totally clear, they do suggest the following: the lags in newly popular girls' names are more likely to follow a "fashion model" such that households with less educated mothers tend to favor names for their daughters and sons *after* those names have been used in households with more highly educated mothers. However, far more than a simple imitation model may be operating here. At this point, the best that we can do is recognize the existence of empirical evidence that actually shows a class-linked ordering in the adoption of newly popular names. A deeper interpretation and analysis will require data for a far longer span than is available here.

BROAD SOCIETAL INFLUENCES: THE CASE OF BLACK GIRLS' NAMES

Possibly the most difficult issue raised in the study of names pertains to the impact of broad societal shifts on naming tastes. From this perspective, the names given to black women represent two of the most striking changes in the United States: shifts in the position of blacks in the society, beginning with the end of slavery, and the movement of women toward equality.[24] A 13-year span in New York State is inadequate for considering the impact of such deep societal changes. But we start with an observation based on black-white contrasts in the names given to black daughters. Presently blacks are far more disposed to using newer names than are whites (by newer, we mean names not listed in Stewart [1979]). Comparing the two groups by education, there is only a modest racial difference in households where the mother has less than a high school education. Among blacks, half of the leading names are not in Stewart, as are eight of the 20 names most often given white girls (table 11). However, this disposition also occurs among blacks in the higher educational categories (although the specific choices change somewhat), whereas it drops off sharply with education among whites. As a working hypothesis, one could argue that these gaps reflect the assertion made in recent decades that many blacks are embracing the distinctive and desirable qualities of black culture and rejecting white cultural styles as a model for blacks. From this perspective, the disposition of blacks toward "unique" names

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[24] Obviously there are many reasons for names to change that would entail far less basic forces.

TABLE 11

NUMBER OF TOP-20 GIRLS' NAMES LISTED IN STEWART, BY RACE AND EDUCATION OF MOTHER

EDUCATION OF MOTHER	NO. OF NAMES*			
	Black		White	
	Yes	No	Yes	No
9–11	11	11	12	8
12	8	12	14	6
13+	9	11	15	5

* Three names tied for twentieth rank among black mothers with 9–11 years of education.

would also fit in as a further indication that the standard white cultural patterns are often rejected. There are, of course, other interpretations of such a pattern, as well. It is not possible to evaluate them all here, but we shall attempt to provide a further empirical test of this proposition and to weigh it against one alternative interpretation.

One alternative possibility is that black-white differences in the choice of names and/or the disposition toward certain phonemes and/or the attitude toward idiosyncratic names reflect a heritage either brought from Africa or resulting from earlier black slavery experiences in the New World (see, e.g., Dillard 1976, pp. 14–19; Paustian 1978; Stuckey 1987, p. 196). There is little question that important differences existed in the naming patterns for blacks and whites in the antebellum South (Lieberson 1984, pp. 83–84). However, were such differences found either in the postbellum South or earlier in this century? This is a critical question since interpretation of the presentday differences will be very much affected by timing of their occurrence. If relatively recent, then they could be a function of the black social and political thrust in the sixties; if earlier, then they might be harmonious with the claim that black naming patterns are affected by historical African links either directly in terms of the names used or indirectly through the cultural dispositions toward certain names.

Newbell N. Puckett (1975) provides us with a rare opportunity to address this question in a somewhat systematic way, thanks to his compilation of black and white names reported in the city directories of Augusta, Georgia, in 1877 and 1937.[25] For black females, Cramer's *V* between 1877 and 1937 in their leading names is .44, compared with an extraordinarily high *V* of .94 between 1937 and New York State at present. In similar fashion, *V* jumps from .25 for males between the earlier two periods to .66 for the more recent comparison.[26] Obviously this widening gap over time could reflect technical factors: since the data obtained for the current period are age-cohort specific, it is possible that the radically higher values of *V* reflect this influence in part or whole. Also, the second time-span comparison confounds the influence of regional effects. Nevertheless this widening shift in leading black names is harmonious with our working hypothesis.

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[25] At that time, the city directories labeled people by race. These data, however, are not cohort specific since year of birth is not given. As a consequence, these are prevalence rather than incidence data, with certain shortcomings that could accompany such a procedure (see, e.g., Lieberson 1966). However, comparisons in 1937 overwhelmingly involve blacks who were born after the demise of slavery.

To further evaluate this interpretation, we examine the specific leading names found among black and white females in Augusta, Georgia, in 1937. Would there be the same pattern of nonstandard names among blacks in that period? If so, obviously current black nationalism could not be the factor. In 1937, the most common names given black females in Augusta were (in declining frequency): Mary, Annie, Mattie, Carrie, Rosa, Lillie, Emma, Mamie, Hattie, Sarah, Fanny, Louise, Elizabeth, Ella, Julia, Lula, Lizzie, Marie, Susie, Alice, Janie. This list is radically different from the top black names in table 1, and the current patterns are not historically rooted (at least as judged by this specific comparison).

Also observe that 17 of these 20 most common names for black females in Augusta in 1937 end in a vowel sound (the only exceptions are Louise, Elizabeth, and Alice). White names from the same period start with the same top two, Mary and Annie, but then the next five end in consonant sounds (Margaret, Elizabeth, Louise, C[K]atharine, Ruth). Other nonvowel ending sounds appearing later in the top 20 names for whites are: Frances, Helen, Alice, Lucille, and Ethel. In terms of the issue at hand, there is a racial difference in the disposition to use consonant versus vowel endings for girls operating at that point. This differential is no longer observed in the top 20 names for black females in the current period; there are 12 black and 11 white girls' names ending with a vowel sound.

A variety of interpretations are possible for this shift, not the least being that blacks are simply more likely than whites to report nicknames—hence these differences are not true reflections of what appears on their birth certificates. But another possibility stems from the fact that names with vowel endings, particularly nicknames ending in *ee* are often less assertive and carry a childlike connotation. The language we use with children often adds an unnecessary *ee* ending, for example, doggie, piggy, kitty cat, bunny rabbit, mommy and daddy (instead of mom and dad), teeny weeny, Mikey (instead of Mike), and so forth. There is also some evidence that nicknames are more common in younger ages and that they indicate intimacy and hence lower social distance (see Skipper 1984, p. 29). In a place and period where there must have been a strong emphasis by whites on keeping blacks in their "place" and the punishment of "uppityness," it is not totally farfetched to speculate that the distinctive black naming pattern would reflect the impact of this emphasis on their subordination.

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[26] All computations are based on the top-20 comparisons described earlier.

The available evidence also suggests that the current usage of unique names is a relatively modern phenomenon. Although accounts of unusual black names occur much earlier, it is important to keep in mind three key facts: (1) there are also accounts of unusual names for whites; (2) not all unusual names are made-up names, rather some are uncommon older names such as obscure names from the bible; and (3) there is a distinction between the existence of a small number of well-publicized names that some whites turned into "amusing" anecdotes as contrasted with the large percentage observed at present. For the most part, H. L. Mencken's (1963, pp. 628–29) review found such unique names to be relatively infrequent. As Puckett (1975, p. 135) concludes, after observing relatively modest percentages of blacks having "unusual" names in an earlier era—and even fewer of these after deducting for "old-fashioned" names—"The whole question of unusual names characteristic of Black Americans is pervaded by stereotyped prejudice and myth." Thus we are reasonably confident that neither the presentday frequency of unique names among blacks nor the sharp white-black difference in leading names simply reflect long-standing historical patterns in the United States.

Thus the available evidence at this point is harmonious with an interpretation of contemporary black naming patterns that reflects broad shifts in their position in society. Definitive work remains to be done, but these naming changes are interpretable in terms of the major shifts in the position of blacks and the affirmation of a distinctive black subculture in the society.[27]

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[27] However, we cannot rule out the possibility that these shifts of black naming tastes had started to occur *before* the eruption of large-scale black nationalism. There is some evidence that the movement toward unique, made-up names is older than the black nationalist movement, albeit not anywhere close to the magnitude found at present. Puckett (1938, p. 48) reports a rise in unusual names among black college students in 1910–30 over the 1880–1910 period, and likewise in successive birth cohorts listed in *Who's Who in Negro America* (for blacks born before 1870, between 1870 and 1900, and after 1900). This question will probably not be resolved until more data on black-white differences are analyzed to see if the period between the late thirties and the sixties was marked by changes that preceded the dramatic events observed more recently.

CONCLUSION

This exploratory study of the intrinsic nature of tastes starts with scrutinizing the naming patterns for white boys and girls. We find a number of intrinsic gender differences in the nature of their names. To take one striking example, parents are relatively more likely to give daughters names that are not standard ones in the existing American repertoire. This takes a variety of forms: choosing a newly encountered name that has no history of use in the nation; making up a name; or selecting names that were initially only a diminutive or pet form of a standard name. By contrast, boys' names are more often chosen from the existing stock of long-standing names in the society, even if they were not popular in recent years. Also the diminutive forms of established boys' names are less frequently adopted as a formal first name. In addition, shifts in the popularity of boys' names are slower than for girls'. These gender differences are even manifested in the etymological origins of the names used—for example, names of French origin are far more often used for girls than for boys. All in all, the results suggest a pattern whereby girls' names tend to be viewed as objects of fashion and expressions of "attractiveness" whereas boys' names are approached in terms of historical continuity and stability—they are taken more "seriously," as it were.

In turn, we ask if social class affects the names given to children, an important question about tastes since the usual class influences of income and knowledge (with some esoteric exceptions) would not be operating here. Since organizations are not particularly relevant for names or, in the case of religion, set relatively broad parameters to choose from, this provides a rare opportunity to examine cultural and structural influences on tastes uncontaminated by these other factors. Substantial differences exist between the classes in their naming patterns for both boys and girls, with a number of gradients observed by education of mother. In other words, the class differences show an orderly pattern overall, with the dissimilarities widening as women with progressively more dissimilar educations are compared.

This raises the difficult issue of determining if there are any dispositions or factors underlying these class differences in taste. Are there any common features shared by the names that are positively or negatively correlated with mother's education? If so, what are the bases of these class differences in taste and why do they exist? A number of such factors are found. For example, there are class differences in the propensity to favor girls' names ending in an *n* sound or in girls' names that are of biblical origin, or in boys' names that have the imagery of strength. These patterns in general suggest class differences in concerns about the roles of females and males and the images that are held for the sexes—for example, “strong” names are preferred for sons born to women with less education. This conclusion is supported by the variation observed between classes in the semantic connotations that they find attractive or unattractive in naming their children. (As observed earlier, the causal chain could be in the opposite direction; we cannot choose between these two different accounts of this interesting result.)

Black naming patterns for girls, both at present and as viewed earlier in this century in Augusta, are at least consistent with our disposition to view taste differences between subsets of the population as in part driven by broad social conditions. First, we find that substantial black-white differences are not explained by mothers’ educational differences. Moreover, the leading names given to black girls appear to be unrelated to earlier tastes found in the South. Speculations are presented to account for these differences in terms of broad social changes in the position of blacks and attitudes toward the maintenance and assertion of a distinctive black culture.

As one might expect in any investigation of a relatively neglected topic, there are more questions at the end of this article than at the beginning. For example, we have not been able to consider the influence that images of physical attractiveness have on choice of names (or, for that matter, traditional “feminine” or “masculine” roles beyond the ones touched on in the semantic differential study). Likewise, we have no information on whether the social classes differ in their images for the same names, in addition to varying in the features which they find appealing. Also, the results are least helpful for understanding the factors affecting name choices for boys born to highly educated mothers. And, of course, there is the complicated problem of understanding why particular names have the images that they do.

Nevertheless, the data systematically indicate how underlying cultural and structural conditions drive taste choices that on the surface appear to be idiosyncratic and haphazard. Even in situations where organizations and institutions seek to alter and manipulate tastes and fashions, as through advertising, it is likely that there are a set of underlying societal and cultural dispositions that set limitations and boundaries around such efforts. Indeed, the success of institutional efforts to affect tastes may reflect the ability to use the underpinnings of existing tastes as opposed to creating new preferences without basing them on existing societal and cultural dispositions.

APPENDIX

TABLE A1

THE 50 MOST POPULAR NAMES FOR WHITE GIRLS, NEW YORK STATE, 1973–85

Name	Frequency*	Valid %
Jennifer	2,987	4.2
Christin(e)**	2,219	3.1
Jessica+	1,657	2.3
Melissa+	1,396	2.0
Michelle	1,348	1.9
Nicole+	1,282	1.8
Sarah+	1,245	1.7
Lisa	921	1.3
Elizabet(h)	913	1.3
Amy+	911	1.3
Heather	891	1.2
Stephani(e)	827	1.2
Kimberly	791	1.1
Amanda	778	1.1
Kelly+	778	1.1
Danielle	759	1.1
Catherin(e)	712	1.0
Rebecca+	664	.9
Laura+	651	.9
Rachel+	630	.9
Megan+	629	.9
Erin+	569	.8
Jamie+	560	.8
Lauren	554	.8
Erica+	549	.8
Stacy+	536	.8
Allison+	523	.7
Karen+	507	.7
Maria	501	.7
Mary	460	.6
Tara	421	.6
Andrea	411	.6
Angela+	403	.6
Kristie+	403	.6
Shannon	397	.6
Tracy+	394	.6
Crystal+	370	.5
Emily	359	.5
Julie	356	.5
Jacqueli(ne)	355	.5
Kathleen	354	.5
Lindsay+	346	.5
Carrie+	326	.5

TABLE A1 (*Continued*)

Name	Frequency*	Valid %
Theresa+	325	.5
Dawn	315	.4
Patricia	315	.4
Deborah+	306	.4
Susan+	306	.4
Diana+	292	.4
Vanessa+	292	.4

NOTE.—Calculations for "Valid %" exclude any child who has not been given a name on her birth certificate.

* These are actual sample numbers.

** Since the survey only allows for the reporting of 8 letters, some names are incomplete. In most cases, the concluding letters are obvious and are supplied here in parentheses. In the case of Christin(e), the spelling could be "Christin(a)" or even "Christin" (although this is more likely to show up as "Kristin").

+ This name may include a variety of spellings, e.g., "Sara" and "Sarah," "Amy" and "Ami," etc.

TABLE A2

THE 50 MOST POPULAR NAMES FOR WHITE BOYS, NEW YORK STATE, 1973–85

Name	Frequency*	Valid %
Michael	4,289	5.7
Christop(her)**	2,565	3.4
John+	1,994	2.7
David	1,988	2.6
Matthew+	1,963	2.6
Joseph+	1,936	2.6
Brian+	1,934	2.6
Jason+	1,933	2.6
Daniel	1,787	2.4
Robert	1,764	2.3
James	1,572	2.1
Steven+	1,499	2.0
Thomas+	1,238	1.6
Eric+	1,212	1.6
William	1,194	1.6
Anthony	1,088	1.4
Sean+	1,018	1.4
Mark+	1,016	1.4
Jeffrey+	978	1.3
Richard	966	1.3
Andrew	958	1.3

TABLE A2 (*Continued*)

Name	Frequency*	Valid %
Kevin	851	1.1
Adam	835	1.1
Jonathan	832	1.1
Joshua	821	1.1
Scott⁺	800	1.1
Nicholas	779	1.0
Paul	750	1.0
Timothy	737	1.0
Ryan	719	1.0
Justin	683	.9
Peter	618	.8
Gregory	547	.7
Patrick	530	.7
Jose	504	.7
Jeremy⁺	489	.7
Charles	471	.6
Edward	449	.6
Benjamin	396	.5
Kenneth	393	.5
Frank	386	.5
Aaron	333	.4
Keith	331	.4
Luis	323	.4
Philip⁺	323	.4
George	315	.4
Craig	291	.4
Vincent	281	.4
Alexande(r)	279	.4
Dennis⁺	278	.4

NOTE.—Calculations for "Valid %" exclude any child who has not been given a name on his birth certificate.

* These are actual sample numbers.

** Since the survey only allows for the reporting of 8 letters, some names are incomplete. In most cases, the concluding letters are obvious and are supplied here in parentheses. Certain variations are possible, e.g., "Christoph" instead of "Christopher."

⁺ This name may include a variety of spellings, e.g., "Eric" and "Erik," "Jeffrey" and "Geoffrey," etc.

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Mesomobilization: Organizing and Framing in Two Protest Campaigns in West Germany[1]

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The concept of mesomobilization is introduced as a specification to the prevailing literature on mobilization processes. Mesomobilization actors have a dual function: They first provide the structural basis for mobilization by coordinating micromobilization groups and collecting the resources required for action and then try to achieve a cultural integration of the various groups by developing a master frame to interpret the triggering event in a way that is conducive to mobilization. Two empirical cases: the mobilization against U.S. President Ronald Reagan's visit in Berlin in 1987 and the mobilization against the yearly meeting of the International Monetary Fund and the World Bank in Berlin in 1988 are investigated to develop hypotheses that indicate what structural and cultural factors are important to a successful mobilization.

I. CONCEPTUAL FRAMEWORK AND RESEARCH QUESTION

Many theories and concepts that are sometimes contradictory have been offered to explain successful mobilization for collective protest. Recent work in this field has made some progress in integrating several theoretical concepts into a common framework (Klandermans and Tarrow 1988; McAdam, McCarthy, and Zald 1988). For example, Bert Klandermans and Dirk Oegema (1987) distinguish between different steps of mobilization and relate specific explanatory variables to each of these steps. A successful mobilization begins with a mobilization potential which, in

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[1] A first version of this article was presented at the Twelfth World Congress of Sociology in Madrid, July 9–13, 1990. We thank Andreas Braun and Bernd Maywald for collecting data on both campaigns, Doug McAdam and the colleagues of our research unit for critical comments on earlier versions of this paper, and Jeffrey Butler and Lee Ann Banaszak for copyediting. Jeffrey Butler also translated the leaflets documented in the Appendix. Finally, we wish to thank several reviewers for their sensitive and thorough comments on the manuscript. Correspondence should be addressed to Jürgen Gerhards, Wissenschaftszentrum Berlin für Sozialforschung gGmbH, Reichpietschufer 50, D-1000 Berlin 30, Germany.

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turn, depends on macrostructural factors such as demographic and ideological variables. These factors predispose individuals and social groups toward the means and goals of mobilization. The transformation of these objectively given dispositions into subjectively perceived definitions of problems presupposes collective actors with resources and access to networks who can reach the "available" mobilization potential and offer meaningful interpretations of the issues at stake. This is a precondition for forming a consensus in a specific campaign (Klandermans 1988; Snow et al. 1986).

A mobilization potential's existence, however, is a necessary, but not a sufficient condition for actual protest; the potential also has to be activated. Activation requires that collective actors with resources gain access to the mobilization potential, convince people to participate in collective protest, and thus achieve action mobilization (Klandermans 1988).[2]

In recent years, various authors have emphasized the relevance of mediating structures for forming consensus and for mobilizing people to act. In their overview of more recent movement research, however, McAdam et al. (1988) conclude that precisely this analysis of intermediary structures represents a decisive deficit in movement research: "what is needed is more systematic, qualitative field work into the dynamics of collective action at the intermediate or mesolevel. We remain convinced that it is *the* level at which most movement action occurs and of which we know the least" (McAdam et al. 1988, p. 729). Nevertheless, a few scholars have dealt explicitly or implicitly with mobilizing structures on the mesolevel by promoting different categories and concepts.

First, there is the notion of *multiorganizational fields*. These are defined as the "total possible number of organizations with which the focal organization might establish specific linkages." (Curtis and Zurcher 1973, p. 53).[3] The authors identify two levels of interorganizational processes that conceptually overlap: "the *organizational* level, where networks are established by joint activities, staff, board of directors, target clientele, resources, etc.; the *individual* level, where networks are established by multiple affiliations of members."[4] Bert Klandermans (1990) has also used the idea of multiorganizational fields in his recent work. However, he deliberately extends the meaning of the term to include opponent organizations under the same label and then distinguishes an alliance system and a conflict system with regard to a social movement organization. With this broader meaning of the term, Klandermans comes close to what other scholars have called the *social movement sector,* which refers to all social movement organizations in a society, no matter to which movements they are attached (Zald and McCarthy 1980; Garner and Zald 1985). The concept of multiorganizational field is important insofar as it emphasizes the embedding of a movement in its wider organizational environment. However, it is not very specific about various targets, steps, and tasks of mobilization.

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[2] The steps necessary for successful mobilization for protest participation could be labeled as follows: macrosocietal conditions → mediating structures and frames → mobilization potential → mediating structures and frames → protest activities.

[3] Roberto M. Fernandez and Doug McAdam (1989) adopted this concept in their analysis of the network that underlay the Freedom Summer mobilization.

[4] Roger V. Gould (1991) made a similar distinction between organizational networks and informal networks. In his analysis of the mobilization process in the Paris Commune in 1871, Gould provides an interesting analysis of the interrelation between both kinds of networks. He empirically demonstrates that the interaction of both levels not only has an additive effect but forges and maintains solidarity among insurgents.

Second, the concept of *political opportunity structure* (Tarrow 1983) also includes the aspect of alliances as one element that facilitates social movement mobilization. In this context, however, allies are considered as agents outside the movement rather than as a part of it. Moreover, there is no distinction between various levels of mobilization, nor any specification of different functions of the alliance structure.

Third, there is a concept that explicitly claims to bridge the gap discussed above at the intermediate level of mobilization. McAdam et al. (1988, p. 709) make a conceptual suggestion for closing the gap by introducing the concept of *micromobilization contexts:* "The key concept linking macro and micro processes is that of the micro-mobilization context." Micromobilization contexts can be preexisting political groups (e.g., unions), nonpolitical groups (e.g., churches), or informal groups (e.g., friendship networks). Despite the differences in the size and degree of formal organization of these various collective settings, common to all of them is that they fulfill three functions for mobilization processes. We consider two of these functions to be central: "First, they provide the context in which the all-important process of collective action can occur" (McAdam et al. 1988, p. 710). This implies framing the issues, causes, and solutions (Snow et al. 1986; Snow and Benford 1988).[5] Second, they "serve as the 'organizational staging ground' for the movement" (McAdam et al. 1988, p. 715) insofar as they provide members, leaders, and communication networks.

The concept of micromobilization contexts contains two ambiguities that we hope to clarify by specifying the concept. First, the authors do not distinguish between the mobilizing collective actors, on the one hand, and their environment, which consists of individuals, groups, and networks (the context in the strict sense of the word), on the other. We suggest that the term "micromobilization contexts" be limited to the environment and that the term "micromobilization actors" be used to indicate the mobilizing collective actors.

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[5] "The term 'frame' (and framework) is borrowed from Goffman to denote the 'schemata of interpretations' that enable individuals 'to locate, perceive, identify, and label' occurrences within their life space and the world at large. By rendering events or occurrences meaningful, frames function to organize experience and guide action, whether individual or collective" (Snow et al. 1986, p. 464).

Second, previous authors do not distinguish between the different levels of the mobilization process and the targets of mobilization. Micromobilization actors function mainly to mobilize individuals to participate in protests. We propose the use of the term *micromobilization potential* for those individuals who are the targets of micromobilization actors. However, the various micromobilization actors, who usually pursue their own special goals, constitute an atomized or only very loosely connected structure. In order to pursue a common campaign, these actors must first be linked together. Besides achieving the involvement of individuals in protest activities, the groups of micromobilization actors themselves, need to be linked and integrated. We assume that mobilization will be successful in quantitative terms only if these micromobilization groups can be linked to each other thereby allowing for a sort of "bloc recruitment" (Oberschall 1973, p. 117; Jenkins 1983, p. 62). The concept of micromobilization leaves open the question of how the micromobilization groups themselves are linked together. McAdam et al.'s (1988) claim to have bridged the macro-micro gap with their concept of micromobilization is only half fulfilled by the part of the bridge that extends from the microshore. We will make an explicit distinction between the micro- and mesolevels and specify the functions of the latter.

We introduce the term *mesomobilization actors* and by this we denote those groups and organizations that coordinate and integrate micromobilization groups.[6] The latter can, from the point of view of the mesomobilization actors, be considered as the *mesomobilization potential.* Mesomobilization actors play a role similar to that of micromobilization actors; in contrast to them, however, they mobilize not individuals but groups, organizations, and networks.[7] In fulfilling this task they have two functions. First, they provide a *structural integration* by connecting groups with each other, collecting resources, preparing protest activities, and doing public relations. Second, they aim at a *cultural integration* of the various groups and networks in developing a common frame of meaning. This serves to interpret the issue at stake and to link the specific concerns to this issue.

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[6] Neidhardt (1985, p. 197) has even suggested considering social movements as "mobilized networks of networks." Similarly, Gerlach and Hine (1970) have emphasized the horizontal more than the vertical linkage of social movement groups. The authors saw "segmented, polycephalous, integrated networks"—SPIN organizations—as an ideal type of social movement organization.

[7] For empirical analyses of the networks underlying a social movement see, e.g., Kretschmer and Rucht (1987) and Klandermans (1990).

Hence, the mobilizing structure for protest activities is twofold, consisting of both mesomobilization actors and micromobilization actors. The first link and integrate various micromobilization groups; the second motivate and mobilize individuals within and outside of these groups. Both types of actors are in a complementary relationship with each other. Together they form the intermediary structure that is a fundamental part of the micro-macro bridge.

We will develop and specify the concept of mesomobilization on a theoretical level and demonstrate its usefulness by investigating empirical cases. We have selected two prominent cases of mass mobilization in the recent history of the Federal Republic of Germany as an illustration: the protest campaign against the visit of Ronald Reagan in 1987 and the campaign against the World Bank and International Monetary Fund (IMF) Congress in 1988.[8] Both events took place in Berlin. With the two cases we want to show how mesomobilization actors (*a*) achieve a structural integration by organizationally connecting groups with each other and (*b*) achieve a cultural integration of the various groups and networks by developing a common frame of meaning to interpret the issue at stake.

Because no refined theory of mesomobilization exists, we use empirical material to develop hypotheses indicating which structural and cultural mesomobilization characteristics are important in a successful mobilization. In this respect, we are not testing a refined theoretical model but are using empirical material to develop theory and generate hypotheses.

It is not possible to compare the relative weight of mesomobilization factors with other variables referred to in the literature such as the political opportunity structure or the frequency and intensity of similar protest activities in the past. On a descriptive level, however, we can demonstrate first, that mobilization resulted not from an aggregation process of individual discontent, but through the activation and coordination of already existing protest groups and, second, that these groups combined their own primary concerns with the new issue. A master frame designed for the controversial issue was connected to the group-specific frames, thus allowing heterogeneous groups to be allied in the same mobilization campaign.

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[8] We assume that these preexisting mesomobilization contexts and the previously developed ideological frames also provided favorable conditions for the mass mobilization opposing the U.S. military engagement against Iraq in 1991. During this war, many large demonstrations also took place in Berlin where more than 100,000 people were mobilized for a single event.

II. A DESCRIPTIVE ACCOUNT OF THE TWO CASES

A. The Anti-Reagan Campaign

A short visit by U.S. President Ronald Reagan to Berlin was planned for July 12, 1987. Once this plan became known, protest groups began to mobilize for a protest campaign as they had on various other occasions, including that of Reagan's visit to Berlin five years earlier.[9] This campaign aimed at demonstrating that Reagan and the political course he represented were not acceptable to the groups protesting and that he was therefore not welcome in Berlin. The leaflet calling for the central mass demonstration said that Reagan "represents interests in the USA which will stop at nothing in their efforts to make the USA the undisputed world and military power" (our translation, see App. fig. A1).

The first considerations and initiatives for organizing a protest demonstration had already begun in December 1986. Preparation intensified in the weeks before Reagan's visit. Various events in these weeks—including clashes with the police, arrests, seizures, house searches, and the suicide of a political figure being held for questioning—had already increased the explosiveness of the political situation. This atmosphere was further heightened by the substantial expansion of the police force including the importation of West German police forces—and media hype regarding the "battle" surrounding the Reagan visit.

The day before the visit, about 50,000 people demonstrated with the motto, "We are saying no to Reagan's policy." A wide spectrum of 140 political groups had joined the call for the demonstration. Among these were the youth organizations of some parties and unions, left-extremist, humanistic, religious, feminist, peace-movement, and ethnic groups.

While Reagan gave his speech to about 20,000 people under heavy police protection (so that he could quickly escape the city relatively unscathed), there were several clashes in the city between demonstrators and the police. The Kreuzberg district, the stronghold of the militant political scene, was cut off from the outside world and public transportation to and from the area was shut down. Several hundred people were surrounded by police and detained for several hours. In the nights that followed, further clashes occurred in Kreuzberg including arrests, people's being chased through the streets, and the use of riot batons and tear gas. A number of innocent bystanders and journalists were injured.

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[9] Aside from this specific event, the political climate among the protest groups in this period was already heated. This was not merely due to the varying policies of the conservative administration, which used both the carrot and the stick in dealing with the protest groups. More specifically, tensions had been caused by the national census carried out at that time and the costly and resplendent ceremonies and festivals to celebrate the 750th anniversary of the city of Berlin. This anniversary, by the way, was the immediate reason for inviting Reagan to visit Berlin.

B. The Anti-IMF Campaign

The International Monetary Fund and the World Bank held their yearly congress in Berlin, September 21–27, 1988. All established parties in Berlin greeted the two bodies as welcome guests of the city. However, protest groups had begun mobilizing against this congress two years before the event. The emphasis of the protests was to be that the IMF and World Bank were not welcome in Berlin because the world economic order they represented actively promotes the exploitation of Third World countries by Western industrialized countries. The protesters hoped that the public mobilization would induce the two institutions to cancel the Third World countries' debts, which were perceived as indicative of this unjust world order.

During the process of mobilization and the protest campaign, we recorded a total of 475 different public actions (Gerhards 1991). These consisted of informational meetings, stage productions, sketches, memorial services, religious services, smaller demonstrations, and two large events: a countercongress with experts speaking against the policies of the World Bank and the IMF and a large demonstration. There were 133 groups calling for the demonstration against the IMF and World Bank congress and some 80,000 people who responded to the call to take part in the demonstration.

The preparations of the security agencies paralleled the preparations and activities of the protest groups. Police from throughout West Germany were again assembled in Berlin in large numbers; the spokesman for the city's Department for Internal Affairs said that this was the largest deployment of force in Berlin since the war. Here, again, the media dramatized the great battle between the congress's opponents and the security forces, and again there were violent clashes between the demonstrators and the police. The police's confining of several journalists for some hours aroused widespread public indignation and also had international repercussions.

We can conclude from the large number of support groups in both campaigns that preexisting micromobilization groups were successfully integrated into the process of mobilization. Moreover, this integration seems to have enabled the mobilization of a large number of participants in the mass demonstrations (they involved 50,000 and 80,000 participants, respectively). In addition, the activities were widely discussed in the mass media. In the case of the IMF conference, we counted 688 articles and short news reports in Berlin-based newspapers on the protest activities (Gerhards 1993).

In the following sections, we want to analyze in greater detail how and why mesomobilization actors were successful in integrating micromobilization groups. We will first analyze the organizational aspects of these integration processes and then the ways in which the issues were framed.

III. THE STRUCTURE AND DYNAMICS OF MESOMOBILIZATION

In this section we will describe which micromobilization groups were recruited to the two campaigns and how well the mesomobilization potential was tapped. We will then ask how the different groups were mobilized and coordinated and which mesomobilization actors participated in the mobilization. In conclusion, we will attempt to determine the factors that led to the successful mesomobilization.

A. Micromobilization Groups and Micromobilization Potential

We begin by analyzing two leaflets, both of them published several days before the mass demonstration. These leaflets give us condensed information about the successfully mobilized micromobilization groups and the content of each campaign. The design and layout of the leaflets are similar (see App. figs. A1 and B1). The front page includes the slogans and arguments in support of the demonstration and the call for action. The back page (not shown) lists, in alphabetical order, all the micromobilization groups that explicitly supported the call for the mass demonstration.[10]

The list for the anti-Reagan campaign includes 140 supporting groups and that of the anti-IMF campaign, 133 groups. When we examine the characteristics of the groups listed on each leaflet, the considerable heterogeneity of the groups becomes apparent. The list includes a wide range of organizational forms—such as branches of established political parties, environmental associations, loose circles of politically engaged individuals, sections of trade unions, neighborhood initiatives, international friendship committees, religious groups, cooperatives, student representative bodies, and so forth. In terms of social groups, they range from Turkish women to "U.S.-Americans for Peace," from "Revolutionary Workers from Iran" to lesbian groups, from "Artists for Peace" to self-help groups of unemployed. In ideological terms, they include orthodox Marxists, Greens, Christians, atheists, anarchists, liberals, and so forth.

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[10] These two lists do provide a complete listing of all supporting and/or participating groups. From media reports immediately following the events we know that the final number of "official" supporting groups was somewhat higher than the number given in the list. Moreover, there were also groups participating in the preparatory work and/or in the protest event itself who were not included in the list. This is particularly true for most of the militant groups called "Autonome." One reason for their absence from the list of individual groups is their semi-underground status, these groups are simply not interested in being publicly identified. Moreover, because of their highly radical stance, some of these groups disagreed with the claims and slogans of the leaflet and therefore would not explicitly support this specific call for action, although they took part in the demonstration and/or related activities.

To understand the composition of these micromobilization groups better, we aggregated groups with the same focus of activity and classified the sets according to their objectives.[11] Groups that were oriented toward more general objectives and could therefore be seen as multi-issue groups were classified as "groups with non-specific aims." Groups with a single specific concern were labeled "issue groups." This category also includes groups especially created to prepare the protest event (anti-IMF groups and anti-Reagan groups). Table 1 gives an overview of the distribution of the various kinds of groups supporting the two protest events.

Although roughly the same number of groups signed the call for a demonstration in each campaign, the composition of the two groups varied. First, only Berlin-based groups participated in the anti-Reagan campaign; whereas the IMF campaign also involved 22 groups from other West German cities and other countries. Obviously, the IMF conference was perceived as an event which went beyond local and even national interest. Although its mobilization had a greater spatial dimension, the anti-IMF campaign mobilized fewer local groups. There seems to have been a trade-off here between the territorial scope of the mobilization and the success rate of the local mobilization.

Second, the attractiveness of the two campaigns varied among specific categories of groups. For example, more peace groups and student/youth groups were mobilized in the anti-Reagan campaign, whereas more Third World groups and women's groups supported the anti-IMF campaign. The absolute number of groups mobilized, however, is not very meaningful because the total number of existing groups in each category could have changed between the campaigns. For this reason we have preferred to define the local mesomobilization potential of the two campaigns in a manner analogous to that of Klandermans and Oegema's (1987) defini-

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[11] Of course, attributing some groups to these main categories is arbitrary. For instance, a group called "Women for Peace" could be classified either among women's groups or peace groups. Our general rule was to categorize a group according to its dominant, or more stable, collective identity. In this particular case, knowing that most women in this group were and still are engaged in other women's issues, we classified the group under women's groups.

TABLE 1

SUPPORT FOR THE ANTI-REAGAN AND ANTI-IMF DEMONSTRATIONS

KINDS OF GROUPS	ABSOLUTE NO.		
		Anti-IMF	
	Anti-Reagan	Total	Berlin-based
Groups with nonspecific aims:			
Groups of general infrastructure	2	4	3
Party organizations	16	11	5
General political groups	6	5	4
Christian groups	11	8	8
Issue groups:			
Event-specific groups	0	2	1
Peace groups	33	18	17
Third World groups/international cooperation	12	29	21
Human rights/citizenship	8	8	8
Women's groups	13	22	22
Ecological/antinuclear groups	6	6	5
Cultural groups	11	3	3
Student and youth groups	10	6	3
Ethnic groups	3	1	1
Trade unions	4	4	4
Neighborhood groups/urban problems	4	4	4
Nonclassified groups	1	2	2
Total	140	133	111

tion of mobilization potential at the individual level. This will allow us to determine how much of the overall mesomobilization potential—measured as the total number of existing micromobilization groups in various segments of the leftist alternative milieu in Berlin—was activated in each campaign. Drawing on *Stattbuch 3 Berlin* (1984) and *Stattbuch 4 Berlin* (1989) (a collection of self-portrayals of groups and organizations in the alternative milieu in West Berlin), we have obtained rough indicators of the absolute size of various alternative segments at different points in time.[12] We have obtained figures from these sources about the size of some of the segments. In table 2, these figures are compared with those of the Berlin-based support groups in the two protest events.

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[12] Again, our analysis remains incomplete. Unfortunately we do not have data on all the types of groups listed in table 1. Moreover, we cannot ascertain the completeness of the *Stattbuch* or whether groups included here are representative. Our current research project, which analyzes the size and changes in the "alternative" movement sector in Berlin using four editions of the *Stattbuch* and additional sources, will allow us to answer these questions.

TABLE 2

Activation of Selected Segments of the Alternative Movement Sector in West Berlin

Kinds of Groups	Total No. of Groups in Berlin		Berlin-based Support Groups			
			Anti-Reagan		Anti-IMF	
	1987[a]	1989[b]	*N*	%[c]	*N*	%[d]
General political groups	41	50	6	14.6	4	8
Peace groups	42	27	33	78.6	17	63
Third World/international cooperation	41	37	12	29.3	21	56.8
Women's groups	125	129	13	10.4	22	17.1
Ecological/antinuclear groups	73	75	6	8.2	5	6.7
Cultural groups	328	324	11	3.4	3	0.9
Ethnic groups	62	75	3	4.8	1	1.3
Neighborhood groups/urban problems	62	56	4	6.5	4	7.1

[a] No. of groups in 1987 is the mean of the groups registered in the 1984 and 1989 editions of the *Stattbuch Berlin*.

[b] No. of the groups registered in the 1989 edition of the *Stattbuch Berlin*.

[c] Percentage of support groups in relation to the total *N* of groups in 1987.

[d] Percentage of support groups in relation to the total *N* of groups in 1989.

As the data indicate, groups and organizations with general political aims were not very much mobilized in either campaign. Because both events—the visit of a U.S. President, representing the world's most powerful nation, on the one hand and a major international congress, representing the "world capitalist interests," on the other—are highly significant in political terms, we expected that the groups with general political aims could be relatively easily mobilized and therefore should be overrepresented. We have no explanation for why this was not so, and we have no comparative data with which to determine if this low level of activation is unusual for these kinds of protest events.

Looking at the actual mobilization rate of issue-specific groups, we can see that the anti-Reagan campaign mobilized more than 75% of the total of the local peace groups and about 30% of the Third World groups. In the anti-IMF campaign, the mobilization rate of the peace groups was lower, but many more Third World groups were activated than in the anti-Reagan campaign.

Activation rates of other single-issue groups were significantly lower.[13] Given the fact that U.S. President Reagan was perceived as an exponent of military bloc confrontation and a risk to peace, it is no wonder that his visit could mobilize so many peace groups in Berlin. Similarly, the IMF and World Bank, which are heavily engaged in Third World countries, mobilized a high share of the Third World groups in Berlin. It seems that preexisting single-issue groups whose concern is close to the topic of the event can be relatively easily mobilized, and vice versa. This bridging between group concerns and the event, however, is not an automatic act but a social construction and the result of an organizing effort (see Sec. IV). We also found that there was surprisingly little overlap between the supporting groups in the two campaigns; only roughly one-third (47 groups) participated in both campaigns. Therefore, despite the fact that both triggering events were rather similar and that they occurred in the same city within a time span of only 15 months, each protest event seems to have had its own rationale and dynamics.

In summary, we can state that both protest campaigns were supported by a wide range of political, humanitarian, religious, and cultural groups. In the anti-IMF campaign, the Third World groups played a dominant role; in the anti-Reagan campaign, the peace groups were the most important. In each case the mobilization potential of micromobilization groups in these two areas was highly activated. This can be interpreted as a success for the organizers.

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[13] Even women's groups and neighborhood groups which, at least at first glance, are not concerned by the nature of the triggering event, were activated to a certain degree (ranging between 6.5% and 17%). We will later show that this was a result of a successful frame bridging (see Sec. IV, C).

B. The Organizational Process and the Role of Mesomobilization Actors

How were the support groups linked to each other? We use several interviews with important organizers in both campaigns and notes from meetings of organization groups to shed some light on the underlying structure and on the process that enabled the bloc recruitment of groups and organizations.

In the anti-Reagan campaign, which we label COORD A, a relatively large and stable network of local groups played a key role.[14] It not only gave the initial impulse for the campaign but also remained its control center during all of its phases. The COORD A network had been established during the rise of the new peace movement in the early 1980s. It embodied a broad range of groups that go far beyond the peace movement, including religious, humanistic, and political groups, with rather different ideological backgrounds, aims, and tactical preferences. One or more representatives of these groups usually met each other once a month to exchange information and discuss common concerns.

When members of this network became aware that President Reagan was to visit Berlin, they decided to launch a protest campaign. As in past joint protest activities, they created a special task force (*Arbeitsgruppe*) to prepare and coordinate the anti-Reagan campaign. This group involved experienced delegates from the core organizations of the network. Apart from organizational and technical matters, the task force was also engaged in the "meaning-work" (see Snow and Benford 1992, p. 5) of formulating a first draft of the leaflet calling for a common action. This first version was produced three months before the protest event. The authors of the draft then either presented it personally to some core groups of the network or mailed it to get reactions from these groups. Although sending a letter seems to be a simple mobilization technique, it was relatively successful because the letter's recipients were groups that had established contacts with the network and been mobilized in previous protest campaigns. Some minor revisions were made on the basis of the feedback from various groups. Together with a letter asking for official support, the final version of the leaflet was then sent to the outer circle of groups belonging to COORD A, as well as to other groups who might support the call for action. Parallel to this, the core organizers directly contacted members of other groups with whom they had personal ties. Through this process of direct and indirect activation the number of support groups grew from 38 initial endorsements in April 1987 to the 140 groups that eventually signed the call for action and were thus represented in the official list of support groups.

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[14] We use a code name for this and similar networks in order to guarantee their anonymity.

For the IMF campaign, the pattern of mobilization deviated to some extent from that of the anti-Reagan campaign. From the very beginning, since the IMF campaign was conceptualized as both a Berlin campaign and a national campaign, it had a twofold structure. For the purpose of the national campaign, COORD B, together with a special subcommittee, served as the basic coordinating group. The network we call COORD B is a nationwide network of highly politicized leftist Third World groups. At the core of COORD B, three full-time, paid organizers coordinated the various groups and organized the campaign. Two additional organizers were employed by COORD B in Berlin during the last three months preceding the IMF conference. The national Green Party provided much of the money needed to run the campaign. In Berlin, the various activities were coordinated by a separately created committee, COORD C, which included several of the groups listed in table 1.[15] Among these was the *Alternative Liste* (AL), a left-liberal party on the state level that later joined the national Green Party. Given the resources available to a party in the state parliament—money, staff, infrastructure—the AL played a crucial role for the Berlin-based organization in the campaign. The AL employed three people for nine months to prepare the campaign. At a later stage of the preparatory work, COORD A, which had coordinated the anti-Reagan campaign, after having formed its own coordinating group for the anti-IMF campaign, joined COORD C. The first draft of the central leaflet was written by four people from COORD C who were selected to represent most of the political spectrum of the mobilizing groups. The leaflet was then sent to the groups belonging to COORD C and to other groups who were to support the call for action.

Because the planning called for a broad range of protest activities beyond the central mass demonstration, COORD C formed a subgroup for each of the specific tasks. These groups were to prepare (*a*) a "counter-congress," (*b*) the mass demonstration, (*c*) the week of daily protest activities, (*d*) the measures for influencing the mass media, (*e*) the publication of an anti-IMF journal, and (*f*) the coordination with non-Berlin-based protest groups. In view of this sophisticated division of labor and the large investment of personal and financial resources, we can conclude that the organization and coordination of the anti-IMF campaign was much more professionally and organizationally elaborated than the anti-Reagan campaign (see fig. 1).

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[15] In addition, the so-called "Autonome," the most radical wing of militant activists, had established a coordinating group which, however, could only represent its own political spectrum. Because these coordinating groups were largely independent from each other and had different opinions about tactical questions, their relationship was marked by some tensions and rivalry. Only in the last few weeks before the protest event could these dividing lines be overcome and common activities become less problematic.

The special committees described above—the task force for COORD A in the anti-Reagan campaign and those of COORD B and COORD C in the anti-IMF campaign—played a crucial role in the mobilization process. They formulated a platform for joint action, collected resources such as money and technical equipment, mobilized the outer circle of the network, and even negotiated with the police on the eve of the protest events. These preparatory teams served as professional or semiprofessional planners and organizers on the mesolevel, whereas the numerous activities designed to inform and mobilize sympathizers and the public at large were carried out by the micromobilization groups of the inner and outer circles of the overall network. Activated members of these groups mobilized their own constituency and probably also mobilized friends and acquaintances.

C. Factors for Successful Mesomobilization

What were the preconditions for this effective mobilization and coordination of micromobilization groups through mesomobilization actors? Four factors appear to us to be especially relevant.

The first is that the basic structure of the network of micro- and mesomobilization actors that fueled the two campaigns in the second half of

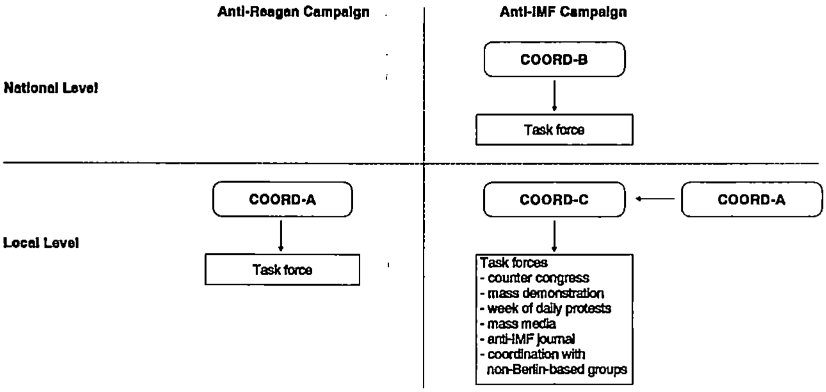
Anti-Reagan Campaign
Anti-IMF Campaign
National Level
COORD-B
Task force
Local Level
COORD-A
Task force
COORD-C
COORD-A
Task forces
- counter congress
- mass demonstration
- week of daily protests
- mass media
- anti-IMF journal
- coordination with non-Berlin-based groups

FIG. 1.—The organizational structure of the anti-Reagan and the anti-IMF campaign.

the 1980s had already been formed during the first years of the decade. The COORD A network, in particular, emerged with the rise of the new peace movement in the early 1980s, although it included many protest groups that had been formed previously. Delegates from about 40 of the existing groups had met regularly over several years, thus creating close interpersonal ties and overlapping memberships. The network had previously carried out activities similar to those of the anti-Reagan and anti-IMF campaigns several times, including a large anti-Reagan demonstration in June 1982.[16] In comparing the 175 support groups from this demonstration with the 140 support groups in 1987, we found that 50 groups were identical. Given the fact that a certain share of the 1982 support groups no longer existed in 1987 and that some of the 1987 support groups did not yet exist in 1982, this shows the continuity of the micro- and mesomobilization actors. Hence, the mobilization for the two campaigns we focus on was based on a preexisting network of experienced groups. Since relatively large common protest campaigns had been previously organized, there was a certain probability that many groups belonging to the local network might also join in the next campaign. What McAdam (1988) has found in a different context could be demonstrated in our cases as well: The leftist community is held together by a dense network of overlapping "bridging" ties that link the various groups together. We have identified these bridging ties on two levels, an underlying, more or less permanent, infrastructure and a specific structure exclusively designed for the concrete campaign.

Besides remarking on the West Berlin protest sector's continuity over time, it is important that we stress its extraordinary size. Berlin had an especially well-developed alternative protest sector (Claesens and de Ahna 1985). The city was considered "the capital" of the leftist and alternative movements in West Germany (Roth 1989). In comparison with other cities in the Federal Republic of Germany, not to speak of many other countries, West Berlin's conditions for protest were especially favorable.[17]

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[16] This demonstration was even larger than that in 1987. According to the organizers, about 100,000 people participated in 1982.

[17] We have to keep in mind that West Germany as a whole is probably also exceptional in regard to these kinds of activities. Looking at the next IMF and World Bank conference after that in Berlin, it becomes obvious that both the size of the mass demonstrations and the violent activities accompanying the event in Berlin were far from being "normal." The congress, held in September 1989 in Washington, D.C., provoked little protest. There were only a few oppositional groups present. Some 50 people came together for a parallel meeting in a Presbyterian church. Some members of more militant U.S. groups such as Earth First and Rain Forest Action Network also organized a small blockade which, in accord with an agreement with the police,

Generalizing these findings, we suggest that the greater the number of preexisting contacts among the mesomobilization actors, the greater the number of successful protest activities they have carried out in the past, and the more developed the overall local infrastructure of protest movements, the more successful the campaign will be on the level of mesomobilization.

The second factor that probably facilitated the mobilization process in the two campaigns was the organizational flexibility and ideological pluralism of the mesomobilization actors. To begin with, the coordination networks were generally open to all people and groups who wanted to participate. As a consequence, each group had a chance to influence the envisaged protest activities in the early planning stage. Then, each group decided autonomously whether, and under what conditions, it would participate. Even a common decision to undertake one kind of activity, for example, a protest rally, usually allowed for various special activities within this broad framework of action, such as performing street theater, distributing group-specific flyers, or selling buttons. Finally, there was hardly any pressure for ideological conformity. Groups coming from very different movements and political backgrounds were invited to participate. We therefore hypothesize that the greater the organizational flexibility and ideological plurality of the mesomobilization actors, the higher the activation rate of the mesomobilization potential.

Third, our description of the process of actual mesomobilization has shown that this work was done by specifically created organizational bodies with experienced organizers and was based on an internal division of labor. In the anti-IMF campaign, a great deal of the preparatory work was even done by staffers hired exclusively for this purpose. For reasons we will refer to in the concluding section, we believe that more organizational efforts were needed to mobilize the same amount of groups in the anti-IMF campaign than in the anti-Reagan campaign. We therefore hypothesize that the more experienced and professional the mesomobilization actors are and the greater their resources and their division of labor, the more successful the mobilization.

Fourth, although the historical continuity and institutionalization of mesomobilization actors, their organizational and ideological openness and, finally, their professionalism and division of labor were good starting conditions for the activation of micromobilization groups, momentum also developed and increased the mobilization chances further. The first

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did not last longer than four minutes. In addition, some 60 nongovernmental organizations held a congress which, however, was hardly noticed by the international press. With this level and type of mobilization in mind, it becomes obvious how successful the Berlin groups were in terms of mass mobilization.

draft of the leaflet had already been signed by so many groups that, in sheer numbers, a critical mass existed. Klandermans and Oegema (1987) have demonstrated that the probable number of people participating in a protest event has a positive effect on the individual motivation to take part in this event. We suspect that this connection is also true for micromobilization groups. When a certain threshold in the number of groups has been exceeded this has a positive effect on the willingness of other groups to join the mobilization. Momentum was further increased by the fact that the initial signers of the call for action, (*a*) covered a wide ideological spectrum, (*b*) included groups that enjoyed an excellent reputation within the leftist-alternative protest milieu, and (*c*) also consisted of groups that belonged to important umbrella organizations or movement segments. This presumably made it easier for the marginal groups to take the step of joining the mobilization. In summary, we formulate the following hypothesis: the greater the number of groups who initially sign the call for action, the greater their ideological range, the better their reputation, and the more these groups belong to important umbrella organizations or movement segments, the more other groups will join the call for action.

No matter how strong, broad, interlinked, and professional the organizational structure for mesomobilization is, it only provides the structural basis for a mobilization process. A second crucial task for a successful mobilization is an adequate framing of the issue. The issue at stake has to be perceived as important and provoking, the forms of the proposed action must be acceptable, and the organizers and allies must appear to be reliable. In order to activate the existing structure for concrete support and to attract many micromobilization groups from the mobilization potential, this "hardware" structure has to create and apply a kind of "software." In the second major step of our analysis, we therefore turn to the framing processes.

IV. FRAMING THE ISSUES

Framing processes play a decisive role in mobilization campaigns. The best chance for protestors to influence society consists in their capacity to make their definition into a public definition of the problem, to convince as many groups and people as possible by their framing of the situation, to create support for their cause, and to motivate others to participate in the protest (Klandermans 1988). Promoting public definitions of problems and their solutions is not just one of many components of protest campaigns and social movements that have to be considered in an analysis; instead, to the extent that movements can exert influence only through mobilizing the public, it is the key factor. Protest movements usually have no other resources or only small amounts of them (money, power, connections to decision makers) at their disposal (Neidhardt and Rucht 1991, p. 452).

Although the importance of framing to mobilization processes has been emphasized time and again in recent years and theoretical concepts for dimensioning have been developed (Neidhardt 1985; Snow et al. 1986; Snow and Benford 1988, 1992; Gamson 1988; Gamson and Modigliani 1989; Klandermans 1988, 1992), there are no empirical analyses of frames and framing processes. The following considerations begin to fill this research gap.

As in the analysis of the structure of mesomobilization, the current literature does not allow us to apply a deductive procedure to test hypotheses in analyzing frames. For this reason we will be developing appropriate hypotheses inductively, using our empirical material. In the first step, we attempt to describe the general interpretive frames of the two mobilization campaigns and look for the central interpretive frames, the master frames (Snow et al. 1988, p. 475), which integrated the different groups and motivated them to participate in the protest campaigns. In the second step, we compare the two master frames and develop hypotheses about the factors that influence the mobilizing capacity of frames. In the third step we finally move from the level of mesomobilization to that of micromobilization. We look at the extent to which the different micromobilization groups that supported the mobilization, but primarily pursued other objectives, could combine their own group-specific frame with the IMF issue or with the Reagan visit. That is, how did they succeed in frame bridging? (See Snow et al. 1986.)

Before we start our analysis, however, we need to make some methodological reflections. The problems with using empirical methods to analyze frames result from the fact that the objects of the analysis are texts and frame analysis refers to *the system of meaning* represented by these texts. Conventional, quantitative-content analysis does represent an intersubjectively verifiable procedure for data analysis. It would not, however, do justice to the goal, which is reconstructing the system of meaning represented by texts, because it breaks up the text into isolated elements—topics, statements, and arguments. Hermeneutic qualitative procedures do reconstruct the context of meaning of texts, but it can be argued that the text analysis procedure, itself, represents a subjective interpretation.

In our analysis we will pursue two strategies to compensate for the methodological problems of a frame analysis mentioned above. First, the difficulties of data analysis can be at least partially compensated for by the selection of the texts, by sampling. We consider the two leaflets signed by all supporting groups as valid indicators for the groups' common frames.[18] The use in each case of only one leaflet signed by the support groups reduces the text corpus to only two pages.[19] At the same time, however, we think that the two pages can be considered to be a highly meaningful source.[20] The reduction of the text corpus to limited, but meaningful material makes it possible to present not only the interpretation, but also the texts themselves (see App. figs. A1 and B1), so that the reader can check the plausibility of the frame analysis.

Second, to analyze the leaflets, we draw on a method for analyzing decision-making processes developed by Robert Axelrod (1976). Using this method, the internal structure of frames can be investigated with the help of a graphic presentation of the argumentative structure of the frames.[21] The graphic presentation takes the form of a directed graph of points and the arrows between the points. The total information of the texts is thus reduced to the bare bones of the argumentation. The degree of abstractness of the frame, the range of the topics addressed, and the logical connection of the arguments thus become visible and comparable between different frames.

A. Master Frames[22]

To be successful, mesomobilization actors must not only organizationally link and coordinate a heterogeneous set of groups, but also integrate these groups ideologically. This idea has recently been elaborated by David Snow and Robert Benford. The authors refer to the ecological scope of a master frame defined as "the diffusion of movement activity across different population and organizational sectors of society" (Snow and Benford 1992, p. 26). The notion of ecological scope parallels our conception of a master frame's capacity to integrate various micromobilization groups.[23] In the following section, we will take a closer look at the overarching frames that are shared by all the support groups in both protest campaigns.

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[18] We believe that the two leaflets represent a commonly shared interpretation of Reagan's visit and the IMF conference, respectively. This is an interpretation that marks both an internal consensus and what is meant as a position to be presented to the external world.

[19] We will only refer to additional sources such as group-specific leaflets for the analysis of frame bridging.

[20] Bert Klandermans (1992) differentiates three levels of public discourse where processes of meaning construction take place: the macrolevel, the mesolevel, where movement organization and opponents practice persuasive communication, and the individual level, where consciousness raising takes place. In the following, we concentrate solely on the mesolevel and only on the frames of the mobilizing actors. The frames of other actors (media, state), including competing frames, are excluded from the analysis for practical reasons, even though we concur with Klandermans that frames are always constituted in opposition to and in conflict with alternative frames.

[21] The term, structure of argumentation, could be defined similarly to Axelrod's definition of cognitive maps: "A cognitive map is a certain way of representing a person's assertion about beliefs with respect to some limited domain, such as a given policy problem. The representation takes the form of a directed graph of points and the narrows between those points" (Axelrod 1976, p. 72).

[22] Before we begin the analysis of our empirical material, there is a need for terminological clarification. The concept of framing, as it was introduced in the study of social movements by Snow et al. (1986), was developed mainly on the basis of symbolic interactionism. It does not have much of a theoretical tradition in political sociology,

We can reconstruct a relatively closed and highly conceptualized interpretive frame, both from the leaflet written by the mobilizing actors for the demonstration against the Reagan visit (App. fig. A1) and from the leaflet for the demonstration against the IMF congress (App. fig. B1). The frame in each case consists of a set of arguments that are linked and complement each other. The frame for the IMF congress will be termed as an "ideology of imperialism," the frame for the Reagan visit as a "hegemonic power ideology."[24] Because both frames are highly conceptualized we use the term ideology.[25] We will first describe the structure of the argumentation of both frames in the following two sections and

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however, which deals with phenomena termed as "belief systems," "ideologies," "cognitive schemes," etc. As the literature in this field is not clear and precise, we would like to introduce some definitions. For our all-encompassing category we refer to belief systems, as suggested by Philip E. Converse (1964, p. 297): "We define a belief system as a configuration of ideas and attitudes in which the elements are bound together by some form of constraint or functional interdependence." In a second step, depending on whether belief systems refer to individuals or collectivities, we can distinguish between individual belief systems and collective belief systems. In the former, the concept refers to individual configurations of interrelated ideas and attitudes situated in the minds of individuals. These ideas serve to interpret the world. The category of individual belief systems is meant to be synonymous with that of cognitive schemes (for an overview, see Schissler and Tuschhoff 1988). In the second case, it refers to the interpretative patterns of collective actors as they are presented, e.g., in programs, resolutions, and leaflets. We suggest the term "frame" for the belief systems of collective actors. Both cognitive schemes (individual belief systems) and frames (collective belief systems) may involve different degrees of conceptualization (Converse 1964). If cognitive schemes are conceptually elaborated, we refer to them as ideological schemes; if frames are conceptually elaborated, we call these ideologies.

[23] A reviewer led us to the still-unpublished paper of Snow and Benford.

[24] Neither "imperialism" nor "hegemonic power" is a term found in the leaflets themselves.

[25] Summing up the respective literature, Hans-Dieter Klingemann (1983, p. 327) defines ideology "as a *far-reaching system* of attitudes whose dominant values and principles involve a high degree of *commitment* and which are *stable* over time. Typically, ideology is represented by *groups* whose interests shape the degree of reality present in the content of their statements."

then compare them in order to develop hypotheses concerning the mobilizing capacity of frames.

The Ideology of Imperialism as a Master Frame

1. The IMF and the World Bank are seen as central institutions of the world economic order.
2. The world economic order is, in its basic structure, an order designed for the exploitation of the Southern Hemisphere countries by the countries in the Northern Hemisphere.
3. The causes for the exploitation are to be found in the capitalist character of this order. This emerges as imperialism in the relationship between North and South and as capitalism within the individual countries.
4. The IMF and the World Bank—with a majority of their members coming from the northern countries—support this system in two ways. On the one hand, they take part in the exploitation through their own project financing and their awarding of loans and determining the conditions for repayment. On the other, these institutions serve as a guide for the policies of the banks and corporations. After the World Bank and the IMF force countries to pursue a capitalist course, then the banks and corporations of the industrialized countries come in and exploit them. Together, arguments 1–4 form the "theory of the world economy."
5. The consequences and problems ensuing from the system of world economy are numerous. They are the direct cause for protest activities. The most important consequences are the large indebtedness of Third World countries and the misery and death among the people living there, the destruction of the ecological requirements for life, the particular burdens on women, the weapon exports to the Third World as a special form of exploitation, the exodus of impoverished people who then seek asylum in the First World, the destruction of cultural identities, and finally, unemployment and reductions in the social welfare system in the First World as consequences of the same capitalist system.
6. The demands of the protesters follow from points 1–5: the problems formulated can only be solved by reformation of the world economic order, and this is what the protesters call for. They do not want Berlin to play host to such representatives of a problem-producing world economic order as the IMF and the World Bank.

Similarly to the manner in which Robert Axelrod (1976) has attempted to identify the structure of decision-making processes of elites by graphic presentations of cognitive maps, the structure of the arguments encompassed by the ideology of imperialism can be reconstructed and demonstrated schematically (see fig. 2).

The Hegemonic Power Ideology as a Master Frame

The anti-Reagan campaign had a similar, although not as elaborately developed, frame.

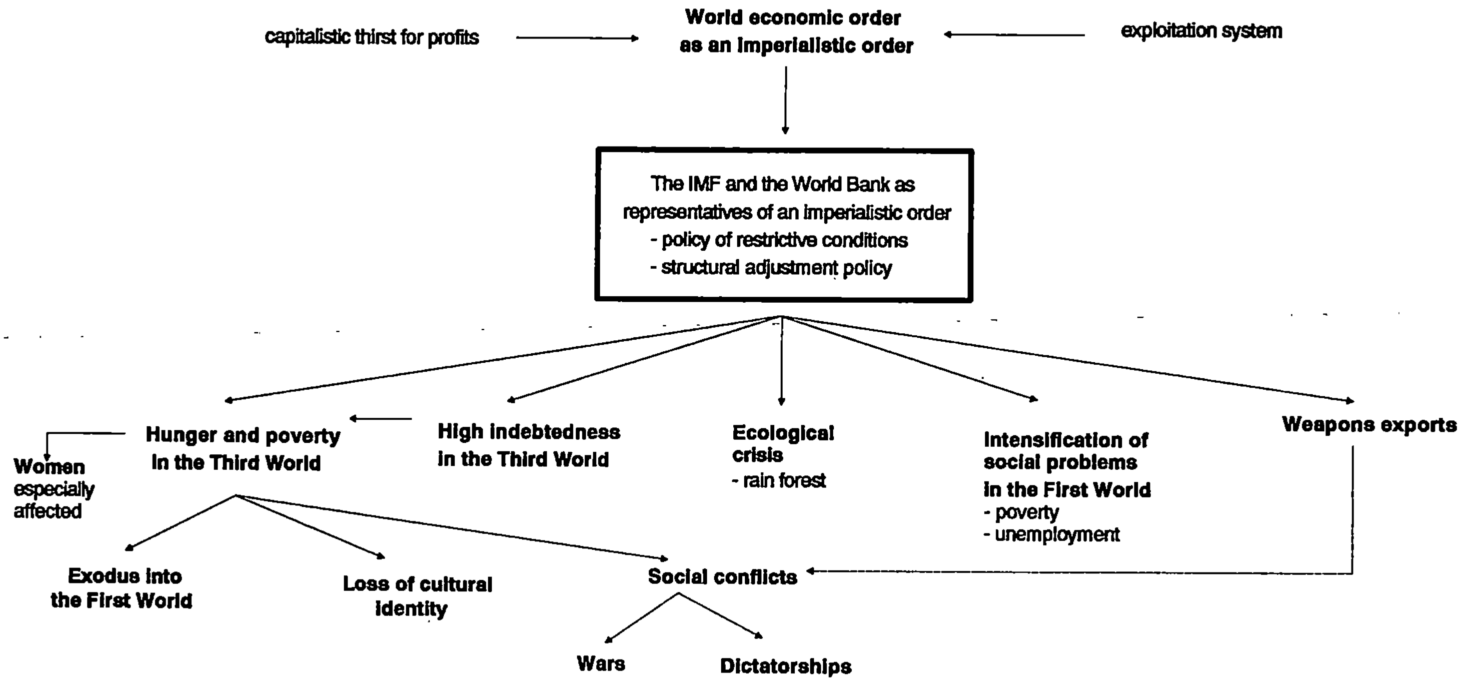
capitalistic thirst for profits
World economic order as an imperialistic order
exploitation system
The IMF and the World Bank as representatives of an imperialistic order
- policy of restrictive conditions
- structural adjustment policy
Hunger and poverty in the Third World
High indebtedness in the Third World
Ecological crisis
- rain forest
Intensification of social problems in the First World
- poverty
- unemployment
Weapons exports
Women especially affected
Exodus into the First World
Loss of cultural identity
Social conflicts
Wars
Dictatorships

FIG. 2.—Master frame of the anti-IMF campaign: imperialism ideology

1. Ronald Reagan was interpreted as a representative of certain circles in the United States who were striving for their nation's unrestricted economic and military dominance in the world.
2. This striving for hegemony found its empirical expression in four different areas:
    - *a*) The United States was carrying out a unilateral armament against the Soviet Union ("crusade against the East"). The SDI program and the stationing of medium-range missiles in Europe were examples of this.
    - *b*) The United States was carrying out a policy of military intervention. The bombing of Libya, the invasion of Grenada, and the mining of Nicaragua's harbors had shown that.
    - *c*) The United States was supporting the apartheid regime in South Africa and the Contras in Nicaragua and was delivering weapons to the Persian Gulf War for strategic reasons.
    - *d*) With the help of the IMF and special military forces, the United States was carrying out the economic subjection of the Third World.
3. At the center of the hegemonic power ideology was the accusation of unilateral armament. Following this line of argument, the possible consequences of an armament policy were explained in greater detail. Unilateral armament not only increased the danger of a new world war, it also had unacceptable consequences during periods of peace ("Arms do not only kill in war"): poverty, mass unemployment, and the reduction of social services were understood as consequences of the armament policy, women were interpreted to be a group hit especially hard by these consequences.
4. The demands of the protesters followed as conclusions from this chain of argumentation.
    - *a*) The protesters reject armament, interventionist policies, support for unjust regimes, and the exploitation of other countries.
    - *b*) The protesters do not want as a guest the American president who is responsible for the problems that have been defined. Berlin ought to be a city of peace and understanding, a city open to the victims of war and exploitation.[26]

Again, the structure of the argumentation can be presented schematically (see fig. 3).

B. The Mobilizing Capacity of the Two Master Frames: Some Hypotheses

Frames developed by mesomobilization actors aim at convincing as many groups as possible to adopt their interpretations of the world and at motivating as many groups as possible to participate in protest activities.

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[26] A further chain of argumentation begins at this point. The protesters accused the Berlin Senate of wanting to divert attention from Berlin's actual problems (rescinding the rent restraints, the struggle for the 35-hour workweek, the discussion of the national census, the reduction of democratic rights) with the invitation to Reagan.

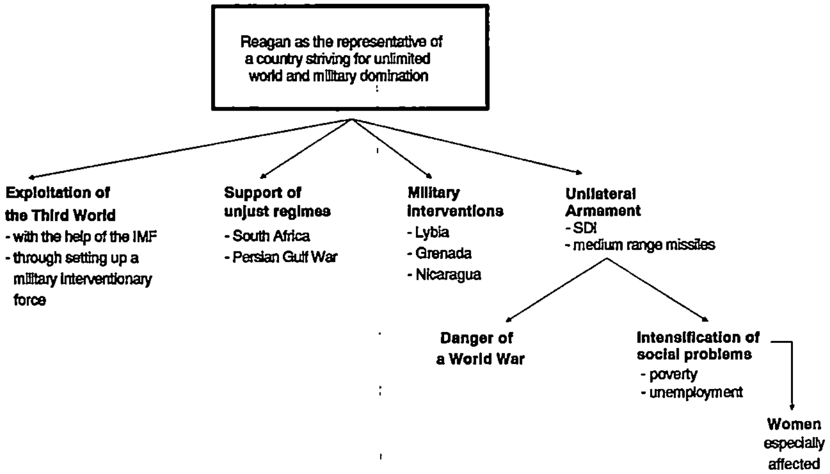
Reagan as the representative of a country striving for unlimited world and military domination
Exploitation of the Third World
- with the help of the IMF
- through setting up a military interventionary force
Support of unjust regimes
- South Africa
- Persian Gulf War
Military interventions
- Lybia
- Grenada
- Nicaragua
Unilateral Armament
- SDI
- medium range missiles
Danger of a World War
Intensification of social problems
- poverty
- unemployment
Women especially affected

FIG. 3.—Master frame for the anti-Reagan campaign: hegemonic power ideology.

Besides systematically describing the arguments embedded in frames, we can try to generate hypotheses about the variables that influence the mobilizing capacity of frames. In this regard, David Snow and Robert Benford (1988) distinguish between variables referring to the *internal structure* of frames and variables that permit *linking frames to the external dispositions* of potential recipients.[27] First we will focus on the internal structure, namely the argumentative logic of frames, and look for internal characteristics that could increase their capacity to convince groups and people.[28] According to the work of John Wilson (1973) and Snow and Benford (1988, pp. 219 ff.), successful framing depends on three elements: diagnostic, prognostic, and motivational framing. We will draw on these three dimensions in order to compare the internal structure of two master frames.

Diagnostic Framing

"Diagnostic framing involves identification of a problem and the attribution of blame and causality" (Snow and Benford 1988, p. 200). Both frames define not just one but a multitude of problems. The fact that we are speaking of problem definitions here does not mean that the problems are invented, that the indebtedness crisis does not exist for many countries in the Third World, and that the United States does not pursue an interventionist policy. It is only that these situations have to be labeled as problems in order for them to become problems. A comparison of the number of elements in the last lines of figures 1 and 2 shows that the number of interpretable problems, the *range and diversity of the problems* defined, is greater in the case of the imperialism ideology.[29] Whereas the anti-Reagan campaign primarily addressed problems related to the issue of peace, the topic catalog of the IMF campaign ranged from the peace issue to the ecological one and to other questions. The range and multitude of the problems defined by the master frame creates points of leverage for a host of political groups focusing on one or several of these particular problems. If we generalize the results of the two cases, then we can formulate the following hypothesis: The larger the range of the problems covered by a frame, the larger the range of societal groups who can be addressed with the frame and the greater the mobilization capacity of the frame.

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[27] In describing both sets of variables, Snow and Benford (1988, pp. 205 ff.) introduce the somewhat clumsy categories "infrastructural constraints of belief systems" and "phenomenological constraints." The latter are subdivided into the dimensions "empirical credibility," "experiential commensurability," and "narrative fidelity."

[28] We cannot test whether the factors we consider important for mobilization processes have actually led to increased mobilization of micromobilization groups. Our hypotheses can be made only theoretically plausible.

The fact that the anti-Reagan frame centers on the issue of peace explains the high mobilization rate for peace groups previously described. The central placement of the Third World topic in the anti-IMF frame explains the high mobilization rate for Third World groups.

The connection we formulated between the range of a frame and its mobilization capacity is only valid under certain conditions. A high mobilization capacity cannot be secured through the sheer range and number of problems enumerated. It is important that these problems can be plausibly connected to each other. Otherwise, too broad a range of problems could lead to an overextension of the frame (Snow and Benford 1988, p. 207). The different problems defined by the frames of the two campaigns are not unconnected but understood as different consequences of one system. Both the imperialism and the hegemonic power ideology try to tie disparate problems together into a meaningful context. The degree of *interrelatedness* (Converse 1964, p. 256) between the defined problems seems to be high. The heterogeneity of the problems can be interpreted as the different consequences of one and the same pattern. This fact is illustrated by the arrows in the graphs of figures 1 and 2, which indicate an argumentative connection between problem definitions. In regard to which factors determine the mobilization capacity of frames, we can derive the following hypothesis: the better the various defined problems can be argumentively connected with each other through a master frame, the more plausible the master frame appears, and the greater the mobilization capacity of the master frame.

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[29] Converse (1964, p. 208) refers to this dimension as the "range of objects that are referents for the ideas and attitudes in the system." See also Snow and Benford (1988, pp. 206–7).

In comparison with the hegemonic power ideology, the ideology of imperialism does not only have a broader, interrelated range; its structure is at the same time more abstract and *more generalized*. This can again be shown by figures 1 and 2. The number of hierarchically ordered levels is greater in the anti-IMF ideology. Whereas the hegemonic power ideology begins directly with the definition of Ronald Reagan as the representative of a hegemonic world power, the imperialism ideology situates the definitions of the IMF and the World Bank in a more abstract framework, which is then again elaborated. We suspect that the following general hypothesis holds true for the mobilization capacity of frames: the more individual problem definitions that can be embedded in a generalized world view, the more plausible the problem definitions, and the higher the mobilization capacity of the frame.

Second, in addition to defining problems, diagnostic framing also includes the definition of causes (Ferree and Miller 1985, pp. 43–44; Snow and Benford 1992, p. 8). Both frames allow for labeling *causes and causal agents*. A congress and a visit of a country's president are "innocent" events in and of themselves. They only become problems if both guests can be labeled as agents causing the problems that have been defined. The causes for the multitude of problems dealt with in the imperialism ideology are located in the system, the world economic order itself. Behind the causes are causal agents in the form of concrete persons and institutions: the IMF, the World Bank, and the large corporations and banks in the First World. The cause of the problems defined in the hegemonic power ideology is the United States's claim to hegemony; the causes are personalized by Ronald Reagan. If both institutions, the IMF and World Bank and the president of the United States, can be successfully labeled as the causal agents for the problems that were highlighted, then the frame gives good reasons to protest against the congress or visit.[30] One can express the connection formulated through concrete examples in the following hypothesis: if the causes for the identified problem can also be defined in a frame and, at the same time, these causes can be related to concrete persons, then this increases the mobilization capacity of a frame.

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[30] Along with the causal agents, the protest's *addressees* and *objects of attack* (Tarrow 1989, pp. 101–4) are also labeled by the frames to a certain extent. The addressees of the protest were, on the one hand, the two figures labeled as causal agents for the problems defined, Reagan and the combination of the IMF and World Bank; on the other, however, the Berlin Senate, which had invited the international personalities to Berlin, also became an addressee for protest.

Prognostic Framing

Prognostic framing implies "a proposed solution to the diagnosed problem that specifies what needs to be done" (Snow and Benford 1988, p. 199). The solutions for the problems and the demands are defined in both frames. The solutions are, in principle, the reversal of the defined problems and causes. The wider range and more generalized design of the imperialism ideology made it possible to develop more extensive demands in this case. The argument is that none of the problems can be solved in the long run if the world economic order itself is not changed. The hegemonic power ideology is more specific in that regard. The demands here—disarmament, stopping the interventionist policies, and no support for South Africa's apartheid regime—refer to the three sets of problems that were highlighted. Though both frames offer proposals, albeit general and vague ones, for solving the defined problems, they fail to define the means and methods necessary to reach the ends. Assuming that the definition of the means for achieving the ends is an important element of successful mobilization, then we can observe a particular weakness of both frames in this respect. The frames identify and label problems, causes and causal agents, and potential solutions, but offer little help in finding solutions to the problems. We suggest the following hypothesis regarding the mobilization capacity of frames. The closer the frames come to giving solutions for the defined problems and ways to reach these solutions, the higher the mobilization capacity of the frames.

However, prognostic framing is apparently far less important to social movements and protest campaigns than diagnostic framing. In contrast to political parties, social movements and protest groups do not compete to occupy administrative positions in order to propose and implement solutions to problems. Therefore, protest groups, unlike parties, are not usually expected to offer solutions to the defined problems.[31]

Motivational Framing

Motivational framing means "a call to arms for engaging in ameliorative or corrective action" (Snow and Benford 1988, p. 199). Though consensus on problems, causes, and solutions is a precondition for mobilization, it does not by itself lead to mobilization. Motives for participation in specific actions must be created.[32] Both frames lack an explicit motivational framing. Implicitly, the motivation to participate in the respective demonstration is stimulated by the moralizing manner in which the problems are interpreted in the diagnostic frame. Terms such as exploitation, erosion of social security, unemployment, poverty, destruction, misery, and starvation are value laden. They inherently convey a call to struggle against them as evils. A motivational framing specifically designed for the demonstrations could not be found, however. Nonetheless, in general we would formulate the following hypothesis: the mobilizing capacity of a frame increases to the extent that it contains explicit or at least implicit motivating elements, such as appeals to generally recognized moral norms.

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[31] Raschke (1985, p. 386) assumes that social movements have a high capacity for raising problems but a low capacity for problem solving.

The mobilization capacity of a frame is not only determined by the factors within the three dimensions of diagnostic, prognostic, and motivational framing, but also by its completeness and, above all, by the way the three dimensions are combined. We assume that the better these three dimensions are integrated; that is, the higher the degree of interrelatedness of the frames' elements, the higher their mobilization capacity (Snow and Benford 1988, p. 199). As a matter of fact, both of the frames we analyzed present a comprehensive pattern for interpretation: diagnostic, prognostic, and motivational framing and their integration into a broader system of meaning.

We have analyzed the internal structure of both frames and formulated some hypotheses about the mobilization capacity of frames. The mobilizing capacity of frames, however, depends not only on their internal structures, but also on the leverage points they provide for the *linkage to external recipients*. We assume that the internal structures of the frames as described were favorable for mobilizing different groups. The mobilization potential of the appeal was limited, though, because both master frames build on or reproduce classic leftist theories. This is more obvious for imperialism ideology than for hegemonic power ideology.[33] Referring to the distinction between restricted and elaborated master frames that Snow and Benford (1992, p. 12) have introduced by drawing on the work of Basil Bernstein (1971), both master frames we have found are restricted insofar as their leverage points were confined only to a specific political environment. Because of the leftist slant of the frames, the mesomobilization potential could not be expanded beyond the leftist political spectrum. However, because of the broad scope of the problems included in the frames, a large proportion of this spectrum could be activated.

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[32] Klandermans (1988) asserts that the generation of motives for participation is applicable solely to the motivation of individuals. However, we believe that they are also important for motivating the participation of micromobilization groups.

[33] Moreover, the two interpretative frames can be combined with each other. The linkage of the hegemonic power ideology with the imperialism ideology was ideologically prepared, although it was not included in either frame.

C. Frame Bridging: The Linking of Master Frames and Group-Specific Frames

The structural analysis showed that the campaign against the IMF and World Bank congress and the one against the Reagan visit were successfully organized by mesomobilization actors who integrated a multitude of different political groups. The organizational linkage was complemented by ideological integration. The analysis of the master frames has demonstrated that in both cases a host of problems was covered, offering particular leverage points for single-issue groups. In the following section, we examine how the groups pick up the master frames' leverage points and link these with their group-specific frames. This process of frame bridging is central for mobilization processes because it forms the connection between the level of mesomobilization and that of micromobilization.[34] Consequently, we shift our attention from the analytical level of mesomobilization to that of micromobilization.

Most of the groups involved in our study focused on other problems than the IMF and Ronald Reagan's visit. What reasons did they use to make their participation plausible to themselves and others? In looking at the individual groups' leaflets from this perspective and comparing them with the central leaflets that called for participation in the large demonstrations and were signed by all of the groups, we can detect *bridging phrases* that established the connection between the imperialism and hegemonic power ideology used by all groups and the group-specific interpretative framework. Our data only allow us to reconstruct the frame bridging that occurred during the IMF campaign.[35]

We will do this by using five examples.[36] The passages we quote will show that frame bridging is done by the groups themselves and is not a construction we have created.

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[34] "Frame bridging is the principal route to alignment. We may wonder whether any other approach works at all at this level" (Klandermans 1992).

[35] We have too few group-specific leaflets from the anti-Reagan campaign. One reason for this lack could be that the relatively long period of time between the protest event and the data collection meant that we could no longer locate many existing leaflets. Another reason could be that there may have been less frame bridging carried out by the different groups. Here it is important to remember that the range of problems under the hegemonic power ideology was narrower than under the ideology of imperialism.

[36] In selecting leaflets from different groups, we first sorted them into various classes (peace groups, Third World groups, women's groups, etc.) and then made a random choice of one leaflet from each class of groups. In this manner, the text corpus was systematically reduced to a few leaflets. The quotations here were selected and translated by the authors, from whom the leaflets are available on request.

1. In their leaflet, the *peace groups* began with the arguments from the imperialism ideology and labeled the world economic order as unjust. The connecting formulation that brings together the ideology of imperialism and the group-specific frame is: "Peace and justice cannot be separated. Therefore, the peace movement has not only a moral obligation for intervention but, according to its own vital interest, must consider Third World problems as its own problems. Injustice leads to a global destabilization which finds its expression in wars." Here, the concept of injustice is the bridge establishing the connection between the peace frame and the imperialism ideology.
2. The *ecology groups* legitimated their participation in the campaign by focusing on one consequence of the activities of the IMF and World Bank. The World Bank and IMF finance large projects that lead to a destruction of the tropical rain forests—reason enough for these groups to feel themselves addressed and to participate. "The ecological consequences of the policy (of the IMF and World Bank) are alarming. Particularly the rapidly progressing destruction of the tropical rain forests, facilitated not least through large projects financed by the World Bank and the IMF, urgently demand a change in the previous foreign aid policy of the World Bank and the IMF." The emphasis on the ecological problems caused by the policies pursued by the IMF creates the link necessary to convince ecological groups and their adherents that they should participate in the campaign against the IMF and the World Bank.
3. The *women's groups* connected the imperialism ideology with a patriarchy frame. Women in both the First and Third World are affected by the capitalist world order. This connection is demonstrated in several points. Thus, the IMF's austerity policy leads to the impoverishment of the people in the Third World. "Women have to bear the primary burden of this situation: In the present system they have the main responsibility for their own immediate survival and that of their families, and have to compensate for deteriorating life conditions by performing additional work (both wage labor and unpaid reproductive work)." For many women, this means that they have to resort to prostitution in order to survive. The flip side of the impoverishment in the Third World is the wealth in the First World, which primarily benefits the men. Correspondingly, the men are also customers for "sex-tourism" in the Third World. With this interpretation of the problem, women's groups legitimated their own involvement as women's groups and, in addition, legitimated direct actions against several travel bureaus selling airline tickets to Thailand.
4. The *neighborhood groups* were also able to achieve a specific connection between the IMF campaign and their group-specific objectives. They anticipated that the local government would limit the freedom of citizens in Berlin in order to ensure a trouble-free congress: they expected traffic controls, a possible cordoning off of districts in the city, and interruptions in the subway. They were not willing to accept such limitations, especially not for a guest who—here was the starting point for the

imperialism ideology—was responsible for exploitation and misery in the Third World. "The Berlin Senate is assuming responsibility for the security of these ladies and gentlemen who are responsible for exploitation, the worldwide indebtedness crisis and hunger, terror and war. Kewenig's [the Minister for Interior Affairs at that time] proven security machinery will be unleashed on us so that they can make their arrangements without serious 'occupational accidents'. . . . We will have to pay for the security needs of the bankers with considerable restrictions."

5. The *unions* attempted to establish the bridge to the IMF issue by focusing on the impairment of worker interests. These interests are affected by the IMF policy in several ways: first, the austerity policy of the IMF leads to unemployment and low wages in the Third World countries and to the repression of the unions in these countries: "this causes the unemployment of thousands of people." Second, the policy of devaluing the currencies in the Third World weakens their import possibilities, which leads to production losses and increased unemployment in the First World: "lacking capacities for imports in developing countries . . . may cause a decrease of production in First World countries." Third, the indebtedness crisis is likely to induce a worldwide breakdown of the monetary system. This would also lead to considerable impoverishment in the industrialized countries, "thus causing social misery in broad strata of the industrialized countries and the emergence of political crises, as was the case in the world economy of the 1930s."

Each of the groups that participated in the mobilization could name reasons why they wanted to take part in the IMF campaign. They legitimated their involvement by semantically connecting group-specific interpretive frameworks with the IMF frame; they motivated their members and potential followers to participate in the campaign against the IMF and World Bank congress by using argumentative persuasion. The development of a homogeneous interpretative master frame which, at the same time, supplied connecting links for the integration of group-specific interpretative frameworks, was one of the preconditions for a broad integration of different groups within the leftist-alternative spectrum. The successful process of frame bridging by the micromobilization groups probably improved mobilization "at lower levels," namely that of individuals.

An additional favorable condition for the mobilization was that the frame bridging was not achieved for just any micromobilization groups, but for those groups and organizations that had already proved to be central for micromobilization processes. The choice of ecological problems, the specific discrimination against women, and the complex problems of weapons and peace as central topics meant that three semantic contexts were included that were issues of recently mobilized West German social movements whose concerns found a widespread public support (Pappi 1988). Picking unemployment and the reduction of social services as consequences of the imperialist world order and the enormous expenditures for armaments creates the reference point for labor's classic cleavage with capital. Both mobilization processes were thus successful in creating a connection between the specific issue and the previously defined issues of existing protest actors. If a new issue can be connected with a legitimated value complex and established interest groups and movements, then the cultural resonance of the issue and, consequently, the mobilization power of the frame, will increase.

V. CONCLUSIONS AND PERSPECTIVES

Various authors have emphasized the relevance of mediating structures for mobilization processes in protest campaigns. As a specification to the prevailing literature on mediating structures we have introduced the concept of mesomobilization and mesomobilization actors. We demonstrated its empirical relevance by investigating two protest campaigns, one directed against the visit of U.S. President Ronald Reagan and one directed against the conference of the World Bank and the IMF in Berlin. In both cases, the direct triggering occurrence was a nonobtrusive event. A state visit and a congress are actually "harmless" and short-lived events. They became highly controversial political issues only because they were symbolically loaded and consequently seen as problematic by many groups and individuals. In each case, more than 130 different groups were activated and ultimately supported mass demonstrations that attracted tens of thousands of people.

Mesomobilization was a crucial factor for the success of both campaigns, both in terms of activated groups and of mobilized individuals. First, mesomobilization actors succeeded in establishing an effective structure for organizationally integrating the wide array of micromobilization groups. Second, mesomobilization actors developed interpretative master frames with diagnostic, prognostic, and motivational elements that also allowed for linking group-specific concerns to the master frames. With respect to both dimensions we have developed some hypotheses as to which structural and cultural factors could account for a successful mesomobilization. To this extent we did not carry out an empirical test of an elaborated theoretical model, but have aimed at using empirical material to expand on theory and generate hypotheses.

In terms of mobilization, the outcome of the two campaigns was roughly the same. However, the amount of resources producing these protests varied significantly. More time and resources were invested in the organization of the anti-IMF campaign than in the anti-Reagan campaign. For example, we registered 475 preparatory meetings and actions for the anti-IMF campaign, whereas "only" 81 could be found for the anti-Reagan campaign. Moreover, in the first campaign, the mesomobilization process was based on a more differentiated and professional structure. Finally, the framing of the IMF campaign was also more extensive in a number of respects. We speculate that the similar outcome of this differential investment in both campaigns has to do with the existence of more favorable conditions in the case of the anti-Reagan campaign. First, there had been similar large-scale campaigns against Reagan in the past. The previously established channels of communication were, therefore, easy to revitalize. Second, the perceived "evil" could be personalized with respect to one well-known representative from a country which, in the view of many activist groups, was the main risk for peace. A package of interpretations had already been developed (Gamson 1988). The IMF conference lacked these advantages, so more organizational and framing efforts were required to politicize it.

What conjectures can be drawn from these two case analyses for the future of mobilization processes? Several authors of political sociology have recently asserted that, at least in West Germany, the mobilization potential for unconventional political action at the individual level has increased during the last two decades. Fuchs (1990) concludes that the mobilization potential on the individual level, that is, the individual willingness to take part in unconventional protest activities, increased rapidly up until the mid-seventies, and has since remained at a relatively high level with only a slight increase.

Generalizing from the results of the two case studies we can speculate that, at least in Berlin, this latent protest potential will also transform itself into manifest protest participation in the future. A leftist-alternative infrastructure consisting of a multitude of micromobilization groups has emerged; they constitute a large mesomobilization potential. At the same time, an interconnected structure of mesomobilization actors has developed and become institutionalized. These actors have served as catalysts, translating the mesomobilization potential into actual participation on various occasions. Finally, a relatively uniform interpretative frame forms the cultural prerequisite for interpreting different but selected occasions and framing them as issues worth protesting.

Since the individual willingness to take part in political actions has increased, the intermediate structure for protest activities—micromobilization groups and mesomobilization actors—has been established, and a set of elaborated leftist master frames is available, we can expect that manifest protest participation will probably continue to occur in the future.[37] In the light of this, the large protest campaigns against the engagement of the United States and its allies in the Gulf War are not surprising. In fact, many of the groups that initiated the protest events investigated in this study also played a crucial role in the anti-war protests in Berlin during January and February 1991.

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[37] It has to be emphasized, however, that the recent developments in Germany and Europe may undermine these favorable protest conditions. Our analysis has demon-

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strated that the ideological superstructure of intermediate mobilization contexts relies heavily on "classic" leftist world views. The rapid decay of socialist societies in Eastern Europe may also cause these leftist ideologies to lose their credibility. In the terms of Snow and Benford (1992, p. 29), the prevailing "cultural climate" has changed, which has a negative impact on the mobilizing capacity of the leftist master frames.

APPENDIX A

We say no to Reagan's politics

President Reagan is coming to Berlin (West) for its 750th anniversary. He represents interests in the USA Which will stop at nothing in their efforts to make the USA the undisputed world and military power. Billions of dollars are being spent for continually new arms programs. New strategies for waging war are constantly being developed in the USA and in the NATO. Finally, the Reagan Administration is threatening all of humanity with its SDI plans.

Reagan is trying to bury the Soviet Union in the arms race, despite the fact that Gorbachev has made far-reaching disarmement proposals.

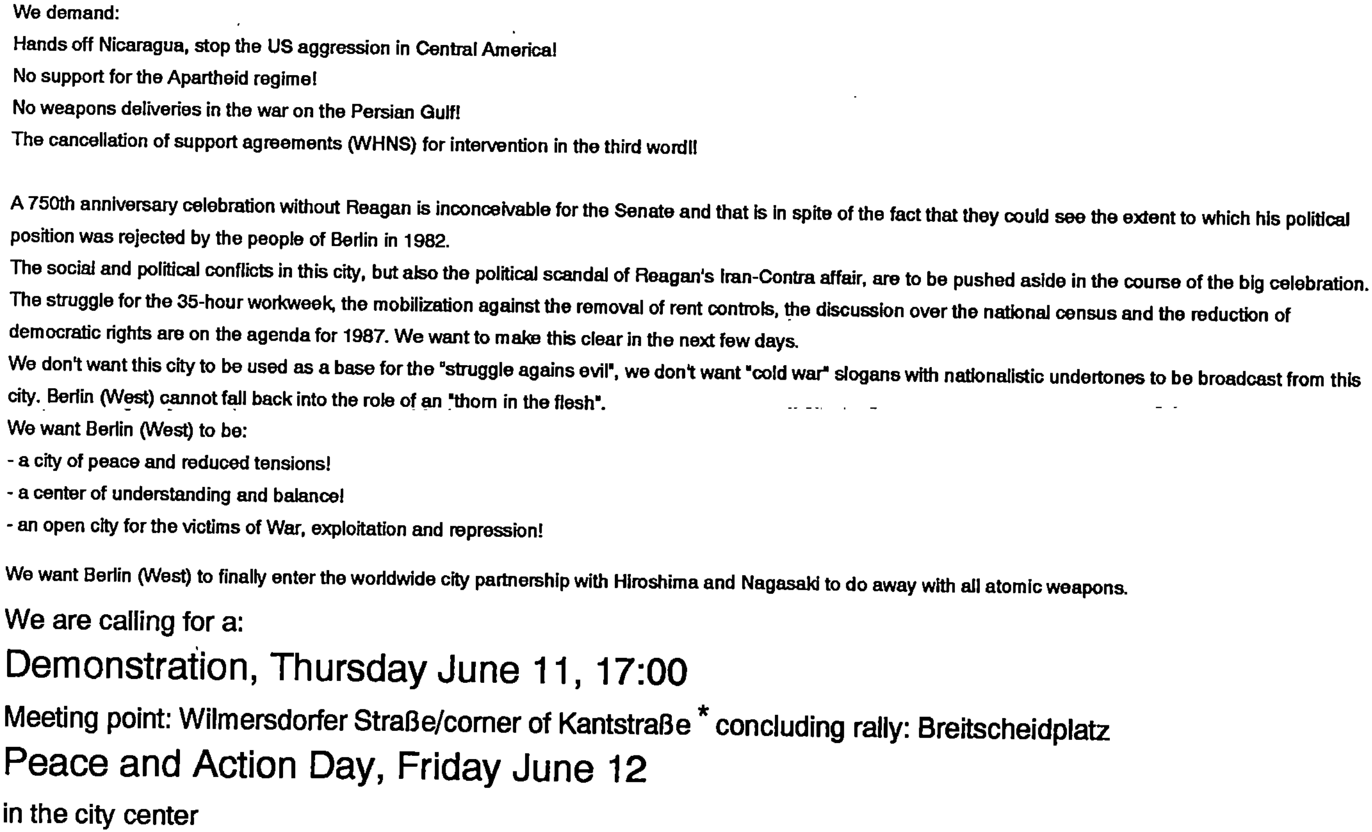
Kohl and Reagan have shown in the past that they want to jointly continue the disastrous "crusade against the East". We demand that the federal government takes seriously the demand that a war should never again be started from German territory and finally introduce concrete steps toward disarmament.

Arms do not only kill in war. The worldwide consequences stemming from the lunacy of the arms race can no longer be ignored. Poverty, reduction of social services, mass unemployment and impoverishment characterize the social climate. Women, more than half of humanity, are especially affected. Complete equality for women - for all people - cannot be achieved under these conditions. We say no to this type of politics and its consequences.

We demand:
- Disarmament in West and East!
- An immediate, sweeping atomic test ban treaty!
- The Immediate removal of all medium range missiles in Europe!
- No militarization of outer space!

The Reagan Administration declared the entire third world to be its sphere of interest and plays "world policeman". For example: it bombed Libya using the bombing of the Berlin discothèque "La Belle" as an excuse. It shot up Beirut, got rid of the government In Grenada and mined the harbors in Nicaragua, openly supported the Contras, and supported the racist white government in South Africa for strategic reasons.

The countries of the "third world" are exploited and forced into submission with the help of the International Monetary Fund (IMF) and rapid deployment forces. This forces millions of people to leave their homelands. We say no to this policy!



We demand:

Hands off Nicaragua, stop the US aggression in Central America!

No support for the Apartheid regime!

No weapons deliveries in the war on the Persian Gulf!

The cancellation of support agreements (WHNS) for intervention in the third world!

A 750th anniversary celebration without Reagan is inconceivable for the Senate and that is in spite of the fact that they could see the extent to which his political position was rejected by the people of Berlin in 1982.

The social and political conflicts in this city, but also the political scandal of Reagan's Iran-Contra affair, are to be pushed aside in the course of the big celebration. The struggle for the 35-hour workweek, the mobilization against the removal of rent controls, the discussion over the national census and the reduction of democratic rights are on the agenda for 1987. We want to make this clear in the next few days.

We don't want this city to be used as a base for the "struggle agains evil", we don't want "cold war" slogans with nationalistic undertones to be broadcast from this city. Berlin (West) cannot fall back into the role of an "thorn in the flesh".

We want Berlin (West) to be:

- a city of peace and reduced tensions!
- a center of understanding and balance!
- an open city for the victims of War, exploitation and repression!

We want Berlin (West) to finally enter the worldwide city partnership with Hiroshima and Nagasaki to do away with all atomic weapons.

We are calling for a:

Demonstration, Thursday June 11, 17:00

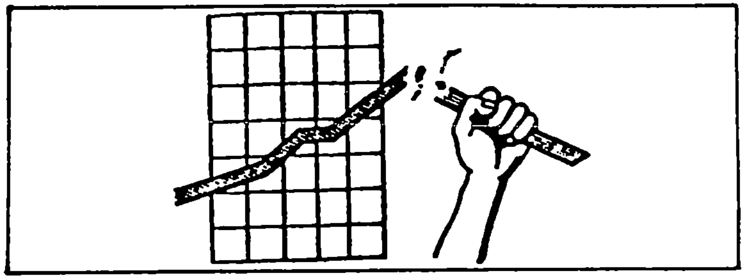
Meeting point: Wilmersdorfer Straße/corner of Kantstraße * concluding rally: Breitscheidplatz

Peace and Action Day, Friday June 12

in the city center

FIG. A1.—Leaflet protesting Reagan's visit (translation by Jeffrey Butler)

For the resolution of the debt crisis - for a just world economic order



In September 1988, the International Monetary Fund (IMF) and the World Bank will hold their yearly meeting in Berlin(West). These two top institutions of the international financial system have a key responsibility in connection with the indebtedness crisis of the "third world" which has been worsening since 1982. Through their rigorous conditions and their so-called structural adjustment programs, the leading Western industrial countries are decisively responsible for the economic exploitation of the "third world", for the impoverishment of the people living there and for the brutal destruction of their natural prerequisites for living.

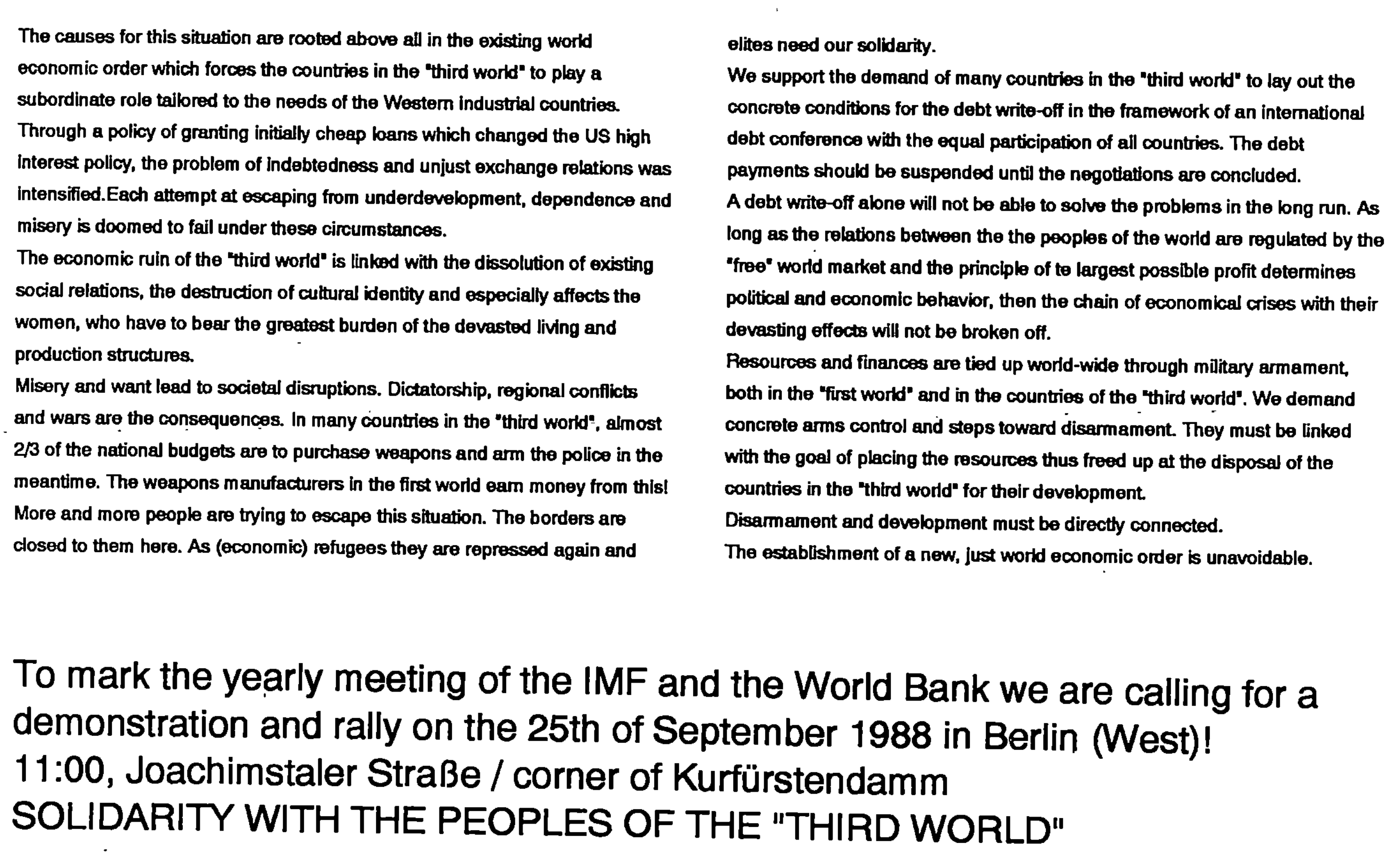
The present foreign debt of the "third world" amounts to the gigantic, prohibitive sum of 1,2 trillion US dollars. The interest and principal payment of these countries far exceed the influx of new capital in the meantime. The debtor countries must expend a larger and larger share of their export income to pay them back, less and less remains for meeting basic internal needs and making domestic investments.

usually deported to face exploitation, torture and death. The circle of impoverishment, underdevelopment and militarization is closed again.

Even the people here do not remain unnscathed: unemployment, new poverty and the cutback of social services are only other expressions of the same crisis which is driving the "third world" into riun. We must find the way out.

The disastrous development has to be stopped. The prevailing debt management by the IMF, the World Bank, commercial banks and Western governments with refinancing, new loans and case by case treatment does not provide a solution; on the contrary, it strengthens the dependence and intensifies the crisis.

There is no way out without writing off the debts. The burdens must be borne by those who are responsible for the situation. This requires, at the same time, a change in international relations and the balance of power. This is why the political and social movements who have to push through their interests against the power cartel of corporations, banks, the IMF, the World Bank and



The causes for this situation are rooted above all in the existing world economic order which forces the countries in the "third world" to play a subordinate role tailored to the needs of the Western industrial countries.

Through a policy of granting initially cheap loans which changed the US high interest policy, the problem of indebtedness and unjust exchange relations was intensified.Each attempt at escaping from underdevelopment, dependence and misery is doomed to fail under these circumstances.

The economic ruin of the "third world" is linked with the dissolution of existing social relations, the destruction of cultural identity and especially affects the women, who have to bear the greatest burden of the devasted living and production structures.

Misery and want lead to societal disruptions. Dictatorship, regional conflicts and wars are the consequences. In many countries in the "third world", almost 2/3 of the national budgets are to purchase weapons and arm the police in the meantime. The weapons manufacturers in the first world earn money from this!

More and more people are trying to escape this situation. The borders are closed to them here. As (economic) refugees they are repressed again and elites need our solidarity.

We support the demand of many countries in the "third world" to lay out the concrete conditions for the debt write-off in the framework of an international debt conference with the equal participation of all countries. The debt payments should be suspended until the negotiations are concluded.

A debt write-off alone will not be able to solve the problems in the long run. As long as the relations between the the peoples of the world are regulated by the "free" world market and the principle of te largest possible profit determines political and economic behavior, then the chain of economical crises with their devasting effects will not be broken off.

Resources and finances are tied up world-wide through military armament, both in the "first world" and in the countries of the "third world". We demand concrete arms control and steps toward disarmament. They must be linked with the goal of placing the resources thus freed up at the disposal of the countries in the "third world" for their development.

Disarmament and development must be directly connected.

The establishment of a new, just world economic order is unavoidable.

To mark the yearly meeting of the IMF and the World Bank we are calling for a demonstration and rally on the 25th of September 1988 in Berlin (West)!

11:00, Joachimstaler Straße / corner of Kurfürstendamm

SOLIDARITY WITH THE PEOPLES OF THE "THIRD WORLD"

FIG. B1.—Leaflet protesting the 1988 yearly meeting of the IMF and the World Bank (translation by Jeffrey Butler)

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Mean Streets: The Theoretical Significance of Situational Delinquency among Homeless Youths[1]

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John Hagan
University of Toronto

Contemporary sociological theories of delinquency emphasize background and developmental factors while neglecting adverse situational conditions. This study uses data from youths on the street and in school to test an integration of strain and control theories that spans background and situational factors. After background and street exposure variables are controlled for and after school and street samples are combined, there is consistent evidence of the effects of adverse situational conditions: hunger causes theft of food, problems of hunger and shelter lead to serious theft, and problems of unemployment and shelter produce prostitution. These findings broaden and increase theoretical understandings of street life and crime.

The idea that adverse situations cause crime and delinquency was once a fundamental theoretical principle of sociological criminology. Early European criminologists including Russell (1847), Fletcher (1849), and Bonger (1916) recognized the significance of adverse situations and attributed crime and delinquency to sudden fluctuations in food prices and the availability of jobs. During the Great Depression in the United States, Sellin (1937) similarly adopted a situational perspective to explain changes in crime rates. He emphasized the sudden onset of unemployment and discounted the longer term influence of chronic poverty. How-

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ever, this situational emphasis did not endure or prosper in sociological criminology; instead, theoretical attention to immediate problems of day-to-day survival diminished, as if the relative prosperity of post-Depression society eliminated adverse situations as causes of crime and delinquency. Gradually, sociological theories of criminogenesis moved from the foreground to the background of the criminal experience.

FROM PAST TO PRESENT IN THE CAUSATION OF CRIME

Three contemporary tendencies reflect an underestimation of situational adversity as a cause of crime; two of these are theoretical, the other methodological.[2] The first tendency is to discount situational effects and focus instead on background and development. The second incorporates situational effects but emphasizes the inducements, opportunities, or attractions of crime, to the neglect of more pressing circumstances. The third involves studying self-reports of delinquency by adolescents living at home and attending school; that is, adolescents who live in institutionally protected situations.

The first inclination of modern sociological theories of crime is to focus on background and developmental factors while disregarding situational circumstances: strain theory locates the motivation for crime in cumulative frustrations arising from structural inequality; control theory focuses on the failure of family and school experiences to establish social bonds; differential association theory emphasizes prior exposure to deviant beliefs; and labeling theory concentrates on successive stages of stigmatization. These theories assume that crime and delinquency are predetermined by background experiences and developments that cumulate over time. This is one reason that their focus is on the formative years of adolescence.

In a variation on this first tendency, some theorists give limited attention to situational variables. The vocabulary varies, but consideration is sometimes given to pressures, inducements, or motivations for crime, variously described as situational, mechanistic, dynamic, endogenous, or contemporaneous. However, these factors are not given extensive consideration and most usually are dismissed. For example, in an early and influential statement, Sutherland and Cressey (1978) acknowledge and then minimize the significance of situational effects by asserting that although situations may contribute to crime, the primary definitions of these situations are determined through differential exposure to competing norms. Thus, they argue that "while a person could define a situation in such a manner that criminal behavior would be the inevitable result, past experiences would, for the most part, determine the way in which he or she defined the situation" (Sutherland and Cressy, 1978, p. 80).

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[2] Aggregate-level research links crime with chronic destitution, poverty, and relative deprivation (see Horowitz 1984). We do not question the importance of this research; however, our concern is with the more immediate effects of situational destitution on individuals.

Briar and Piliavin (1965), and more recently Gottfredson and Hirschi (1990), arrive at a similar conclusion, although these authors argue it is the lack of commitment to conformity or self-control that determines whether situational factors will lead to crime. So a predominant view, even among those who acknowledge the presence of situational factors, is to assign them a secondary role in the causation of crime and delinquency.

Alternatively, a few writers argue for the importance of situational effects. Although these writers give greater attention to situational variables, their work dwells on opportunities or attractions to crime. For example, Gibbons (1971) argues that there are external motivations for crimes that result from opportunities that accompany "criminogenic conditions." According to Gibbons (1971, p. 271), "in many cases, criminality may be a response to nothing more temporal than provocations and attractions bound up in the immediate circumstances."

Katz (1988) echoes this view in an evocative articulation of the situational approach. According to Katz, traditional theories neglect the "seductions of crime" that result from attempts to maximize pleasure (e.g., thrills) or satisfaction (e.g., from overcoming challenges to one's authority or moral position) in intensely felt situations. Katz (1988, p. 4) focuses on situational events that immediately precede crimes and propel offenders, since "something causally essential happens in the very moment in which crime is committed." Like Gibbons's, Katz's emphasis is restricted to particular situational factors: the opportunities or the sensual attractiveness of situations that seduce people to offend. This argument is more persuasive in situations that present opportunities or temptations for crime than in situations that pose problems of day-to-day survival; focusing on the former instead of the latter obscures much of the force of the situational argument.

The neglect of more pressing circumstances is institutionalized by the third tendency noted above: a shift to self-report surveys of school students for tests of crime and delinquency theories. Data collection through home and school-based self-report surveys (from Nye and Short [1957] through Hindelang, Hirschi, and Weis [1981] to Nagin and Paternoster [1991]) was an important innovation because it freed researchers from reliance on official records and allowed the systematic and extensive measurement of background and developmental variables. However, as Sullivan (1989, p. 6) notes, "there is doubt about the extent to which such surveys ever reached the population of serious delinquents." Moreover, exclusive reliance on school and home samples has diverted attention from less protected settings, like the street, that once formed an important focal point for delinquency research (from Thrasher [1927] to Short and Strodtbeck [1965]).

It is perhaps not surprising, given the tendencies we have outlined, that circumstances that pose problems of survival and well-being have come to occupy a secondary position, at best, in sociological theory and research on crime.

BACK TO THE STREETS

Our purpose is to reexamine the role of adverse situations in crime causation. We accept the place of background and developmental variables in our reexamination and are guided by an integration of background and developmental variables from strain and control theories. However, we also extend these theories by incorporating situational variables that derive from our attention to the conditions of homeless youths on the streets. Our premise is that reconceptualizing strain and control to include the adverse circumstances of homeless street youths, draws more attention to the role of these situational factors than is currently acknowledged in theories of delinquency causation.

Our integration links Agnew's (1985) strain theory with Hirschi's control theory (see Hirschi 1969). Classical Mertonian strain theory asserts that delinquency is a response to encounters with structural impediments to the attainment of socially determined success goals. Agnew (1985) recasts this conceptualization by suggesting that delinquency may also be a response to living in painful or punishing environments. This perspective is particularly relevant in the present context because homeless youths may have fled from one set of adverse situations (i.e., abuse in the home and conflict at school) only to find themselves in another (i.e., the difficult circumstances of life on the street).

Meanwhile, youths who encounter strain in the home and on the street may also experience little social control in these settings. In more conventional terms, they may experience neglect at home and have few attachments or involvements in conventional activities on the street; thus, they may have weak social bonds. Our integration of strain and control theory reflects our assumption that both the presence of strain and the absence of control can push youths to delinquency as well as free them for it. However, to the extent that either or both theoretical sources of this integration operate, it is important to establish the relative contribution of each and that the influence of one set of factors is not a spurious product of the other.

ADVERSE SITUATIONS

Many homeless adults live in conditions of great difficulty (see Rossi 1989; Momeni 1989). Many adolescents who leave and stay away from home encounter conditions similar to adults, including unemployment, poverty, hunger, and lack of shelter (Kufeldt and Nimmo 1987; McCarthy 1990; Stephenson 1989). So the conditions confronted by youths on the streets can clearly be adverse in the theoretical sense that Agnew suggests.

Although the great majority of studies on the homeless and on runaways have not collected data on crime (see Brennan, Huizinga, and Elliott [1978] and Rossi [1989] for reviews), there is also some evidence that homelessness is conducive to crime. For example, police arrest records from Austin, Texas, were used in a study by Snow, Baker, and Anderson (1989), who found that the arrest rate for homeless males (aged 18–68) was significantly greater than for males who were not homeless. They reported an arrest rate for property offenses of 179 per 100,000 for homeless males, compared with 130 per 100,000 for men who have homes (also see Fischer 1988; Gelberg, Linn, and Leake 1988).

Several studies on runaways also point to criminogenic aspects of street life. In a study using data on youths from Colorado and from a national sample of adolescents, Brennan et al. (1978) found that 33% of runaways admitted to stealing after leaving home (15% indicated that they stole objects worth $50 or more) and 20% reported selling drugs. Brennan et al. (1978) also note that these activities were positively associated with the length of time away from home and with the number of previous departures. In a more recent investigation of 489 runaway youths in Edmonton (Canada), Kufeldt and Nimmo (1987) indicate that 71% of runaways reported being encouraged by others to participate in crime while on the street; moreover, 49% admitted to using unspecified illegal means to satisfy their needs. Several other studies also suggest that crime is common among runaways, particularly those with extensive street careers; unfortunately, these studies provide insufficient data to document this possibility fully (see Janus et al. 1987; Palenski 1984; Shellow et al. 1967). In the sample of homeless youths analyzed below, we find that 46% made drug sales, 49% stole goods valued up to $50, and 27% broke into homes or businesses. The respective figures reported in Hindelang et al.'s (1981) research, which is stratified to overrepresent adolescents with police contacts, are 32%, 13%, and 13%. Larger differences characterize more representative studies based on school or home samples (e.g., see Hirschi 1969; Elliott and Ageton 1980; Paternoster and Triplett 1988).

Although less commonly reported, comparisons of the frequency of offending also suggest that homeless youths are disproportionately involved in crime. For example, in a study of 1,544 eleventh-grade students, Paternoster and Triplett (1988) report that, among those committing at least one offense, the median frequency of petty theft is 3 and for smoking marijuana it is 10. In our sample of homeless youths the median frequency of more *serious theft* (over $50) while on the street is 8, and for *selling hard drugs,* it is 10. So, even though homeless youths constitute a relatively small proportion of all adolescents, they are involved in a substantial and disproportionate share of crime (also see Hagan and McCarthy, in press).[3] However, the causal relationship between crime and homelessness has not been adequately specified. The importance of understanding the relationship between street life and crime is heightened by the growing numbers of youths who are leaving and staying away from home. According to recent estimates, there are 750,000–1,500,000 homeless youths in the United States (Shane 1989).

DATA, VARIABLES, AND METHODS

We propose that adverse circumstances in themselves can compel people to commit crimes. However, a meaningful test of this proposition requires a sample of individuals exposed to such circumstances, indicators of these difficulties, and measures of other variables that may account for a possibly spurious association between indicators of adversity and delinquency among street youths.

Data

In the main part of our analysis, we study Canadian youths surveyed in the city of Toronto during 1987 and 1988.. These adolescents were not living with their families (nor were they from homeless families), nor did they have a permanent or fixed address; in other words, they were "living on the street" or "homeless."

Note that we do not refer to these street youths as "runaways." This term is of questionable applicability here. It implies that leaving home is inappropriate and that a return is both possible and desirable. Yet previous research reveals that many youths who leave home are forced out by parents (i.e., they are "throwaways") or are escaping from abusive environments; not surprisingly, many are unwilling to return home (Adams, Gullotta, and Clancy 1985; Janus et al. 1987; Stephenson 1989). Furthermore the term runaway is commonly used as a legal classification.

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[3] It is now common in criminology to note that a small proportion of all offenders commit a large proportion of all offenses, especially the more serious offenses (e.g., Wolfgang, Figlio, and Sellin 1972).

As noted by Sellin, Sutherland, and others, it is often misleading to use a legal classification for sociological research, especially when the classification has no legal application. In Great Britain (see Liddiard and Hutson 1991) and Canada's province of Ontario, adolescents can legally leave home at age 16; thus, being a runaway is not a criminal or delinquent offense for these youths.

We used a two-pronged procedure to collect anonymous self-report data from adolescents at social service agencies and in nonagency settings. Toronto offers a unique opportunity for studying "street" or homeless youths because it has a number of social service agencies that provide free services for homeless people; moreover, many of these services are reserved explicitly for adolescents. Although these agencies provide necessities of life (e.g., shelter, food, clothing, and companionship), some youths do not use them (i.e., agency use is *voluntary*); like their adult counterparts (Rossi 1989), some youths avoid social service agencies because they find agencies have too many rules and limit their freedom of movement and activity.

To include a wide cross-section of clients, we surveyed youths in nine social agencies: a short-term hostel (maximum stay, five days) restricted to adolescents under the age of 21; a "drop-in" agency that provided a place for homeless youths to socialize; a long-term shelter (maximum stay, three months) for people under 21; a drop-in agency for adolescent prostitutes; three gender-specific shelters; one hostel for youths 16 and under; and one shelter without client restrictions.

The nonagency locations included inner-city parks, street corners, and places for sleeping. In these places, we distinguished homeless youths through observation and conversation. We approached only adolescents who were panhandling or preparing to sleep in a public place. We subsequently screened these youths on their knowledge of general "street topics" (e.g., could they recommend a place to sleep or panhandle) and to determine their ages (the study was limited to those aged 19 and under).

We used a weekly schedule to survey youths in both settings. However, every visit did not provide respondents. Instead, a considerable amount of time was spent "hanging out" in agencies, parks, and on street corners talking with workers and street youths. Interested adolescents completed a consent form and an anonymous 30-minute survey, for which they received $10 in restaurant coupons. The instrument required about a grade 7 reading level (Laubach and Koschnick 1977; Lorge 1959) and 32 respondents needed reading assistance. Over a one-year period, we approached 475 adolescents; of these 390 completed usable surveys (an 82% response rate).

Demographic data reveal that 67% of our respondents were male and 33% female; at the time of the survey, 12% were under 16 years old (the youngest was 13), 35% were 19, and the remaining 63% were almost evenly distributed across ages 16, 17, and 18. Of our respondents, 62% were staying in a hostel, 23% were living on the street, 12% were temporarily residing with friends or relatives, and 3% were living elsewhere (e.g., in a hotel). While we cannot be certain that our sample of street youths is representative, comparisons with accounts of other Canadian street youths (Janus et al. 1987; Kufeldt and Nimmo 1987; Stephenson 1989) and demographic data from social service agencies indicate that our sample does not differ significantly from the population of homeless youths (see McCarthy 1990).

In a second part of our analysis we introduce data from a supplementary sample of adolescents living at home and attending school. We randomly selected one school from each of three areas in Toronto and distributed survey invitations and parental permission forms to all students in randomly chosen ninth through twelfth-grade classes. A 30-minute instrument designed to parallel that used with the street youths was administered in classrooms and cafeterias, with a maximum of 40 students at a sitting. We offered popular music tapes as inducements and obtained 562 usable questionnaires with a response rate of 68%. This level of response falls within the range of 60%–75% commonly reported for comparable school-based surveys (e.g., Hindelang et al. 1981; Elliott and Ageton 1980).

Dependent Variables

Our primary interest is in explaining street youths' involvement in several kinds of delinquency. Our first dependent variable is a measure that asks about stealing food since leaving home (variables are summarized in table 1). Our second measure, serious theft, includes responses to seven items asking how often the respondent did the following since leaving home: (1) took things worth over $50, (2) took things from a car (tape deck, etc.), (3) took things from a store worth less than $50, (4) used a bank card or credit card without the owner's permission, (5) took things from a store worth more than $50, (6) took a stranger's automobile, and (7) broke into and took things from a house or building. Our final dependent variable, prostitution, is based on the question, "How often have you had sex for money since leaving home?"

We use these particular measures of street crime for two reasons.[4]

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[4] We also examined effects of measures of situational destitution and desperation on drug selling and violence. The results of these analyses are comparable to those reported for theft of food. In addition, we estimated models of theft at home for the samples of homeless and at-home youth. The results of these models are similar. All results are available upon request.

First, previous research on homeless adults (Rossi 1989; Snow et al. 1989) and youths (Janus et al. 1987; Kufeldt and Nimmo 1987) points to the disproportionate level of more serious crime on the street; although theft of food might still be considered a minor form of delinquency, prostitution and serious theft are not. The potential economic returns combined with the costs of detection clearly separate serious theft and prostitution from theft of food and other status offenses or crimes of mischief commonly used to measure delinquencies among high school youths. These distinctions are made clear in the public rankings of these activities in crime-seriousness scales (e.g., see Rossi et al. 1974) and in conversations with street and school youths. Second, we include the theft and prostitution measures because they are likely to reveal gender-specific patterns, with situational concerns and difficulties more likely to cause male involvement in theft and female participation in prostitution. That is, strains of the street are most likely to exert their effects along previously observed lines of gender specialization in crime (see, e.g., Steffensmeier 1980; Harris 1977).

Independent Variables

We use three kinds of items to tap situational difficulties on the street. The first item inquires about present and past unemployment since leaving home. We use this information to categorize respondents as continuously employed since leaving home, employed since leaving home but presently unemployed, or unemployed since leaving home. Our second measure is of hunger and asks how frequently the respondent has spent entire days without food since leaving home. Our final indicator of situational problems, shelter, uses information on the living conditions the respondent has experienced since leaving home. Our coding of shelter uses four categories that are ordered in increasing levels of risk and decreasing levels of security: living exclusively with friends or relatives (coded "1"), staying in public hostels and shelters (coded "2"), living on the street as well as in hostels (coded "3"), and staying on the street, in hostels, and hotels (coded "4"). The kinds of private hotels street youths use may be especially hazardous in exposing them to sites of ongoing criminal activities (e.g., drugs and prostitution).

In addition to measures of adversity, we also include information on strain and control at home. Our first set of background variables measure family class position and are indicators of structurally induced strain transmitted through the family. We use a modified version of Wright's (see 1978, 1985) class categories. This conceptualization uses information on parental employment, supervisory responsibilities, and ownership to divide families into the following classes (see table 1): an unemployed

TABLE 1

VARIABLE NAMES, DESCRIPTIONS, AND DESCRIPTIVE STATISTICS FOR HOMELESS AND AT-HOME SAMPLES

VARIABLE	DESCRIPTION	HOMELESS		AT-HOME	
		X	SE	*X*	SE
Age	Coded as years of age	17.597	1.457	16.632***	1.326
Gender	Coded "0" for females, "1" for males	.667	.472	.458***	.499
Family class position:					
Surplus workers	Coded "1" for unemployed head of household	.074	.263	.021***	.145
Workers	Coded "1" for head of household who does not employ or supervise others	.262	.440	.291	.455
Supervisors	Coded "1" for head of household who supervises but does employ others	.482	.500	.510	.500
Petite bourgeoisie	Coded "1" for self-employed head of household who does not employ or supervise others	.028	.166	.030	.171
Employers	Coded "1" for head of household who employs and supervises others	.154	.361	.147	.355
Family structure:					
Intact family	Coded "1" for presence of two biological parents	.313	.464	.718***	.451
Family control and strain:					
Maternal instrumental control	A two-item scale (.524, .622): "Did your mother know your whereabouts/who you are/were with when you were away from home?" (0 = never; 1 = sometimes; 2 = usually; 3 = always)	2.410	1.535	3.813***	1.579
Maternal relational control	A two-item scale (.380, .453): "Did you talk with your mother about your thoughts and feelings?" (0 = never; 1 = sometimes; 2 = usually; 3 = always) and "Would you like to be the kind of person your mother is?" (0 = not at all; 1 = in a few ways; 2 = in most ways; 3 = in every way)	1.585	1.428	2.147***	1.456

Paternal instrumental control	As above, for fathers (.586, .688)	1.946	1.497	3.067***	1.685
Paternal relational control	As above, for fathers (.403, .424)	1.379	1.412	1.671***	1.371
Coercive control	A two-item scale (.676, .488): “Did your parents or guardians ever use physical forms of discipline?” and “Have you ever been intentionally struck so hard by a parent or guardian that it caused a bruise or bleeding?” (0 = never; 1 = once or twice; 2 = sometimes; 3 = often; 4 = always)	3.221	2.306	1.181***	1.561
Sexual abuse	Coded “1” for abused	.063	.243	.007***	.084
School involvement, commitment, and strain:					
Homework	“How often did you do homework, projects, etc. after school?” (0 = never; 1 = rarely; 2 = sometimes; 3 = often; 4 = always)	1.703	1.329	2.490***	1.129
School goals	“How much schooling would you like to get eventually?” (0 = no more than present; 1 = more of high school; 2 = high school graduation; 3 = job apprenticeship or vocational school; 4 = college or university)	2.790	1.420	3.654***	.798
Conflict with teachers	“How often did you have trouble with teachers?” (0 = never; 1 = rarely; 2 = sometimes; 3 = often; 4 = always)	2.097	1.267	1.167***	.942
Criminal support at home:					
Home friends arrested	“When you lived at home how many of your friends had ever been arrested?” (0 = none; 1 = one; 2 = two–five; 3 = six–ten; 4 = more than 10)	2.138	1.540	.709***	1.074
Crime at home:					
Minor theft scale	A two-item scale (.503, .470): “While living at home how many times did you take things worth less than $5 that didn’t belong to you? things worth between $5 and $50 that didn’t belong to you?” (0 = never; 1 = one; 2 = two; 3 = three; 4 = four; 5 = five or more for both questions)	2.362	2.309	1.815***	2.009

TABLE 1 (*Continued*)

Variable	Description	Homeless		At-Home	
		X	SE	X	SE
Serious theft scale	A seven-item scale (.778, .534): “While living at home how many times did you take [item] without the owner’s permission?” Items include (1) things from a car (tape deck, etc.); (2) things from a store worth less than $50; (3) things from a store worth more than $50; (4) things worth over $50 that didn’t belong to you; (5) things from a house or building that you broke into; (6) a stranger’s car; and (7) a bank or credit card. (0 = never; 1 = one; 2 = two; 3 = three; 4 = four; 5 = five or more)	2.582	2.304	1.174***	1.796
Prostitution	“While living at home how many times did you have sex for money?”	.077	.617	.028	.303
Situational adversity					
Street unemployment	0 = presently employed; 1 = previously employed and currently unemployed; 2 = never employed	1.038	.753		
Hunger	“Since leaving home have you gone a whole day without eating?” (0 = no; 1 = once or twice; 2 = a number of times; 3 = most of the time)	1.405	.951		

Shelter	1 = with friends or relatives; 2 = exclusively in public hostels and shelters; 3 = on the street (abandoned buildings and cars, parks, etc. as well as hostels); 4 = on the street, public hostels and private hotels	2.640	.947
Other street variables:			
Number of times left home	"Including this time, how many times have you left home?"	7.608	15.77
Length of time on the street	"How long has it been since you last lived at home with other members of your family?" (in months to a maximum of 16)	10.323	5.711
Criminal support on the street:			
Street friends arrested	"How many of the friends you've met since leaving home have ever been arrested?" (0 = none; 1 = one; 2 = two through five; 3 = six through 10; 4 = more than 10)	2.241	1.546
Street crime:			
Theft of food	"Have you had to steal food since leaving home?" (0 = never; 1 = once or twice; 2 = a number of times; 3 = most of the time)	.726	.895
Serious theft	A seven-item scale (.819) same as theft at home but limited to activities since leaving home (0 = never; 1 = one; 2 = two; 3 = three; 4 = four; 5 = five or more)	3.000	2.269
Prostitution	"Since leaving home how many times have you had sex for money?" (0 = never; 1 = one; 2 = two; 3 = three; 4 = four; 5 = five or more)	1.326	2.158

NOTE.—Standardized correlation coefficients and Cronbach's α (for scales with more than two items) for street (first) and at-home samples are reported in parentheses. For homeless sample, $N = 390$; for at-home sample, $N = 563$.

*** $P \leq .01$, t-test of means (two-tailed).

surplus population, workers, supervisors, the petty bourgeoisie, and employers. Our measures of family class position use information on both parents (if present) to determine the parent with the greater class power (we use the remaining parent in single-parent families). We assume that families with the least class power (i.e., surplus-population families) experience the most strain.

We include two additional indicators of familial strain: a scale measuring coercive control and a single item tapping sexual abuse. The former is based on questions that ask, "Did your parents or guardians ever use physical forms of discipline?" and "Have you ever been intentionally struck so hard by a parent or guardian that it caused a bruise or bleeding?" Street workers argued the inappropriateness of inquiring directly about sexual abuse (especially without follow-up counseling); instead we asked respondents to describe problems they experienced at home and coded responses reflecting sexual abuse as a dummy variable. Consequently, this measure may underestimate sexual abuse.

Our measures of familial control include indicators of family structure and parental control. We measure family structure with an indicator of intactness; that is, the presence of two biological parents in the respondent's family. Parental control is conceptualized along two dimensions: relational control and instrumental control (Hagan, Gillis, and Simpson 1988). The former involves attachment and identification within the family, whereas the latter concerns supervision and surveillance. Four separate maternal and paternal relational control measures ask, "Did you talk with your mother/father about your thoughts and feelings?" and "Would you like to be the kind of person your mother/father is?" The four maternal and paternal instrumental control measures ask, "Did your mother/father know your whereabouts/who you were with when you were away from home?"

Our indicators of school experiences include measures of school control and school-induced strain. We measure two elements of the former, involvement and commitment, with items that inquire about time devoted to homework while in school and educational aspirations (see Hirschi 1969; Wiatrowski, Griswold, and Roberts 1981). We measure school-induced strain by asking about the frequency of problems with teachers.

Although we have measured a wide range of concepts, we may still have misspecified the causation of crime in some way; this could result in spuriously attributing causal significance to situational variables. To guard further against this, we include several omnibus controls: length of time on the street (the length of time at risk), number of times the respondent left home (previous street experiences), criminal support at home (the number of home friends arrested) and on the street (the number of street friends arrested), and respondent's participation in crime and delinquency while at home.[5]

Our indicators of delinquency at home are comparable to our measures of street delinquency introduced above. For example, we measure serious theft and prostitution at home with items parallel to those used for the street. However, since we do not have an indicator of theft of food at home, we use a two-item scale measuring minor theft. This scale utilizes items that ask how often the respondents took things valued at less than $5, and at between $5 and $10.

Controls for crime at home are important because compared with nonrunaways, runaways may have more extensive criminal backgrounds before leaving home (see Brennan et al. 1978). Compared with at-home samples used in other research, our homeless youths have higher prevalence rates of crime while living at home (see Hindelang et al. 1981; Hirschi 1969; Paternoster and Triplett 1988). Nonetheless, the prevalence of crime on the street is significantly higher than crime at home (McCarthy and Hagan 1991).

Methods

We first present bivariate relationships between our independent variables and the three dependent measures of delinquency. We then separately estimate three ordinary least squares (OLS) regression models for each measure of street delinquency: model 1 includes only the situational and control street variables; model 2 is restricted to background and developmental variables; and model 3 combines street with background/developmental variables. These models allow us to consider the extent to which any influence of situational variables is predetermined by background and developmental as well as by control variables. To the extent that our situational measures of difficulties on the street have measurable direct effects on theft of food, serious theft, and prostitution—independent of time on the street and delinquent associations on the street and at home, and independent of behaviors and background differences of strain and control at home—it would seem methodologically conservative and substantively justifiable to regard these situational effects as causal.

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[5] We also added controls for drug use at home and on the streets, but this produced collinearity problems. Alternatively, replacing the home measures of crime and delinquency with drug-use measures produced results comparable with those presented below. Meanwhile, although we include up to 24 direct effects in our models, multicollinearity does not otherwise appear to be a problem. All models were successfully estimated with the OLS regression tolerance level reset to .30. Regression diagnostics also indicate that there are only two occurrences of a component associated with a high condition index on two or more variables: maternal and paternal instrumental control, and the arrest of home and street friends. Collapsing these items into two scale measures produces results consistent with those reported in the text.

We incorporate the school sample in a second, supplementary part of our analysis. We do this because it is likely, and consistent with arguments of both strain and control theories, that the background factors that encourage crime on the street encourage youths to leave home in the first place. That is, the error terms for leaving home and street crime may be correlated, leading to specification errors in the estimation of causal effects in the street sample.

To address these interrelated issues we introduce data on school youths and estimate a "selection" probit equation of homelessness together with "substantive" OLS equations of delinquency on the street (see Berk 1983). The predicted values from the probit equation are transformed into a hazard rate and entered into OLS street-delinquency equations to address concerns about sample selection. In the selection equation, homelessness is a dummy endogenous variable, with homelessness coded "1." Since street youths are overrepresented, we reweight the street and school populations in the selection probit equation in proportion to their representation in the general population (see Manski and Lerman 1977; Xie and Manski 1989). Although the exact size of Toronto's homeless adolescent population is unknown, estimates range from 10,000 to 12,000 (see Kelly 1989). We use a conservative estimate of 7,500 for a weighting ratio of 20 at-home youths to one homeless youth.[6]

Descriptive Statistics

The descriptive statistics in table 1 inform us about youths at home and on the street. Our sample of homeless youths is, on average, older, with a greater proportion of males. The class backgrounds of these two groups are similar, but there is a larger representation of surplus worker parents among the homeless. More prominent contrasts occur for measures of family control and strain. Compared with youths at home, homeless youths are more likely to have lived in a family lacking one or both biological parents, and they have experienced lower amounts of parental relational and instrumental control and greater amounts of coercive control and sexual abuse.

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[6] Varying the weighting ratio from a low of 10:1 to a high of 25:1 produces only marginally different results. The estimated ratio of at-home to homeless youth in the United States is within this range. As already stated above, there are an estimated 750,000–1,500,000 homeless youths in the United States (Shane 1989), with somewhere in the area of 20 million youths in the population age groups most at risk (Zeldin 1991). This adds confidence to our Toronto estimates.

The mean measures of school experience reveal that homeless adolescents did significantly less homework, had lower academic aspirations than their at-home counterparts, and had more problems with teachers. These youths also had greater exposure to crime while living at home. Compared with those at home, youths on the street had more extensive networks of arrested peers and reported more minor and serious theft (neither group reported much home prostitution).

On average, homeless youths had left home on a number of occasions and had not lived at home for a period of six months or more. During this period, the average street adolescent worked but was unemployed at the time of the study and had spent an entire day without food at least once or twice. These youths were also more likely to have used the street and public hostels for shelter, rather than have relied on friends and relatives.

Since taking to the street, the majority of homeless youths had replaced their delinquent friends from home with a comparable group on the street. Street youths also report extensive participation in criminal activities: 45% reported stealing food, 75% indicated they had committed a serious theft, and 30% reported working in prostitution since leaving home.

In sum, our data indicate that, compared with adolescents still at home, street youths are disproportionately drawn from families characterized by aversive strain and an absence of control. That is, the latter have a substantially greater likelihood of being the victims of physical and sexual abuse and parental neglect. Not surprisingly, they are also more likely to have experienced problems at school, have delinquent friends, and to have violated the law. These factors are important in understanding the etiology of leaving home (see Brennan et al. 1978; Janus et al. 1987; Kufeldt and Nimmo, 1987; Hagan and McCarthy, in press). Although it is not central here, we will discuss this process later when we examine the results from a probit analysis of leaving home.

Unfortunately, our cross-sectional data do not allow us to temporally order the relationships between background variables; thus, in our analysis we enter these variables simultaneously. However, we do feel comfortable in assuming that familial abuse and neglect precede delinquency. Although parents may neglect their adolescent children or use physical force as a response to their childrens' delinquencies or contacts with delinquent friends, we know of no literature that demonstrates that previously nonviolent parents *begin* to use force when their children reach adolescence and misbehave in the ways just described.[7] Rather, the research literature suggests the primacy of abuse that we also assume (Wauchope and Straus 1990; Wilson and Daly, 1987; Wilson, Daly, and Weghorst 1980).

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[7] Agnew and Huguley (1989, p. 701), e.g., show that some parents respond to delinquency with physical force, but they also suggest that this kind of violence is associated

RESULTS

Adversity and Crime

The bivariate correlations reported in table 2 between independent variables and the dependent measures of theft of food, serious theft, and prostitution reveal that the strongest and most consistent relationships involve two measures of situational problems and two street control variables: hunger and shelter, and length of time on the street and arrests of street friends. In contrast, our third situational measure, unemployment, is significantly associated with only one indicator of street crime, prostitution. There is also evidence of gender specialization in street crime; gender is positively related to theft of food and serious theft but negatively associated with prostitution. This indicates that, as expected, males are more likely to steal while females are more likely to work in the sex trade.

The bivariate effects also reveal several significant correlations between strain and control variables and street crime, but the pattern of relationships is less clear. Perhaps all we can say to this point is that situational variables are at least as (if not more) prominently associated with street crime as background and developmental variables are, even though situational variables are rarely measured in crime or delinquency research; furthermore, gender appears to channel involvement in street crime.

We begin our multivariate analysis with the models of theft of food in table 3. We report unstandardized regression coefficients in the tables that follow and, where useful, standardized coefficients in the text. Model 1 introduces the three measures of situational problems (unemployment, hunger, and shelter) together with three measures of street control variables (length of time on the street, and number of previous street experiences and street friends arrested). Although all these measures except unemployment are significantly related to theft of food at the bivariate level, hunger alone has a substantial and statistically significant effect on theft of food when they are introduced together ($\beta = .311$, $P \leq .01$).

We introduce background and developmental variables alone in model 2. Of these 18 variables, four have significant effects: theft of food de-

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with childhood disciplinary techniques that are punitive or violent. This is consistent with the sequence we assume.

TABLE 2

BIVARIATE COEFFICIENTS FOR STREET CRIME (THEFT OF FOOD, SERIOUS THEFT, AND PROSTITUTION) FOR HOMELESS SAMPLE

INDEPENDENT VARIABLES	DEPENDENT VARIABLES		
	Theft of Food	Serious Theft	Prostitution
Age	−.048	.026	−.008
Gender	.160***	.180***	−.096**
Surplus workers	−.001	.017	.048
Workers	−.039	−.018	−.123***
Petite bourgeoisie	−.034	−.082*	−.062
Employers	.075	−.022	.031
Intact family	−.003	.010	.034
Maternal instrumental control	−.135***	−.162***	−.042
Paternal instrumental control	−.026	−.040	−.075*
Maternal relational control	−.011	−.092**	−.004
Paternal relational control	−.029	−.046	−.064
Coercive control	−.019	−.021	.105**
Sexual abuse	−.003	−.038	.129***
Homework	−.276***	−.236***	.101**
School goals	.027	.035	−.030
Conflict with teachers	.171***	.251***	.018
Home friends arrested	.212***	.252***	−.039
Minor theft scale (at home)	.308***	.319***	.011
Serious theft scale (at home)	.303***	.461***	.011
Prostitution (at home)	−.008	.036	.184***
Unemployed	−.034	−.045	.103**
Hunger	.369***	.262***	.194***
Shelter	.169***	.164***	.230***
Number of times left home	.086**	.036	.203***
Length of time on street	.128***	.251***	.227***
Street friends arrested	.197***	.394***	.168***

NOTE.—$N = 390$. All significant tests are two-tailed.
* $P \leq .10$. ** $P \leq .05$. *** $P \leq .01$.

creases with commitment to homework ($\beta = -.203$, $P \leq .01$), increases with the number of home friends arrested ($\beta = .114$, $P \leq .05$) and with minor theft at home ($\beta = .150$, $P \leq .01$), and is more common among males ($\beta = .097$, $P \leq .10$). In model 3 we combine street and background/developmental variables. With the exception of gender, the above effects remain significant and the effect of hunger increases slightly. Two additional effects are also significant in the combined model: theft of food is positively related to employer-class origin and negatively associated with age. Overall, these models suggest that hunger does not simply mediate effects of background or developmental variables on theft of

TABLE 3

UNSTANDARDIZED COEFFICIENTS FOR REGRESSIONS OF THEFT OF FOOD FOR HOMELESS SAMPLE

	MODEL 1		MODEL 2		MODEL 3	
INDEPENDENT VARIABLES	b	SE	b	SE	b	SE
Age			−.038	.032	−.058*	.032
Gender			.183*	.102	.154	.097
Surplus workers			−.146	.171	−.117	.162
Workers			−.051	.105	−.028	.098
Petite bourgeoisie			−.204	.258	−.275	.246
Employers			.182	.124	.278**	.117
Intact family			−.035	.094	−.084	.089
Maternal instrumental control			−.037	.036	−.024	.034
Paternal instrumental control			.019	.036	.033	.033
Maternal relational control			.021	.032	.025	.030
Paternal relational control			−.024	.033	−.001	.032
Coercive control			.006	.019	−.004	.018
Sexual abuse			.201	.182	−.046	.174
Homework			−.137***	.035	−.141***	.033
School goals			.028	.031	.043	.029
Conflict with teachers			.043	.036	.006	.034
Home friends arrested			.066**	.029	.058*	.030
Home minor-theft scale			.087***	.019	.079***	.018
Unemployment	−.068	.059			−.092	.057
Hunger	.293***	.150			.301***	.048
Shelter	.050	.045			.058	.046
Number of times left home	.002	.003			.001	.002
Length of time on street	.005	.008			.009	.008
Street friends arrested	.050*	.029			−.012	.031
Constant	.028	.159	1.016	.582	.859	.604
R^2	.152		.196		.311	

NOTE.—$N = 390$. All significance tests are two-tailed.
* $P \leq .10$. ** $P \leq .05$. *** $P \leq .01$.

food on the street; rather, hunger accounts for much of this theft in its own right.

We turn next to table 4 and the models of serious theft. When we introduce street variables in model 1, hunger again has a substantial and significant effect ($\beta = .114$, $P \leq .05$). In addition, theft is also related to shelter ($\beta = .093$, $P \leq .10$) and two street control variables: time on the street ($\beta = .153$, $P \leq .01$) and street friends arrested ($\beta = .318$, $P \leq .01$).

Model 2 considers background and developmental variables alone.

TABLE 4

UNSTANDARDIZED COEFFICIENTS FOR REGRESSIONS OF SERIOUS THEFT FOR HOMELESS SAMPLE

	MODEL 1		MODEL 2		MODEL 3	
INDEPENDENT VARIABLES	b	SE	b	SE	b	SE
Age			.017	.077	−.125	.077
Gender			.163	.249	.015	.236
Surplus workers			−.404	.410	−.232	.384
Workers			−.071	.250	.072	.234
Petite bourgeoisie			−1.414**	.616	−1.074*	.583
Employers			−.259	.296	−.031	.278
Intact family			−.067	.224	−.129	.211
Maternal instrumental control			−.062	.085	.014	.081
Paternal instrumental control			.090	.085	.069	.079
Maternal relational control			−.099	.077	−.093	.072
Paternal relational control			−.040	.080	.015	.076
Coercive control			.008	.045	−.015	.042
Sexual abuse			.142	.437	−.508	.414
Homework			−.173**	.083	−.192**	.078
School goals			.092	.073	.094	.069
Conflict with teachers			.182**	.085	.125	.081
Home friends arrested			.195***	.070	.061	.071
Home serious-theft scale			.383***	.049	.360***	.046
Unemployment	−.141	.144			−.183	.135
Hunger	.270**	.121			.288***	.113
Shelter	.225*	.116			.221**	.108
Number of times left home	−.002	.007			−.006	.006
Length of time on street	.061***	.019			.072***	.019
Street friends arrested	.467***	.071			.289***	.073
Constant	.503	.389	1.145	1.386	1.914	1.431
R^2	.209		.286		.398	

NOTE.—$N = 390$. All significance tests are two-tailed.
* $P \leq .10$. ** $P \leq .05$. *** $P \leq .01$.

Like theft of food, serious theft is negatively related to homework ($\beta = -.101$, $P \leq 05$), and positively associated with the number of home friends arrested ($\beta = .132$, $P \leq .01$) and theft at home ($\beta = .389$, $P \leq .01$). Serious theft is also less common among those from families belonging to petty bourgeoisie ($\beta = .103$, $P \leq .05$).

When we introduce the background/developmental and street variables together in model 3, most of the previously reported effects are

TABLE 5

UNSTANDARDIZED COEFFICIENTS FOR REGRESSIONS OF PROSTITUTION FOR HOMELESS SAMPLE

	MODEL 1		MODEL 2		MODEL 3	
INDEPENDENT VARIABLES	b	SE	b	SE	b	SE
Age			.015	.082	.015	.084
Gender			−.165	.261	−.288	.249
Surplus workers			.159	.436	.285	.413
Workers			−.622**	.268	−.494**	.253
Petite bourgeoisie			−1.033	.664	−.486	.636
Employers			−.146	.318	.147	.302
Intact family			.115	.241	.127	.229
Maternal instrumental control			.001	.091	.060	.088
Paternal instrumental control			−.081	.091	−.068	.086
Maternal relational control			−.008	.083	.006	.078
Paternal relational control			−.039	.086	−.025	.082
Coercive control			.078	.048	.039	.046
Sexual abuse			.998**	.468	.458	.449
Homework			.145	.089	.161	.099
School goals			−.084	.078	−.062	.074
Conflict with teachers			.107	.092	.024	.088
Home friends arrested			−.046	.074	−.120	.077
Prostitution at home			.671***	.177	.578***	.167
Unemployment	.301**	.143			.286**	.147
Hunger	.124	.119			.083	.123
Shelter	.361***	.115			.330***	.118
Number of times left home	.020***	.007			.020***	.007
Length of time on street	.076***	.019			.070***	.021
Street friends arrested	.114	.070			.173**	.079
Constant	−1.317	.385	1.071	1.487	−1.328	1.550
R^2	.147		.102		.232	

NOTE.—N = 390. All significance tests are two-tailed.
* $P \leq .10$. ** $P \leq .05$. *** $P \leq .01$.

relatively unchanged. Exceptions are that home friends arrested and conflict with teachers lose significance. In contrast, both hunger and shelter remain significantly related to serious theft on the street; so, as with hunger and the theft of food, these situational measures operate in their own rights and are not simply products or mediators of background or developmental variables.

We turn next to models of street prostitution in table 5. Two situational measures and two street control variables are significantly associated with prostitution when we introduce street variables alone in model 1: unemployment ($\beta = .105$, $P \leq .05$), shelter ($\beta = .158$, $P \leq .01$), time on the street ($\beta = .201$, $P \leq .01$), and the number of times (respondent) left home ($\beta = .146$, $P \leq .01$). In contrast to the models for theft, the significant bivariate relationship between hunger and prostitution decreases to nonsignificance.

According to the results in model 2, prostitution is also related to a number of background variables: sexual abuse ($\beta = .112$, $P \leq .05$), prostitution at home ($\beta = .192$, $P \leq .01$), and a family background in the working class ($\beta = -.127$, $P \leq .05$). With a few exceptions, the above effects remain essentially unchanged when we add background and developmental variables to a model with street variables (see model 3). The only substantial difference is the emergence of a significant relationship between prostitution and street friends arrested ($\beta = .125$, $P \leq .05$). Thus, although we introduce 21 control variables, two situational measures—unemployment and shelter—retain their sizable, direct, and significant effects on prostitution ($b = .286$ and $.330$).

In addition to the above, we also estimated models that added multiplicative terms representing gender interactions with measures of unemployment, shelter, and hunger. We did this for three reasons: previous research indicates that gender interacts with other variables in the causation of crime and delinquency (e.g., Morash and Chesney-Lind 1991); other studies of homeless youths point to gender-specific effects of street life (e.g., Whitbeck and Simons 1990); and because we wish to understand more fully the effects of situational factors. None of the interaction effects are significant in models for food theft and prostitution. However, gender interactions with unemployment and shelter significantly affect serious theft ($b = .504$, $P \leq .10$; $b = .369$, $P \leq .10$); these indicate that the effects of unemployment and shelter are pronounced among males.

A Further Test

Our results suggest that adverse situations are prominent in the causation of street crime. However, thus far we have considered only youths who are on the street, and we have had no means of taking into account the process of their getting there. The final part of our analysis brings that process into the picture by incorporating the Toronto school sample we introduced in our "Methods" section above. This involves using a response-based sampling strategy (see Xie and Manski 1989) which brings a relatively rare outcome-defined population (i.e., street youths) together with the remaining larger population (i.e., youths not on the street). As previously noted (n. 5 above), our conservative estimate is that over the approximate year of the study, for every 20 youths in school in Toronto, there was one on the street. This ratio is used to reweight the samples to represent the youth population of Toronto and to estimate a selective probit model of taking to the street. This probit model is used to adjust OLS substantive models (like those for street delinquency) for the process by which adolescents are selected into the street. The selection models allow us to see how home variables operate in determining the composition of the street-youth population, while the substantive models reveal how the street situational variables would affect a population of more broadly representative youth.

In table 6 we present unweighted and weighted bivariate correlations between background variables and the response-based outcome of taking to the streets. The overwhelming majority of these variables are significantly associated with being on the street, whether the samples are weighted or not. However, the picture is simplified in the multivariate probit models. Consistent with previous research, we find that the likelihood of leaving home increases with age, coercive control, sexual abuse, conflict with teachers, and having delinquent friends; conversely, it decreases with being in an intact family and having high academic aspirations (see Brennan et al. 1978; Shellow et al. 1967). While suppressed bivariate effects of age, intact family, and sexual abuse increase in the multivariate model, the effect of delinquent friends (i.e., home friends arrested) remains substantial and nearly unchanged. The stability of the effect of delinquent friends suggests that they are not the prior cause of other problems (e.g., with parents) that mediate their effect on leaving home; rather, the presence of delinquent friends is either a closely adjacent intervening cause or a direct cause of leaving home. Alternatively, the multivariate effect of coercive control declines by about 13%, suggesting that some of its effect is transmitted through subsequent variables.

It is important that the significant weighted bivariate coefficient for serious crime at home is reduced to nonsignificance when 17 other variables are considered along with home crime in the multivariate probit equation. This suggests that serious crime at home is spuriously associated with leaving home and that other variables such as delinquent friends, school problems, and parental neglect and abuse are more salient causes of taking to the street. It is possible that home delinquency causes problems with parents and that these mediate delinquency's effect on leaving home, but this requires an assumption that home delinquency has temporal priority to these variables, an assumption we have not found supported in the research literatures on delinquency or runaways. Nonetheless, the time-dependent sequence of these events should be in-

TABLE 6

STANDARDIZED BIVARIATE REGRESSION COEFFICIENTS AND PROBIT ESTIMATES FOR MODELS OF LEAVING HOME

	BIVARIATE COEFFICIENTS		PROBIT ESTIMATES	
INDEPENDENT VARIABLES	Unweighted	Weighted	Coefficient	SE
Age	.325***	.153***	.266***	.076
Gender	.206***	.089**	.044	.215
Surplus workers	.128***	.074***	.606	.461
Workers	−.033	−.014	−.112	.233
Petite bourgeoisie	−.006	−.003	.105	.552
Employers	.009	.004	.020	.279
Intact family	−.400***	−.189***	−.606***	.196
Maternal instrumental control	−.405***	−.187***	−.087	.072
Paternal instrumental control	−.324***	−.142***	−.076	.072
Maternal relational control	−.188***	−.083**	.001	.074
Paternal relational control	−.103***	−.046*	.041	.082
Coercive control	.467***	.262***	.225***	.046
Sexual abuse	.162***	.121***	1.089**	.556
Homework	−.304***	−.146***	−.026	.091
School goals	−.362***	−.216***	−.206***	.065
Conflict with teachers	.388***	.203***	.198**	.096
Home friends arrested	.481***	.268***	.264***	.077
Serious crime at home scale	.325***	.163***	−.012	.047
Constant			−5.937	1.420
χ^2 (*df* 18)			1073.400***	

NOTE.—For homeless sample, $N = 390$; for at-home sample, $N = 563$. All significance tests are two-tailed.
* $P \leq .10$.   ** $P \leq .05$.   *** $P \leq .01$.

vestigated further. Meanwhile, we control for combined effects of all at-home variables in the selection equations presented next.

We now estimate a series of substantive equations for the three kinds of street crime in which the results of the probit equations are expressed as hazard rates. The hazard rate introduces the composite influence of background variables on taking to the street into the substantive equations, thereby adjusting for the process of selection into the street sample. Our goal is to remove bias that may result from sample selection in the OLS estimates of causal effects presented above. Introducing the home social control and strain variables simultaneously with the hazard rates produces collinearity problems, so in this analysis we include only the hazard rates.

The results of these substantive equations (see table 7) are consistent with those reported above. For example, the argument that strain and

TABLE 7

Unstandardized Coefficients for Regressions of Theft of Food, Serious Theft, and Prostitution on the Street, Correcting for Sample Selection Bias

	Theft of Food				Serious Theft				Prostitution			
	Model 1		Model 2		Model 1		Model 2		Model 1		Model 2	
Independent Variables	Coefficients	SE	Coefficients	SE	Coefficients	SE	Coefficients	SE	Coefficients	SE	Coefficients	SE
λ	−.142**	.065	−.018	.063	−.590***	.167	−.124	.155	−.256*	.157	.042	.153
Unemployment			−.065	.058			−.118	.142			.299**	.140
Hunger			.293***	.049			.268**	.119			.144	.118
Shelter			.067	.045			.184*	.110			.329***	.109
Number of times left home			.002	.003			−.002	.006			.020***	.006
Length of time on street			.005	.008			.060***	.019			.077***	.019
Street friends arrested			.050	.029			.461***	.072			.124*	.071
Constant	.905	.094	.056	.188	3.746	.240	.783	.464	1.647	.226	−1.331	.459
R^2	.012		.154		.033		.211		.007		.147	

Note.—For homeless sample $N = 390$; for at-home sample, $N = 563$. All significance tests are two-tailed.
* $P \geq .10$.
** $P \geq .05$.
*** $P \geq .01$.

control background variables operate on crime through youths' taking to the street is supported by the finding that the hazard rate (Λ) is significant in the reduced equations and then nonsignificant in the full structural equations for all three dependent variables (see models 1 and 2 for each dependent variable). The findings from the substantive equations (see model 2 for each variable) also reaffirm the importance of the situational variables. According to these results, theft of food and serious theft increase with hunger; serious theft and prostitution increase with problems of shelter; and prostitution increases with unemployment. These effects are all significant and substantial and therefore increase our confidence that situational problems of sustenance and security cause street crime.

DISCUSSION AND CONCLUSIONS

Contemporary sociological criminology is characterized by its focus on background and developmental factors that are assumed to generate delinquent and criminal behavior (see Matza 1964; Gottfredson and Hirschi 1990). The result is an array of theoretical orientations that highlight preadolescent and adolescent life experiences that take place in the family, at school, and among peers, that lead to delinquency. In the background of this orientation is an unanswered question of just how far back such causes might be traced (e.g., White et al. 1990). This question animates a renewed interest among criminologists in the role of sociobiological predispositions and propensities in the causation of crime.

Our purpose here is to push sociological criminology in precisely the opposite direction: toward foreground causes of delinquency and crime, through the study of criminogenic situations. Such situations were once central in the study of crime, as exemplified in Sellin's (1937) study of crime during the Great Depression. The Chicago tradition of field research provided a means of incorporating street situations into the study of delinquency that lasted from Thrasher's (1927) investigation of *The Gang* through Short and Strodtbeck's (1965) work on *Group Process and Gang Delinquency*. This important tradition made street settings central to the collection of data and the organization of delinquency research. However, the emergence of the contemporary self-report paradigm had the unintended consequences of moving crime and delinquency research away from the street and into the schools and of shifting attention away from the criminogenic nature of situations in which youths on the street often find themselves. The timing of this shift, in the late 1950s and early 1960s, may be significant because this was a period of relative prosperity in America, when it was perhaps possible to believe that conditions of the city were more malleable and the prospects of the urban underclass more benign. Yet, the 1980s and 1990s have made many citizens more sensitive to the harsh conditions that often characterize inner-city life and the potentially criminogenic conditions of contemporary street life. It is time to return our research to these street settings.

Our return to the street builds on what we have learned about background and developmental factors as causes of leaving home for the street, and of crime and delinquency. We propose that an integration of strain and control theories can span background, developmental, and foreground factors. A key to this integration is a focus on aversive conditions at home and in school because taking to the street and subsequent criminal activity are often responses to successive and causally linked aversive conditions; as for example, when parental abuse and neglect at home lead to youths' taking to the street. Our integration also acknowledges that absence of family and school ties can lead to the adoption of street life, so that the presence of strain and absence of control can lead to street crime indirectly as well as directly. We see no contradiction in the integration of these theoretical orientations. Indeed, our perspective on strain is in some ways reminiscent of the focus on conditions of overcontrol and undercontrol found in early versions of control theory (e.g., Durkheim [1897] 1951; Nye 1958). For example, coercive parental control of children can be conceived as either a source of strain or as overcontrol.

Our findings provide empirical support for our integration of strain and control theory. This support is most apparent in the final stage of our combined analysis of separately sampled school youths and street youths. This analysis involves estimation of a substantive model of taking to the street, and subsequent estimations of selection models that include composite variables that incorporate effects of leaving home into analyses of theft of food, more serious theft, and prostitution. The composite variables in the selection models support the premise that background and developmental variables—especially, but not exclusively, including coercive parental control, sexual abuse, and delinquency at home—directly influence taking to the street and indirectly influence theft and prostitution. In effect, this indicates that the youths who are most likely to become homeless are also the most likely to engage in theft and prostitution while on the street.

Meanwhile, variables measuring adverse situations exercise strong direct effects on street crime, apart from the mediating role they can also play. These effects vary somewhat by type of crime and gender, but overall, situational variables play a prominent role in the causation of street crime. This is true even when street variables are pitted against each other with a wide range of background/developmental and control variables and when sample-selection effects are taken into account. Consistently, hunger causes theft of food, problems of hunger and shelter lead to serious theft, and problems of shelter and unemployment produce prostitution. These situational effects gain plausibility from the comprehensive range of control measures we have imposed.

The finding that variables measuring situational adversity are salient in the causation of street delinquency is important not only because it redirects and broadens our theoretical perspective on crime and delinquency, but also because it extends our understanding of street life more generally. That is, this study confirms that many of the most serious problems of the street derive from the conditions of street life itself, including the problems of sustenance and security that street life produces. There is, of course, policy relevance in the fact that street situations can be changed.

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Reciprocal Effects of Religiosity, Cohabitation, and Marriage[1]

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This article formulates and tests theoretical hypotheses of the reciprocal causal relationships between the formation of cohabiting and marital unions and religious commitment and participation. The article uses data from a panel study of mothers and children to show that the religiosity of both mothers and children influences the cohabiting and marital behavior of children, with those from less religious families having higher rates of entering intimate coresidential unions and a tendency to substitute cohabitation for marriage. Analyses of the reciprocal influences of cohabitation and marriage on religiosity indicate that cohabitation decreases religiosity, while marriage leads to increased religious participation.

INTRODUCTION

The connection between religion and family has long been recognized by social scientists and religious scholars. Religious affiliation has been shown to be associated with many dimensions of family life, including marriage, divorce, family size, and premarital sex. Premarital sex, childbearing, and divorce are also related to various dimensions of religiosity, including attendance, commitment, and the centrality of religion

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in people's lives. Scholars have also observed that historical changes in the content and authority of religion have been associated with trends in family behavior, such as falling fertility, rising divorce, increased nonmarital cohabitation, and more premarital sex and pregnancy.

Virtually all the scholarly literature has interpreted the connection between religion and family behavior using a causal model that posits religious affiliation and commitment as exogenous explanatory variables and family behaviors and attitudes as the endogenous results of variation in religious affiliation and commitment. This model assumes that family behavior has no influence on religious activity and commitment and interprets all the empirical association between the two domains as the effect of religion on family.

An emerging literature, however, suggests that this view of the family and religion connection is simplistic since it ignores the reciprocal effects of family behavior on religious attendance and commitment. As we document below, there are theoretical reasons to believe that family values and behavior influence religious behavior and orientations, and these substantive influences merit serious scholarly examination. Furthermore, without taking into account the reciprocal effects of family on religion, it is impossible to obtain unbiased empirical estimates of the influence of religion on family.

In this article we further understand the causal interconnections between family and religion by investigating the reciprocal relationships between religious participation and commitment and one specific domain of family life—marriage and cohabitation. We formulate a theoretical model that considers the causal influences of cohabitation and marriage on religious involvement as well as the influence of religion on young people's union-formation experience. This model of reciprocal connections between religion and family is evaluated empirically using data from a panel study of mothers and their young adult children. We investigate the causal influence of religion by estimating the extent to which children's cohabitational and marital experiences between the ages of 18 and 23 are related to the mother's and child's religious commitments and involvements when the young people were 18. The reciprocal effects of union formation on religion are evaluated by predicting changes in religious involvement between the ages of 18 and 23 from cohabitational and marital experiences observed during the intervening five years.

THEORETICAL REASONING AND HYPOTHESES

Marriage, sexuality, and childbearing have been intricately interwoven with the ethics of Christian churches for centuries. The Roman Catholic church declared marriage to be a sacrament, sanctioned sexual expression only within marriage, and forbade artificial means of contraception. The Protestant Reformation modified the Roman Catholic view of marriage, but several of these family doctrines continued to characterize subsequent Protestant teachings (Fagley 1960; Hoyt 1968; Kennedy 1970; Noonan 1966; U.S. Catholic Conference 1977). While there have been recent changes in the family doctrines of many religious groups, most Christian churches continue to value marriage and family life and either proscribe premarital sex or encourage the postponement of the initiation of sexual relations (Hadden 1983; Hargrove 1983; U.S. Catholic Conference 1977). As a result, many studies document strong associations between people's religious affiliations and commitments and their sexual values and behavior (Chilman 1983; DeLamater and MacCorquodale 1979; Inazu and Fox 1980; Jessor and Jessor 1975; Sweet and Bumpass 1990; Thornton and Camburn 1989; Zelnik, Kantner, and Ford 1981).

The connection between nonmarital sex and religious affiliation and commitment is probably even stronger when the sexual relationship includes the sharing of residential quarters. Since cohabitation publicly acknowledges a sexual relationship, an acknowledgement not usually made in other intimate nonmarital relationships, it can attract the attention of important reference groups to the individual's nonconformity with religious teachings and provoke a confrontation of the religious community's norms with the individual's behavior. Recent empirical research confirms a correlation between cohabitation and religious affiliation and participation (Tanfer 1987; Sweet 1989; Sweet and Bumpass 1990).

The Catholic church and many of the more fundamentalist Protestant groups have maintained more restrictive teachings concerning sex outside of marriage than have less fundamentalist Protestants (Hargrove 1983; U.S. Catholic Conference 1977). Historically, the Catholic church has also been more authoritarian than Protestantism, although this difference has shrunk in recent years as the Catholic church's capacity to control the personal and familial behavior of its adherents has declined (Greeley, McCready, and McCourt 1976; Alwin 1984). These considerations suggest that cohabitational experience is lower among Catholics and fundamentalist Protestants than among nonfundamentalist Protestants. It is also likely that young people without religious affiliations are the most likely to endorse and experience cohabitation outside of marriage.

Since most religious groups discourage premarital sex and cohabitation, commitment to and participation in religious activities is likely to decrease the incidence of cohabitation. Religiosity may also increase the marriage rate because many religious groups place a high value on marriage, procreation, and family life. Frequent attendance at religious services and activities probably increases contact with religious messages encouraging marriage and discouraging premarital sex and cohabitation.

Involvement in religious institutions also enhances interaction with peers with positive feelings about marriage and restrictive attitudes toward premarital sex and cohabitation. In addition religious young people may have more contact with adults who encourage marriage and discourage cohabitation outside of marriage. The importance of religion in one's life could also influence cohabitation and marriage since young people who value religion highly are probably more likely to accept the teachings of their religious institutions. We thus expect that those who value religion and who are active in their religious groups are less likely to enter into a cohabiting relationship or will postpone such relationships until later in life.

The cohabitational experience of young adults can also be influenced by the religious commitment and participation of parents. Young people are usually socialized to affiliate with their parents' religious denominations and frequently adopt their parents' levels of religious commitment and participation. As a result, parents can influence the courtship and marriage values and behaviors of their children by influencing the children's own religiosity.

Strongly religious parents can also influence their children's cohabitational and marital behavior directly through their guidance and supervision. Young people with religious parents may also decide not to cohabit without marriage even if they, themselves, do not oppose the practice. The public nature of nonmarital cohabitation may enhance this decision in that children of religious parents may not want to embarrass their parents, create interpersonal conflict, or incur negative parental sanctions.

The positive values toward marriage of most religious groups and their negative attitudes toward cohabitation lead us to expect a trade-off between cohabitation and marriage according to levels of religiosity. A religious young couple contemplating entrance into a coresidential union would be positively drawn toward marriage and negatively oriented toward cohabitation, while less religious couples would be attracted more to cohabitation and less to marriage. This pattern leads to the hypothesis that young people from highly religious families and who have great personal religious commitments themselves have high marriage rates and low cohabitation rates while the less religious follow the opposite pattern.

The substitution of cohabitation for marriage would also result in lower overall rates of marriage for the less religious, regardless of whether or not there had been an earlier nonmarital cohabitation because a significant portion of those who cohabited might have gotten married if cohabitation had not been an alternative. If the cohabitation were lengthy or did not result in marriage, its delaying effect on eventual marriage would be substantial.

The less religious are also expected to have higher overall rates of union formation than others. This would occur if their higher acceptance of cohabitation led them to cohabit in situations where they believed they were not yet ready to marry. This could be true both for couples for whom cohabitation was a part of the courtship process leading to marriage and for couples not interested in marriage who wanted to enjoy the social and economic benefits of sharing residences with their partner. There are also situations where couples had decided on marriage but shared residences between their engagements and their marriages. The circumstances above are more likely for the less religious, which would lead them to form coresidential unions faster than those more involved with religious institutions.

While the preceding discussion casts religion as an explanatory variable determining marriage and cohabitation, there are good reasons to believe that union formation also influences religious behavior and commitment. Inconsistencies between personal behavior and the teachings of religious leaders can change individual religious commitments and participation (Greeley et al. 1976; Thornton 1985). Religious young people who enter into an intimate relationship outside of marriage would have strong motivation to question religious teachings that marriage was necessary for legitimation of sexual relationships (Thornton and Camburn 1989). They might also question the authority of their religious teachers in other vital areas. Commitment to religious faith and principles could also decline as individuals found their personal lives to be outside the normative teachings of their religious groups.

It is likely that attendance at religious services is less rewarding for young people who are cohabiting. Their behavior could receive implicit, and perhaps explicit, negative sanctioning by religious leaders and other adults attending services, and there could be deterioration in the quality of their relationships with their religious peers. With fewer positive interactions and many negative messages from religious sources, cohabiting individuals would have less motivation to attend services.

Childbearing and rearing is also less common among those cohabiting than among the married, which could have implications for differential religiosity. Since the provision of religious training for children can be an important motivation for parental religious attendance, the higher fertility of the married could provide an impetus to religious participation that cohabitors lack. Cohabitors, on the other hand, will be more highly motivated to control their fertility, which would involve them more with contraception and abortion. Since many religious groups strongly discourage abortion, a cohabitor's having had an abortion could result in additional conflicts between religious involvement and personal behavior. The expected result of all these considerations is high levels of religious attendance for the married, low levels among cohabitors, and intermediate levels among those remaining single.

DATA AND METHODS

The families participating in this study were selected from a probability sample of first-, second-, or fourth-born white children drawn from the 1961 birth records of the Detroit metropolitan area. Approximately equal numbers were selected from each parity group. The mothers were first interviewed in the winter of 1962, and subsequent interviews were conducted in the following fall (1962) and in 1963, 1966, 1977, 1980, and 1985. The children were interviewed in 1980 when they were 18 years old and in 1985 when they were 23. The original 1962 survey interviewed 92% of the sampled mothers, and the 1985 study interviewed both mothers and children in 82% of the families interviewed in 1962.[2]

The cohabitation and marriage variables were measured through a life-history calendar, described in detail elsewhere (Freedman et al. 1988). Monthly information was obtained for the time the participants were between 15 and 23.5 years old

The marriage domain was introduced with the question, "Have you ever been married?" Ever-married respondents were asked the dates of all marriages and marital dissolutions. The cohabitation domain was introduced by the question, "Have you *ever* lived together as a partner in an intimate relationship with a (man/woman) without being married to (him/her)?" For participants who had ever cohabited, information was ascertained about the dates cohabiting unions began and ended. The data collection procedures also allowed identification of cohabiting unions subsequently transformed into marriages. Since all the young adults participating in the study were less than 24 years old in 1985, the analysis was limited to entry into first cohabitation and marriage.

Sixty percent of the women and 45% of the men had experienced either a cohabiting or marital union. For the women the union-formation experience was distributed equally between cohabitation and marriage, with 30% exiting single life to both cohabitation and marriage. While a similar percentage of men had exited single life to enter a cohabiting union, only 14% had exited for marriage. Of course, many of the men and women whose first union was cohabitation went on to marry. By age 23.5, 24% of the men and 45% of the women had married.

The religious affiliation of the young adults was ascertained in 1980. Five groups were recognized in the analysis: Catholic, nonfundamentalist

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[2] The 82% figure was calculated with the small number of deceased or seriously ill mothers and children excluded from the denominator.

Protestant, fundamentalist Protestant or Baptist, other, and none. The five categories were converted to four dummy variables with nonfundamentalist Protestants treated as the reference group.

Two different concepts of religiosity were used in the analysis. One measured the participation of the mothers and children in religious services in 1980 and 1985 using the question: "How often do you usually attend religious services—would you say several times a week, once a week, a few times a month, once a month, or less than once a month?" Some study participants volunteered that they never attended religious services.

Our second measure of religiosity focused on another dimension of religious life—the importance of religion beyond attending services. The importance of religion was measured in 1980 by asking mothers and children, "Quite apart from attending religious services, how important would you say religion is to you—very important, somewhat important, or not important?" Both participation in religion and religion's importance were treated as interval-level measures in the analysis by coding the categories incrementally by units of one from low to high levels of religiosity.

To ensure the appropriate causal ordering of religion and union formation we excluded from the analysis all men and women who had cohabited or married prior to the 1980 interview. This excluded 6% of the men and 7% of the women in the sample.

Several control variables were used in the analysis to minimize the possibility that observed relationships between religion and union formation were produced by other variables. Three of these variables concerned the marital history of the mother, which previous analyses have shown to be related to the sexual, cohabitational, and marital experiences of children (Hayes 1987; McLanahan and Bumpass 1988; Hogan and Kitagawa 1985; Thornton 1991; Thornton and Camburn 1987). One such variable was premarital pregnancy, which was ascertained by a comparison of birth and marriage records and coded as a dichotomous dummy variable (Pratt 1965). The mother's age at marriage was coded in years and treated as an interval-level variable. The mother's experience with marital dissolution and remarriage from 1962 through 1980 was summarized with a four-category variable: continuously married, divorced and remarried, divorced and not remarried, and widowed. The stably married group was treated as the reference category.

Both the mother's and father's educations were included in the analysis to control the socioeconomic position of the family. The mother's education was measured as of 1980 and the father's as of 1962, with both variables treated as interval-level indicators.

We examine several aspects of the union-formation process. One approach considers the total union-formation rate, which is defined as the rate of entrance into *either* cohabitation or marriage, ignoring the distinction between the two. A second approach disaggregates the total union-formation rate and considers the two union types separately, with cohabitation and marriage treated as competing risks that terminate the single state. This approach permits separate analysis of the effects of religious affiliation and commitment on cohabitation and marriage. In the cohabitation analysis, people who married without first cohabiting were treated as censored at the time of their marriage. In the marriage analysis people who first cohabited were treated as censored at the time they entered their cohabitational relationship. A third approach investigated marriage only and ignored cohabitation.[3]

Our analysis used the proportional hazards approach first developed by Cox (1972; also see Teachman 1983 and Namboodiri and Suchindran 1987). In this model the hazard or rate of entering a union is written as $h(a) = ho(a)\ X \exp(\Sigma \mathbf{B}_k \mathbf{Z}_k)$, where $h(a)$ is the underlying or baseline rate, $\mathbf{Z}_k$ is the vector of the explanatory variables in the equation, and $\mathbf{B}_k$ is the vector of the effect parameters associated with the predictor variables (Namboodiri and Suchindran 1987; Teachman 1983). The underlying hazard rate, $ho(a)$, is not estimated but is adjusted multiplicatively by the predictor variables. We report the exponents of $\mathbf{B}_k$, which can be interpreted as the amount that the underlying hazard rate is multiplied by each unit change in the explanatory variable (Teachman 1983; Namboodiri and Suchindran 1987). The percentage change in the underlying hazard associated with each unit of change in the explanatory variable is equal to $100(\exp[\mathbf{B}_k] - 1)$.[4]

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[3] The analytic approach we outlined for studying marriage and cohabitation as competing risks assumes independence between the two alternatives ending single life. This occurs because we treat cohabiting people as censored at the time of cohabitation in the marriage analysis and similarly censor the marrying people in the cohabitation analysis. The implication of this assumption for our competing risks analysis of cohabitation and marriage is that, once all the independent variables are included in the analysis, the rate of marriage or cohabitation is unaffected by whether or not the other is an alternative. This is the independence of irrelevant alternatives assumption discussed by Luce (1959) and more recently by McFadden (1975) as the "red bus–blue bus" problem. Since both marriage and cohabitation involve the formation of a coresidential union, there is reason to believe that, compared with remaining single, they are not independent outcomes. For this reason we checked the results of our hazard analyses by using multinomial nested logit procedures that have been developed for use when competing choices are not independent (Maddala 1983; Ben-Akiva and Lerman 1987). We created a trichotomous dependent variable that collapses union-formation experience into three categories: cohabited first, married first, and neither cohabited or married. We used a nested multinomial, logit competing hazards model that handles timing in a manner analogous to the methodology of Allison (1982). Our multinomial nested logit results suggest that cohabitation and marriage are not independent alternatives, but the analyses that model this dependence produce virtually the same estimated effects as those obtained by the procedures described above.

Our investigation of the effects of union-formation experience on participation in religion estimated ordinary least squares (OLS) regression equations of the child's attendance at religious meetings at age 23.[5] The predictor variables in these equations included the control variables and measures of the child's union-formation experience during the subsequent five years. In order to control for the child's religiosity at the beginning of the period we also included the measure of the child's religious participation at age 18 in the equation.

RESULTS

Effects of Religious Affiliation on Union Formation

In table 1, we summarize the effects of religious affiliation on the various dimensions of union formation: the total union-formation rate; marriage and cohabitation treated as competing risks; and marriage, ignoring cohabitation. These analyses were conducted using the full set of controls discussed earlier, but excluding any measures of religious commitment or participation. Since the effects of religion on union formation might vary by the gender of the child, separate analyses were conducted for young women and men.[6]

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[4] The proportionality assumption was evaluated in a series of discrete-time analyses where the effects of religious attendance and of religion's being important were interacted with the month of observation in predicting marriage and cohabitation as competing risks. These tests revealed no statistically significant interactions between any of the religiosity variables—for both the mother and child—on either cohabitation or marriage for females, confirming the proportionality assumption for them. Among males, one of the four interactions between the child's religiosity variables and the child's age was statistically significant. However, three of the four interactions between maternal religiosity and age were statistically significant for males. These results suggest that the effects of maternal religiosity on the marriage and cohabitation experience of young men may decline with age.

[5] Although the intervals between these categories of religious attendance are not equal, we have used ordinary least squares regression to estimate models of this dependent variable. However we also used ordered-probit techniques to investigate the consequences of the ordering of categories imposed by this measure. Our results indicate that the ordering is not ideal, but that our estimates of the substantive coefficients are robust and that, when we use ordered-probit techniques, those coefficients are virtually identical.

[6] There are several theoretical reasons to expect the marital process to operate differently for men and women (Goldscheider and Waite 1986). Because of these we present separate models of cohabitation and marriage by gender. Empirically we found significant differences by sex, primarily in the base hazard rates, but also in the coefficients. These sex differences are discussed in the text.

TABLE 1

MULTIVARIATE ANALYSIS OF VARIOUS DIMENSIONS OF THE UNION-FORMATION PROCESS, BY GENDER

	TOTAL UNION FORMATION[a]		COHABITATION[b]		MARRIAGE[c]		MARRIAGE, IGNORING COHABITATION[d]	
VARIABLES	Female (1)	Male (2)	Female (3)	Male (4)	Female (5)	Male (6)	Female (7)	Male (8)
Child's religious affiliation:[e]								
Catholic	.63***	1.01	.59*	.96	.65*	1.06	.66*	1.06
Fundamentalist or Baptist	.86*	1.70	.84	1.77	.91	1.53	.99	1.30
Other	.59	.69	.46	1.01	.73		.70	
None	1.19	1.15	1.50	1.54	.80	.22	1.04	1.03
Mother's premarital pregnancy	1.32	1.72***	1.44	1.69**	1.26	1.86	1.25	1.48
Mother's age at marriage	.96*	.94*	.97	.94*	.94*	.96	.95*	.94*
Mother's marital disruption:[f]								
Widowed	2.09**	1.74	5.03***	2.15*	.55	1.12	1.15	.72
Divorced/remarried	1.68**	1.06	2.41***	1.58	1.19	.21	1.55*	.89
Divorced/not remarried	1.18**	1.41	1.98***	2.04***	.66	.38	.90	.79
Mother's education	.94	.85***	.99	.87***	.90*	.80***	.90***	.87**
Father's education	1.01	1.01	1.08	1.01	.95	1.02	.99	.96
N	384	398	384	398	384	381	406	404
χ^2	31.42	45.02	36.39	40.59	22.31	24.70	29.64	25.08
df	11	11	11	11	11	10	11	10

NOTE.—The effects shown here represent the multiplicative shift in the underlying rate associated with one unit change in the predictor variable.

[a] Marriage and cohabitation are treated as equivalent events here.

[b] For this analysis people who marry without cohabiting are censored from analysis at the time of the marriage.

[c] For this analysis people who cohabit before marriage are censored from analysis at the time of the cohabitation.

[d] For this analysis all cohabitation experience is treated as if it never occurred.

[e] The omitted category is nonfundamentalist Protestant.

[f] The omitted category is continuously married.

* $P < .05$.

** $P < .01$.

*** $P < .001$.

Looking first at Catholics, we see that the results are different for males and females. For young men, there are no substantial or statistically significant differences between Catholics and nonfundamentalist Protestants (the reference group) on any of the dimensions of union formation. There are, however, statistically significant differences between young Catholic women and their nonfundamentalist Protestant counterparts. Catholic women have substantially lower rates than nonfundamentalist Protestants on every dimension of union formation.[7] This indicates that the primary influence of Catholicism on the union-formation behavior of young women is to slow entrance into both cohabitation and marriage without shifting the balance between the two types of union formation.

Among women, fundamentalist Protestants may have lower overall rates of union formation than their nonfundamentalist counterparts, with this difference generally holding for both marriage and cohabitation. While the estimated effects of being fundamentalist or Baptist are not statistically significant for men, they are all in the opposite direction from those for women; these men have higher cohabitation and marriage rates than others.[8]

Both males and females without religious affiliations have cohabitational rates that are approximately 50% higher than those for nonfundamentalist Protestants. In the competing risks analysis the marriage rates for both males and females with no religious affiliations are substantially lower than those of the reference group. Although not statistically significant, the unaffiliated people have slightly higher total rates of union formation than others. These results indicate that having no religious affiliation may somewhat increase the pace of union formation, but it is more important that it shifts the pathway to coresidential unions from direct entrance into marriage to first experiencing cohabitation.[9]

Effects of Religiosity on Union Formation

Table 2 summarizes the effects of religious importance and participation on cohabitation, and table 3 summarizes the influence of the same vari-

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[7] A formal test of statistical significance shows that the interaction between gender and Catholic affiliation is significant at the .05 level for the marriage rate and the total union-formation rate.

[8] The gender interactions with being fundamentalist are statistically significant at the .11 level or less for cohabitation, marriage, and the total union-formation rate.

[9] The relatively small number of people without religious affiliations results in quite large standard errors for this group. As a result none of the reported coefficients for this group are statistically significant. However, in analyses that combined the data for women and men, the coefficient for those without religious groups was statistically significant at less than the .07 level in both the cohabitation and marriage analyses.

TABLE 2

MULTIVARIATE ANALYSIS OF COHABITATION TREATING MARRIAGE AS A COMPETING RISK, BY GENDER[a]

	1	2	3	4	5	6
Female:						
Mother's religiosity:						
Attendance	.84*				.98	
Importance		.88				1.01
Child's own religiosity:						
Attendance			.67***		.67***	
Importance				.65***		.65***
Male:						
Mother's religiosity:						
Attendance	.87*				.92	
Importance		.86				.91
Child's own religiosity:						
Attendance			.83***		.86*	
Importance				.79		.81

NOTE.—For definition of effects, see the first note of table 1. *All* equations also contain religious affiliation, premarital pregnancy, age at marriage, marital disruption, and mother's and father's education as noted in table 1. $N = 384$ for females; $N = 398$ for males.

[a] For this analysis people who marry without cohabiting are censored from analysis at the time of the marriage.

* $P < .05$.

** $P < .01$.

*** $P < .001$.

ables on marriage.[10] These data indicate that religiosity has substantial and opposite effects on cohabitation and marriage. Both religion's importance and participation in it are negatively related to cohabitation, and this is true for measures obtained from the mothers as well as the children (table 2). Furthermore, the depressing effect on cohabitation of both religious participation and religion's importance extends to both males and females.[11]

The data clearly support the hypothesis that maternal religious attendance reduces the cohabitation rate. Each unit on the maternal six-point attendance scale lowers the rate of cohabitation from 13 to 16 percentage

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[10] Tables 2 and 3 list only the coefficients for the religiosity variables, even though all of the variables listed in table 1 are also included in *all* of the equations estimated.

[11] Additional analyses investigated whether the effects of religiosity on cohabitation and marriage varied by religious affiliation. This was done by introducing interaction terms between religiosity and being Catholic and between religiosity and fundamentalist Protestantism. None of the interactions were statistically significant for men, and for women the only statistically significant interaction was between Catholicism and religiosity.

TABLE 3

MULTIVARIATE ANALYSIS OF MARRIAGE TREATING COHABITATION AS A COMPETING RISK, BY GENDER[a]

	1	2	3	4	5	6
Female:						
Mother's religiosity:						
Attendance	1.11				.97	
Importance		1.60**				1.46*
Child's own religiosity:						
Attendance			1.24**		1.26***	
Importance				1.43**		1.32
Male:						
Mother's religiosity:						
Attendance	1.18*				1.16	
Importance		1.01				.73
Child's own religiosity:						
Attendance			1.13		1.05	
Importance				1.34		1.33

NOTE.—For definition of effects see the first note of table 1. *All* equations also contain religious affiliation, premarital pregnancy, age at marriage, marital disruption, and mother's and father's education as noted in table 1. $N = 384$ for females; $N = 381$ for males.

[a] For this analysis people who cohabit before marriage are censored from analysis at the time of cohabitation.

* $P < .05$.

** $P < .01$.

*** $P < .001$.

points. The 13 percentage-point difference suggests that the cohabitation rate for young people whose mothers attended religious services several times a week would be only 50% of the rate for those whose mothers never attended. The estimated effect on cohabitation of the importance of religion to the mother is also negative for both men and women, but is not statistically significant.[12]

The data in table 2 also indicate that a substantial fraction of the influence of the mother's religious participation and religion's importance to her works through the child's religiosity. This is apparent from comparing the effects of the two maternal variables in equations that include the child's variables with equations that exclude measures from the child. For both men and women, the estimated effects of the mother's variables are substantially lower and statistically insignificant when the children's variables are also included in the equation.

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[12] However, in an analysis that combines the data for men and women the effect of the importance of religion to the mother has a probability level of less than .08.

Both the importance of religion and the participation in religion by the young people themselves are associated with lower rates of cohabitation. While this is true for both women and men, the effects for women are larger than those for men, with separate tests showing that these gender interactions are statistically significant. Furthermore, the reductions in cohabitation associated with religiosity are substantial. For example, even when mother's religious attendance is controlled, each unit of daughter's religious attendance is associated with a one-third decline in the cohabitation rate. This estimate implies that young women who attend religious services several times a week have a cohabitation rate only 14% as large as those who never attend. Putting this in the opposite way, the cohabitation rate of those who never attend religious services is about seven times as large as the rate for those who attend several times a week. A less drastic comparison is between those who attend once a week and those who attend less than once a month—the two modal categories of religious attendance for young adults. In this comparison the less religious daughters have cohabitation rates more than three times those of people who attend services frequently. The contrast between those saying that religion is not important and those saying it is very important is of only slightly smaller magnitude.

The data for marriage in table 3 generally represent a mirror image of the cohabitation data in table 2. Both the importance of religion to mothers and their church attendance are positively associated with marriage when cohabitation is treated as a competing risk. Both measures of children's religiosity are also positively related to the marriage rate, and most of these effects are substantial. As with the cohabitation analysis, the children's religiosity variables appear to have more influence on the behavior of women than men, although these gender interactions are not statistically significant.

The findings of tables 2 and 3 suggest that participating in religion and considering it important strongly affect the pathway to coresidential unions, with highly religious people choosing marriage over cohabitation more frequently than others do, and the less religious tending to make cohabitation their first union experience.

However, the data in table 4, which summarize the effects of religiosity on marriage, ignoring whether or not cohabitation had occurred, suggest that cohabitation is not a permanent substitute for marriage among many of those who are less religious. This conclusion is suggested by the fact that with only one exception all of the religiosity variables in table 4 have smaller effects when cohabitation is ignored than in table 3 where cohabitation is treated as a competing risk. The coefficients are generally

TABLE 4

MULTIVARIATE ANALYSIS OF MARRIAGE, IGNORING COHABITATION, BY GENDER[a]

	1	2	3	4	5	6
Female:						
Mother's religiosity:						
Attendance	1.05				.99	
Importance		1.37**				1.32*
Child's religiosity:						
Attendance			1.12**		1.12*	
Importance				1.23		1.16
Male:						
Mother's religiosity:						
Attendance	1.06				1.05	
Importance		1.04				1.03
Child's religiosity:						
Attendance			1.05		1.03	
Importance				1.02		1.02

NOTE.—For definition of effects see the first note of table 1. *All* equations also contain religious affiliation, premarital pregnancy, age at marriage, marital disruption, and mother's and father's education as noted in table 1. N = 406 for females; N = 404 for males.

[a] For this analysis all cohabitation experience is treated as if it never occurred.

* $P < .05$.

** $P < .01$.

*** $P < .001$.

smaller in table 4 than in table 3 because substantial numbers of those who initially cohabit go on to marry—and many do so rather quickly after initiating cohabitation. Furthermore, many of those who marry after cohabitation are probably less religious than those who went directly to marriage without first cohabiting. As a result, the addition of those who have previously cohabited to the ranks of the married increases the overall marriage rate for those with low religiosity more than it increases the marriage rate of the highly religious.

Nevertheless, even when the married population is increased by those who have previously cohabited, as in table 4, there is still a tendency for the more religious to have higher rates of marriage. The association is only modest and not statistically significant for men, but for women it is both substantial and statistically significant for some variables. These results thus indicate that less religious individuals are not only more likely to cohabit before marriage, but that this substitution of cohabitation for marriage also leads to overall lower rates of marriage.

Although the more religious and less religious young people take different pathways to coresidential unions, their overall rates of union formation are much less different. When the distinction between cohabitation and marriage is ignored, the effects of the religious participation and importance of young people are greatly reduced. While there is a general tendency for religious young people to enter coresidential unions more slowly than the less religious—consistent with our earlier hypothesis—this effect is quite small and not statistically significant (results not shown in tables).[13]

The Effects of Union Formation on Religious Participation

Shifting our attention from the effects of religiosity on union formation to the reciprocal influence of marriage and cohabitation on religious participation, we find strong evidence supporting the hypothesis of reciprocal causation (table 5). The young people who had cohabited as they matured from 18–23 years old had significantly lower rates of religious participation than those who had not cohabited. This is true even with controls for religious participation at age 18 and controls for a large number of other important variables.

Looking at the more complex union-formation history variable (cols. 2 and 4 of table 5), we find opposite effects of cohabitation and marriage. There is a substantial negative effect on religious attendance of having only cohabited during the previous five years. At the same time, there is a positive effect of having married without cohabiting on subsequent attendance at religious services. These results suggest that cohabiting reduces religious attendance, while going directly to marriage increases the religious involvement of young people. The effects of experiencing both cohabitation and marriage, however, are not clear since the estimated coefficients for men and women are in the opposite directions and not statistically significant.[14]

We also investigated the possibility that cohabitation influences the religious participation of Catholics and fundamentalist Protestants more than nonfundamentalist Protestants. This was done by introducing interaction terms between cohabitation and these two religious affiliation variables. The interaction effect between cohabitation experience and fundamentalist Protestantism was statistically significant for both men and women, while the interaction between Catholicism and cohabitation was statistically significant only for men. All of the statistically significant

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[13] Although none of the gender-specific religiosity coefficients were statistically significant, the coefficient for the young adult's religious participation was significant at the .07 level when the male and female samples were combined.

[14] While neither the coefficient for cohabited only or for both cohabited and married is statistically significant at the .05 level for women, both would be statistically significant at the .07 level.

TABLE 5

MULTIVARIATE ANALYSIS OF RELIGIOUS ATTENDANCE, BY GENDER

	FEMALE		MALE	
	(1)	(2)	(3)	(4)
Cohabited	−.40**		−.24*	
Union-formation experience:[a]				
Cohabited only		−.32		−.31*
Married only		.26*		.21
Both cohabited and married		−.30		.07
Religious attendance at age 18	.57***	.56***	.55***	.54***
Child's religious affiliation:[b]				
Catholic	−.03	.00	.15	.15
Fundamentalist or Baptist	.00	.02	.23	.25
Other	.06	.07	−.39	−.35
None	.07	.07	.08	.05
Mother's premarital pregnancy	.23	.21	.19	.16
Mother's age at marriage	.00	.00	.00	.00
Mother's marital disruption:[c]				
Widowed	.11	.12	.01	.04
Diovorced/remarried	−.03	−.05	−.09	−.06
Divorced/not remarried	.29	.30	−.06	−.03
Mother's education	−.01	.00	.05	.05
Father's education	−.05	−.05	.01	.01
N	381	381	396	396
R^2	.35	.36	.36	.37

NOTE.—The effects shown are unstandardized ordinary least square regression estimates.

[a] The omitted category is neither cohabited nor married.

[b] The omitted category is nonfundamentalist Protestant.

[c] The omitted category is continuously married.

* $P < .05$.

** $P < .01$.

*** $P < .001$.

interactions were in the direction of showing a stronger effect of cohabitational behavior on religious participation for Catholics and fundamentalist Protestants than for nonfundamentalist Protestants (results not shown). These results suggest that the decision to cohabit leads to a greater reduction in religious participation in those religious groups that are most opposed to sex outside of marriage.

Further insights into the interconnections between religious participation and union-formation behavior are provided in table 6, where 1985 religious attendance by union-formation experience is cross-tabulated within categories of religious attendance in 1980. Focusing first on the behavior of young people who attended religious services less than once a month in 1980, we see that subsequent union-formation experience had

TABLE 6

CROSS-TABULATION OF 1985 RELIGIOUS ATTENDANCE BY 1980–85 UNION-FORMATION EXPERIENCE, WITHIN CATEGORIES OF 1980 RELIGIOUS ATTENDANCE

1985 Religious Attendance	Neither Cohabited nor Married	Cohabited*	Married Only
A. Attended less than once a month in 1980:			
Less than once a month	87.4	83.3	81.8
Once or a few times a month	7.5	10.9	9.1
Once a week or more	5.2	5.8	9.1
Total	100	100	100
N	174	138	55
B. Attended once or a few times a month in 1980:			
Less than once a month	51.4	67.5	47.5
Once or a few times a month	33.3	30.0	37.5
Once a week or more	15.3	2.5	15.0
Total	100	100	100
N	72	40	40
C. Attended once a week or more in 1980:			
Less than once a month	21.2	53.8	10.3
Once or a few times a month	22.4	20.5	25.0
Once a week or more	56.4	25.6	64.7
Total	100	100	100
N	156	39	68

* Includes all those who cohabited, including those who did and did not subsequently marry.

little influence on their 1985 attendance (table 6, pt. A). Somewhat over four-fifths of all those who had attended religious services less than once a month in 1980 continued such infrequent attendance, a fraction that differed little by union-formation experience. These people already had little involvement in religious services in 1980, and there was minimal possibility for cohabitational experience to diminish that involvement.[15]

Shifting to the more frequent attenders in 1980, we see larger effects of subsequent union-formation experience. Among the cohabitors who attended services once a month or a few times a month in 1980, two-thirds reduced their attendance in 1985 to less than once a month, as compared with only one-half of similar noncohabitors (table 6, pt. B). The effect of cohabitation on those who had attended services at least weekly in 1980 is even greater. More than one-half of these people who later cohabited reduced their attendance to less than once a month—a fraction more than double the reduction for those who remained single or experienced marriage without cohabiting (table 6, pt. C).

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[15] In table 6 we collapse into one category those who had cohabited only and those who had both cohabited and married.

The statistical significance of the interaction between religious participation in 1980 and cohabitational experience was evaluated through two formal tests. The first test added an interaction term between cohabitational experience and 1980 religious participation to the models shown in columns 1 and 3 of table 5, but using the combined male and female samples. This interaction variable was highly significant ($t = 5.7$) and in the direction of showing that the negative effect of cohabitation on religious participation was greatest among those who were originally the most religious. The second approach estimated the bivariate effect of cohabitational experience on 1985 religious participation within the categories of 1980 religious participation shown in table 6. In this analysis there was no effect of cohabitation on the religious participation of those who attended less than once a month in 1980, but there was a strong and statistically significant effect among those who attended once a week or more in 1980 ($t = 5.2$).

Thus, these results show that the major effect of union-formation experience is among the cohabitors who had previously attended services frequently. Many of these people experienced dramatic reductions in their religious attendance, presumably as a result of the decision to cohabit without marriage. For those who were not particularly religious in the beginning, subsequent union-formation experience had little effect.

Since cohabitation only influences the religious participation of those whose original attendance is moderate or high, the potential overall effect of cohabitation is limited. Cohabitors are disproportionately drawn from the infrequent attenders; only 18% of them had attended services once a week or more and 18% had attended either once a month or a few times a month. Consequently, just over one-third of all cohabitors could reduce their religious participation significantly.

This reduction in religious participation by cohabitors who had previously attended often should also be understood in the context of generally declining religious attendance among those 18–23 in the total sample of young people. Whereas about one-third attended services once a week or more at age 18 in 1980, less than one-fourth attended that frequently at age 23 in 1985. Conversely, the fraction attending less than once a month increased from 47% at age 18 to 58% at age 23. Thus, the decision by frequent attenders to cohabit at age 18 substantially reinforced the general trend toward less frequent attendance during this part of the life course.

CONTROLLING THE CHILD'S EDUCATIONAL EXPERIENCE

As a final test of the robustness of our results, we investigated the reciprocal causal effects of religiosity and cohabitation and marriage, controlling the child's own educational experiences. We conducted an analysis predicting cohabitation and marriage, controlling a time-varying covariate of school attendance and the child's school enjoyment, grades, and aspirations in 1980. To facilitate the incorporation of the time-varying covariate, this analysis was conducted using discrete-time, hazard-model procedures. These analyses revealed that the introduction of these education controls does not substantially modify the findings. The coefficients for the religiosity variables are modified only slightly and in the direction of somewhat increasing the estimated effects on marriage and somewhat decreasing the estimated effects on cohabitation (results not shown). These results leave unchanged the central conclusion that the pathway to union formation—through cohabitation or marriage—is strongly influenced by religiosity.

We also conducted an analysis predicting the child's 1985 religious attendance and controlling the child's school grades, enjoyment, and aspirations in 1980. Measures of school accomplishment through 1985 were not included because they could be the effects of cohabitation and marriage as well as their causes. Controlling the child's school grades, enjoyment, and aspirations in 1980 only slightly modified the estimated effects of cohabitation and marriage experience on 1985 religious participation. Furthermore, while some of the estimated effects were weakened slightly by the introduction of these controls, others were strengthened with them (results not shown). These results, therefore, suggest that the effects of union-formation experience on religious participation reported in table 5 are not the result of educational experience causing both union-formation experience and religious participation.

SUMMARY AND CONCLUSIONS

The empirical results of this article provide strong support for the proposition that the cohabitational and marital behavior of young people is influenced by their religious commitment and participation. Low levels of religious importance and participation are related to high rates of cohabitation and low rates of marriage in that less religious young people are much more likely than their more religious peers to cohabit than to marry. People without religious affiliations also opt more for cohabitation and less for marriage than do people who identify with a religious group.

Our data also provide evidence that parental religiosity influences the cohabitational and marital behavior of young people. Although parental religiosity appears to operate most powerfully on children's behavior through its influence on the religious importance and participation of the children, there may be a modest influence that operates independently of the children's religiosity.

It appears that the importance of religion and participation in it are more powerful and pervasive determinants of marriage and cohabitation than religious affiliation. The observed religious-affiliation effects are not consistent for men and women, and there is no obvious interpretation of the differences across gender. The results consistently show that both importance of religion and religious participation are strongly related to union-formation behavior, with no clear indication that one dimension of religiosity is more important than the other.

Our data also confirm theoretical expectations that the interconnections between family and religion are much more complex than is usually assumed by social scientists. In addition to the causal influence of religiosity on cohabitation and marriage, there is also an important reciprocal influence of union-formation patterns on religious participation. This suggests that the decisions of young people concerning their personal and family lives have important ramifications for their religious involvements.

While this article has investigated directly only the interconnections between religion and one dimension of family behavior—union formation—the results have wider implications for the understanding of the religion and family connection. There are good reasons to expect that the model of intergenerational and reciprocal causation between religion and family extends to other dimensions of family life, including divorce, contraception, abortion, and the roles of women. While religious commitment probably influences people's behavior in these domains of life, as has been hypothesized by others, our theoretical model and empirical findings suggest that people who decide to have a divorce, practice contraception, or have an abortion when such behavior is proscribed by their religious organizations may also frequently question their commitments to their religious group. The likely result is that many people who have these experiences diminish their levels of religious participation and commitment, just as the people in this study who cohabited reduced their participation in religious services. We believe that these expectations are compelling enough to merit further investigations into the validity of this reciprocal causation model in other domains of family life.

Finally, the findings here may have important implications for understanding aggregate trends in marriage and cohabitation. Other research has shown that, during the 1960s and 1970s, the importance and centrality of religion to Americans declined as did confidence in religious institutions and authorities (Glenn 1987; Thornton 1989; Roof and Mc-

Kinney 1987; Alwin 1988). During the same years the marriage rate fell dramatically, and cohabitation emerged as a major new life-style (Rodgers and Thornton 1985; Thornton 1988; Bumpass and Sweet 1989). It has been suggested elsewhere that these major aggregate trends may be causally interconnected, with religious changes being both causes and effects of familial trends (Lesthaeghe and Surkyn 1988; Lesthaeghe and Wilson 1986; Thornton 1985, 1989).

Our analysis provides strong support at the micro level for the hypothesis that aggregate religious trends may have been important determinants of national trends in marriage and cohabitation. As the centrality and importance of people's religious commitments declined during this period, there may have been a relaxation of the religious proscriptions against premarital sexual relations and cohabitation. Also, the authority of religious institutions to produce conformity to religious ideas concerning sex, cohabitation, and marriage may have weakened. These changes may have been important determinants of the substitution of cohabitation for marriage, at least temporarily, for large fractions of young Americans, with the resulting decline in the prevalence of marriage and increase in the incidence of cohabitation. Our data indicate that these processes were working strongly among individuals, and it is likely that they were operative at the aggregate level as well. If so, trends in religious commitment and importance may be very helpful in explaining the concurrent trends in many dimensions of family life, including premarital sex, cohabitation, and marriage.

Our micro-level data are also consistent with the hypothesis that aggregate changes in premarital sex, cohabitation, and marriage were important determinants of the accompanying trends in religious participation and commitment. Our research suggests that cohabitation leads toward lower levels of religious involvement while marriage tends to increase religious participation. Aggregating this association to the national level suggests that increasing cohabitation and declining marriage, along with other family trends such as divorce, contraception, and abortion, may have helped to cause a questioning of religious commitments in America in recent decades.

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Book Reviews

Affirmative Action and Justice: A Philosophical and Constitutional Inquiry. By Michel Rosenfeld. New Haven, Conn.: Yale University Press, 1991. Pp. 373. $30.00.

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Michel Rosenfeld provides an insightful critical analysis of liberal political philosophies of justice and equality as they relate to affirmative action. His purpose is to outline a philosophical, and constitutionally acceptable, position that will reestablish the integrity of the American distributive system. The integrity of the distributive system has been lost through the pervasive effects of historically institutionalized racism and sexism. Though race and gender should not have any moral relevance in the allocation of education and employment resources, systematic and prolonged racism and sexism make such group affiliations morally relevant. Using preferential affirmative action to compensate individuals on the basis of group affiliation will restore the integrity of a distributive system based on equal opportunity rights. Distributive and compensatory justice are intertwined; therefore, what appears to be distributive justice is actually compensatory justice when there are glaring group inequalities. Glaring inequality, especially poverty, indicates the pervasiveness of historical first-order discrimination (i.e., discrimination grounded in assumptions of inferiority or difference). Compensation requires distribution given preferential treatment.

Rosenfeld clearly establishes that liberal political theories centered in libertarian, contractarian, utilitarian, and egalitarian forms of justice are unable to support race and gender preferential treatment because they validate first-order discrimination. They also fail to provide a balance in the tension between individual autonomy and societal welfare as they relate to justice, and identity and difference as they relate to equality. Thus, his aim is to elucidate a theory of justice that does not contain these flaws and that adheres to the postulate of equality (i.e., that all individuals are morally equal as individuals and are, therefore, deserving of self-respect, human dignity, and the opportunity to choose and pursue a reasonable life plan). Rosenfeld argues that the postulate of equality is constitutionalized in the equal protection clause but that discrimination has circumvented the equal opportunity rights of blacks and women.

These equal opportunity rights can be established through conceptualizing justice as reversibility. This form of justice requires a dialogical

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process based on a Habermasian theory of communicative action (pp. 258–75) and on Kohlberg's conception of justice as reversibility and the reciprocity of perspectives (pp. 249–54). It requires all participants in the dialogical process to accept the postulate of equality and to be able to assume the perspective of the other participants. This form of justice combines elements of contractarian and utilitarian justice that are consistent with the postulate of equality while balancing individual-versus-community concerns, as well as those of identity versus difference. The perspectives of persons involved in all aspects and outcomes of both discrimination and affirmative action would be considered in determining the validity of affirmative action cases. Though white males who had not actually practiced discrimination might believe they would be treated unequally, they would not be denied their equal opportunities rights; instead, their prospects for success would take place in an arena enlarged by the absence of discrimination. Since the results would be inclusionary, not exclusionary, they would not institute reverse discrimination.

Rosenfeld's conception of justice would require the state to move beyond adherence to negative rights to the establishment of positive rights by replacing antidiscrimination with equal opportunity as the mediating principle of the equal protection clause. Rosenfeld clearly demonstrates the failure of the Supreme Court to establish clear procedures for affirmative action rulings. While the justices agree on compensation for actual victims when specific wrongdoers are identified, they do not agree when cases involve a class of victims and societal discrimination. The justices vary in their support of marginal equality (i.e., all treated as equals) and global equality (i.e., in order to treat some as equals they must be treated differently). The marginal equality position supports the strict scrutiny test in assessing the presence of discrimination. It allows for affirmative action with only narrow compensatory goals. Global equality supports an intermediate scrutiny test that acknowledges distributive justice and societal discrimination. Marginal equality causally links discrimination and victims through an atomistic model that accepts as valid only direct, unbroken causal chains. Global equality links the two through an ecological model that recognizes more complex and context-specific causality. In respect to these differences Rosenfeld states, "From the standpoint of equal opportunity, stubborn adherence to the marginal equality position and to an overly narrow compensatory framework may appear to be but the means employed to insure the preservation of unfair advantages gained through systematic first-order discrimination" (p. 332). He argues that the rulings of the conservative court may signal an abandoning of affirmative action before it has had the opportunity to complete its mission; allowing it to balance the distributive system would eliminate the need for it.

By using equal opportunity as the mediating principle, the dialogical process and reversible reciprocity as the framework for judicial procedure, and the presence of a class of victims who have had their prospects for success limited by pervasive first-order discrimination to determine the constitutionality of affirmative action (Rosenfeld outlines six determining factors; see pp. 332–33), the integrity of the American distributive system can be restored. It is impossible for a review to display the complexity and insightfulness of Rosenfeld's analysis. If it has any substantive weakness, it is that the reader becomes engrossed in the philosophical arguments that seek to define justice and may briefly forget that the central discussion is the justice of affirmative action. Rosenfeld's analysis can inform discussions of sociological theory and provide essential background information for empirical analyses of affirmative action policies.

Law as Metaphor: From Islamic Courts to the Palace of Justice. By June Starr. Albany: State University of New York Press, 1992. Pp. xli+243. $16.95.

Jonathan M. Cohen
University of Chicago

While June Starr's newest book has as its main research question how the development of Turkish courts has resulted from a power struggle between proponents of an Islamic justice system and a secularized Western judicial system, it provides a more broad argument about the evolution of legal systems as metaphorically connected to "group competition over the law of a nation" (p. xviii). Using a combination of empirical ethnographic field research and historical data, Starr has presented a unique view into the ways through which legal systems develop by viewing law as a multilayered set of meanings. It is the struggle among elites to define these meanings that she sees as integral to understanding the formation of the laws of a society.

The author has argued that, because of the unique evolution of the Turkish system in contrast to other modern Middle Eastern Islamic nations, Turkish courts are almost entirely secular. The first section of the book reviews the development of the secular legal system from the 19th century in order to provide a historical context for her ethnographic study of the Turkish courts in the late 1960s. Starr argues that the rise of a secular educational system foreshadowed the rise of the secular court system. She claims that a secular educational system first arose to parallel and supplement the religious educational system, but then later began to rival it. In much the same way, administrative courts were created by nationalistic elites in the mid-19th century and developed until the present highly secularized courts dominated Turkey's formal dispute resolution. Starr's painstaking exactness in her description of the uneven and sporadic stages by which these secular administrative courts became more and more entrenched in the Turkish social structure enables her to vividly portray the broad social tensions present in Turkish society between religious and secular tendencies.

Starr then proceeds to assess the impact of the legal secularization on gender relations and on land ownership. These two chapters are intriguing because Starr argues that changes in the legal structure can change the way in which individuals within the society are able to conduct their lives even at the microlevel. Starr's connection between the secularization of the legal system and gender empowerment is especially subtle. She utilizes historical evidence to establish the effects of changing secularization as well as ethnographic data to display the complexity of the micro-macro link that she supposes. This historical embedding of her ethnographic results is perhaps this book's most striking and positive quality.

Finally, Starr provides an exceptionally well-written analysis of the role of Islam in conflict resolution in late-1960s Turkey. She loosely divides conflict resolution into those resolved through informal means and those resolved in court by judges. Using a number of cases from her fieldwork, Starr identifies the varieties of informal dispute resolution and satisfactorily illustrates that the role of Islam is virtually nonexistent. Similarly, by using a substantial number of observed cases, Starr describes the Turkish judicial decision-making process and asserts that judges are highly secularized to the exclusion of religious motivations. But, Starr states, this complete secularization is certainly more extreme than any of the proponents of secularization would have envisioned or wanted.

In this book, Starr has provided strong evidence for a view of modern legal systems as the result of a set of small and orderly changes that have grown from the interplay of diverse agents with diverse cultural and social backgrounds and agendas. The logic of legal change that Starr presents is far from flawless, but by stressing crisis and historical change arising from shifting power relations rather than stressing an ethnographic assessment of a modern court without historical embeddedness, Starr has asserted what is certainly a profound insight into the nature and consequences of legal change and process.

Bending the Law: The Story of the Dalkon Shield Bankruptcy. By Richard B. Sobol. Chicago: University of Chicago Press, 1991. Pp. xii+408. $29.95.

Teresa A. Sullivan
University of Texas at Austin

This is not a sociology book, and its intended audience is not sociologists. It deserves to be reviewed here as a potential type of data. The book is an excellent case study of how corporate bankruptcy can intervene in confrontations between corporations and other institutions. The bankruptcy of A. H. Robins, the pharmaceutical company located in Richmond, Virginia, was a final attempt to avoid the company's product liability for the Dalkon Shield, a contraceptive device held responsible for injuries to thousands of women.

Several books have detailed the Dalkon Shield scandal: the incomplete testing that preceded its sale, the misleading advertising that persuaded physicians to use it, and the deliberate evasion of responsibility by the company in response to the early product liability lawsuits. Dalkon Shield users suffered pelvic inflammatory disease, uterine injury, septic abortions, and even death. Women who sued their personal physicians or A. H. Robins were, like rape victims, subjected to cross-examination concerning their sexual partners and practices, while Robins's attorneys insinuated alternative causes for pelvic inflammatory disease or septic abortions. What is new in this book is the continuity from this phase of the story to the strategic decision to file for protection under the U.S. Bankruptcy Code.

Sobol smooths some of the procedural tangles so that lay readers can follow the machinations and countermachinations of the lawyers. Arrayed against Robins were numerous plaintiffs' attorneys who coalesced around different positions in various venues. The cast of characters is complex, and readers may need to keep a list of the principal actors and their putative positions in the litigation. Bankruptcy law is sufficiently specialized and complex that a whole new platoon of lawyers, trustees, and others enter the scene after the filing. Sobol offers a fascinating glimpse of how these specialists changed the nature of the Dalkon Shield controversy. The book illustrates well how bankruptcy turns other laws upside down and inside out.

In theory, bankruptcy is a single forum where creditors are on an equal footing. The most important legal change caused by the bankruptcy filing was that the women injured by the Dalkon Shield were transformed from tort victims into unsecured creditors. No one—Judge Robert Merhige, Robins's management, or even Sobol himself—is truly reconciled to this transformation of roles.

Robins, in effect, recognized a class of "real" creditors, who were not the injured women. Robins was not willing to treat its suppliers, distributors, and other commercial partners as merely one more group of creditors; they were the "real" creditors, and, if the business was to continue, the real creditors had to be paid. Caught making preferential payments to some of its commercial creditors, Robins fired its own bankruptcy counsel, claiming that its lawyers had inadequately advised the company. Judge Merhige fined the company $10,000.

Judge Merhige, for his part, seemed occasionally inclined to treat the injured women as a class of supercreditors. Many women injured by the Dalkon Shield were already in their thirties, and they needed expensive surgical reconstruction of their reproductive organs if they were ever to bear children. These costs should have been borne by Robins. Delays in the case, many of them caused by Robins's counsel, could mean that some women would never have their fecundity restored. Judge Merhige sought to create an emergency fund so that fertility-saving medical procedures could be performed before the lengthy litigation ended. His efforts were ultimately struck down by the court of appeals—ironically, as the same sort of preferential treatment of a group of creditors for which Judge Merhige fined Robins.

Sobol seems to fault Judge Merhige for not doing enough to treat the injured women as a class of supercreditors. Sobol casts Judge Merhige in the role of villain, allowing no credit for Merhige's efforts to help the injured women. Sobol's account is full of suspicions, coincidences, and allegations that stop just short of accusing Merhige of colluding with Robins. Sobol has little sympathy for the maintenance of the pharmaceutical company, or for the situation of the other creditors.

My sympathies also lie with the women injured by a poorly tested, faulty contraceptive device. But from my own interviews with bankruptcy judges, I know that they approach a Chapter 11 reorganization with multiple objectives. Many judges have told me that their first concern is to save the jobs of the workers. Bankruptcy judges end up balancing justice to the victims against unemployment for the current work force. Sobol is dissatisfied with Merhige's balancing act.

Perhaps the dissatisfaction should be directed elsewhere, for bankruptcy is a clumsy and inadequate remedy for mass torts. In some countries, Robins's management would have faced criminal indictments. In other countries, their directors' involvement in the bankruptcy would have resulted in heavy fines for the directors and their potential disqualification from future service on corporate boards. Perhaps this book should not lead to the question "What is wrong with the bankruptcy system?" but rather "How do we find the right remedy?"

The Battered Woman and Shelters: The Social Construction of Wife Abuse. By Donileen R. Loseke. Albany: State University of New York, 1992. Pp. vii+216. $14.95.

Laurence E. Lynn, Jr.
University of Chicago

In his classic study, *Street-Level Bureaucracy: Dilemmas of the Individual in Public Services* (New York: Russell Sage Foundation, 1980), Michael Lipsky drew attention to what he called, paraphrasing Peter Berger and Thomas Luckman, "the social construction of a client" (p. 59). Lipsky argued that "the processing of *people* into *clients,* assigning them to categories . . . and treating them in terms of those categories, is a social process" (p. 59).

Lipsky's provocative analysis of actions and decisions by street-level bureaucrats did not, unfortunately, start a flourishing research enterprise. Scholars of social service delivery must cope with literature and data sources that are dispersed, often fugitive, and seldom productive of more than partial insights into service delivery processes and their implications. Loseke's brilliant contribution to that literature should reinvigorate interest in a neglected area of social problems research.

Drawing on data obtained as a participant-observer at a private shelter for battered women, Loseke "shows the process of 'making up people' in human service organizations—the process of transforming heterogeneity into homogeneity, of constructing a type of person out of the raw confusion of practical experience, of reproducing the social collectivities of 'wife abuse' and the 'battered woman'" (p. 5).

The author first analyzes the creation through social and political advocacy of three standardized images or "collective representations": the phenomenon of "wife abuse," the "battered woman," and the battered woman's "shelter." As socially constructed, wife abuse is "severe, frequent, and continuing violence that escalates over time and is unstoppable" (p. 20). A battered woman is "a type of woman who is emotionally confused and therefore unable to define leaving [her abusive husband] as her most reasonable course of action" (p. 27). A battered woman needs a shelter, "an organization offering services without authority structures, rules, professionals, or individual treatment" (p. 34). The social construction of a morally intolerable violence that is "extreme, repeated, unstoppable and consequential" (p.148) is based not on "objective reality" but on advocates' determination to provoke a sympathetic response from a public grown skeptical of an ever-lengthening and morally ambiguous list of social claims for public intervention and support.

The author then describes the "South Coast" shelter for women, a specific organization in a specific place, where "the heterogeneity of lived realities" is transformed by nonprofessional shelter workers into "the homogeneity of social types," which conforms to the socially constructed images on which public support for such shelters is based. "Regardless of complexities," she says, "a particular event becomes one of 'wife abuse' or it does not; a particular woman becomes an instance of the 'battered woman' or she does not" as a result of what goes on at the shelter (p. 54).

In the next three chapters, the heart of her analysis, the author draws on her personal observations, conversations with shelter workers and clients, and the daily journals maintained by shelter workers to show how "worker practical activity in places such as South Coast . . . determines which women will and which women will not be allowed entry and thus become official members of the social collectivity called 'battered woman'; . . . [this activity] shapes some women—rhetorically if not in fact—into a type of person seeable as a battered woman; . . . [it] reproduces collective representations such as 'wife abuse' and the 'battered woman'; . . . [and it] repairs disjunctures between the homogeneity of collective images and the heterogeneity of [the] lived realities" of the women appearing at the shelter's doors (p. 9).

The author shows, for example, how "workers accomplished the social problems work of protecting the organization from evidence challenging the correctness of organizational ideology and form. Hence, they reproduced the belief that shelters are organized and effective for the battered woman. Critically, this was accomplished *not* primarily by modifying collective representations to fit practical experience [but] by bringing experience into line with collective representations" (p. 163). Thus worker practical activity determines the reality of socially constructed images and both produces and reproduces social problems.

Thus those who, in the shelter worker's judgment, failed to fit the image of a battered woman needing support in a shelter were denied entry or sent away and thus were not counted as part of the "wife abuse" problem. If a woman was not "incompetent" or if evidence of having been abused was ambiguous, if she was self-assertive or demanding or resistant to a shelter worker's particular offers of support or to the regimen of the shelter, then she aroused the worker's anger, not sympathy or forgiveness. "Clients can be known," the author argues, "primarily as they are instances of the type of person for whom the organization exists" (p. 114).

"Out of the entire universe of conditions possibly pertaining," the author says in her concluding chapter, "only a small portion come to public attention and become the object of public concern." Her lucid, intellectually rigorous, and utterly fascinating analysis of the social response to wife abuse can serve as a model for empirical research into this wider universe of social responses to troublesome conditions.

Formations of Violence: The Narrative of the Body and Political Terror in Northern Ireland. By Allen Feldman. Chicago: University of Chicago Press, 1991. Pp. xii+319. $48.00 (cloth); $17.95 (paper).

Tanya M. Cassidy
University of Chicago

Since the summer of 1969, Northern Ireland has been plagued by sporadic, politically motivated violence. This violence continues: 26 people have been killed in the first five weeks of 1992. Nationalist (those seeking a united Ireland) and loyalist (those wishing to remain in the United Kingdom) paramilitary groups and the state have all contributed to this body count. Over the years, many social researchers have attempted to explain how the violence is symptomatic of some deeper problem. Feldman, however, offers a unique analysis that concentrates on the violence itself. He conducts an intelligent, forceful, and, sometimes, graphic analysis of the genetic history of violence as it pertains to political agency. Feldman (p. 2) explores "the material contexts within which the politicized body, violence, and oral history emerge as artifacts and instruments of agency."

Feldman has organized his discussion of violence in Northern Ireland around decreasing dimensions of political space, beginning with the open space associated with Belfast neighborhoods and the start of "the

Troubles," and ending with the closed space of prisons and the bodies of the 1981 hunger strikers.

"The Troubles," as the pattern of intercommunal violence is commonly called within Ireland, have plagued Northern Ireland since the late 1960s. Feldman talks about how this violence is interconnected with the trend toward complete community segregation (including the creation of physical barriers where none previously existed). This relocation of people, which took place mainly in 1969–70, was one of the largest movements of civilian populations in postwar Europe. He argues that the resulting physical transformation of Belfast has itself contributed to the cycle of violence (most of the intergroup clashes occur along the boundaries of these ethnic enclaves).

In the years leading up to the hunger strike, the physical space associated with much of the political action decreased, as attention swung toward internment (without trial), torture in police custody, and the criminalization of political prisoners. He argues that, as a result of this process, the individual's own body becomes more central to political action (it being the only thing over which a prisoner retains some measure of control). Out of this were born the Blanket Protest, the Dirty Campaign, and the 1981 hunger strike, culminating in the death of 10 individuals (all nationalist paramilitary prisoners).

He argues that the hunger strike achieved little except its contribution to the continuation of political violence. I do not agree with this conclusion, however. In the wake of the hunger strike, the New Ireland Forum was set up in Dublin in May 1983. This body heard testimony on the political future of the North from all interested individuals and groups. Its final report was rejected outright by Prime Minister Thatcher but led, indirectly, to the Anglo-Irish Accord of 1985. The Republic of Ireland has traditionally played the role of advocate for the rights of the nationalist minority in the North, and this role was formalized by this accord. Feldman's book ignores any role that the Republic of Ireland and individuals in the republic play in the affairs of the North. This omission is consistent with his concentration on organized group violence to which the republic does not significantly contribute; however, culturally, ideologically, and politically, the people and government of the republic represent a significant piece of the political puzzle that is Northern Ireland.

Despite this omission, this well-written book is an excellent introduction to this ongoing tragedy. In addition, people interested in the historical and cultural analysis of violence, as well as researchers of semiotics and discourse analysis will find this text invaluable.

Point Blank: Guns and Violence in America. By Gary Kleck. Hawthorne, N.Y.: Aldine De Gruyter, 1991. Pp. xvi+512. $59.95.

H. Laurence Ross
University of New Mexico

Point Blank endorses two general propositions: that gun ownership does not significantly increase the amount of death and violence in a society and that legal control efforts are largely incapable of reducing either gun ownership or violence. In detail, Kleck attacks the view—held by numerous reputable scholars as well as liberal social activists—that when firearms are easily available their use renders social conflict more deadly. This view also encompasses the idea that easily available firearms facilitate suicide and accidental deaths. Kleck shows how efforts to reduce the availability of firearms through controls on ownership, carrying, and use run into a variety of problems. The most serious of these is the nonconforming and deviant character of the people who most frequently use guns in aggressive or careless ways; the most fundamental problem is the enormous number of guns and gun owners already in place, which results in an interest group opposed to all but the narrowest of control legislation. If total prohibition of civilian firearms ownership is political pie in the sky, more limited prohibitions run the risk of favoring substitution of even more dangerous weapons for classes that are banned.

Kleck's arguments are supported with a review and presentation of an enormous amount of empirical research. Citations and data are ranged in copious pages of supplements and appendixes, a fact that has made the volume costly to produce and expensive to purchase. However, if only as a resource concerning the gun control literature, this book is a necessary acquisition for all social science library collections and for any serious scholar working in the area.

When I consider how little information and scholarship exists in my own-substantive area, the drunk driving problem, I am green with envy of those who study gun control. However, the profusion of data does not necessarily illuminate. Much of Kleck's analysis is perforce devoted to explaining the thicket of conflicting results surrounding such questions as the balance between stimulating and deterrent functions of gun ownership in violent crime, suicide, and accidents. The empirical effect of the wide variety of gun control laws is also a matter of considerable ambiguity.

Kleck does the gun control policy debate a great service in demonstrating the complexity of issues that too often are discussed in simplistic ways in the political arena. He sketches a problem of considerably reduced dimensions compared with its size in conventional discussion—for example, fewer than 1% of all guns, and fewer than 2% even of handguns, will ever be used in a violent crime; more people are killed in swimming pool accidents than in firearms accidents. He clarifies the difficulties involved in speaking of "good" hunting rifles, which are more lethal than "bad" handguns. He shows that the consequences of firearms use vary in different crimes and at different stages of their perpetration.

Kleck does not deny that gun ownership has costs and constitutes a social problem, and he does not reject the effort to use law to control these costs. He does, properly, reject the search for silver bullet remedies, including the mandatory imposition of severe penalties for carrying and using firearms (a situation that brings to my mind the image of MADD members trying to stuff 2 million drunk drivers annually into the nation's overburdened jails). He believes that an instant criminal-records check on transfers, including those in the private market, is politically feasible, affordable, and likely to have measurable impact on the availability of guns to those most likely to use them for illegal and immoral ends.

But despite the masses of data and the cleverness of his analysis and argument, Kleck's policy position does not satisfy me. In my mind, he seems too easily to embrace a society based on an internal as well as an external balance of terror. The social order is seen to rest adequately on masses of potential victims using the threat of gun violence to deter masses of potential armed criminals. If this depiction of the view across Rock Creek is unfortunately not too exaggerated, the spectacle is one that ought to disgust rather than cheer the civilized observer.

Kleck sees the gun control controversy in part as a conflict of cultures: bedrock America versus cosmopolitan America, in the language of *Time* magazine. Bedrock prevails politically because its adherents feel deeply about the issue and are well organized. They will allow only the narrowest and most easily evaded restrictions on gun ownership, carrying, and use. The arguably larger (as measured in the polls) minions of cosmopolitan America can enact and enforce the broad control measures that have a greater chance of impact only with similar commitment. On the background of routine bloodshed in our cities, occasional outrages such as the fate of James Brady can provide impetus for attempts at broader gun control laws, but perhaps we will need more incidents, more heinous ones with more tragic or more important victims, to develop the necessary determination.

Layered Violence: The Detroit Rioters of 1943. Dominic J. Capeci, Jr., and Martha Wilkerson. Jackson: University Press of Mississippi, 1991. Pp. xvii+329. $32.50.

Benjamin D. Singer
University of Western Ontario

Much riot research is in situ—instant research. Memories, perceptions, and events are fresh, even if unreliable. However, with the perspective of time, reanalysis of raw data and of earlier research may reveal new kinds of information and generate new insights. That is precisely what the authors had in mind—to do a retrospective examination of the events surrounding the Detroit riot of June 1943 and to focus upon the sociological profiles of arrestees. Rather than analyzing the collective dynamics of the riot per se (a frequent approach to riot research), Capeci, a historian, and Wilkerson, a sociologist, have returned to the event nearly five decades later to seek new explanations by doing highly detailed explorations of the arrestees.

They examined a subsample of "arrest tickets" of 246 persons charged with riot offenses and obtained information from police, court, and probation records, oral histories, unpublished manuscripts, published and unpublished research reports dealing with the 1943 riot and other riots, and reports in the mass media. In the first of seven chapters, "Riot and Reconstruction," the riot activities and background events are described, along with such contextual factors as the explosive growth of the population due to the war industries in the city, the increase in interracial incidents, discrimination and intolerable living conditions for blacks and some whites, their competition with each other for jobs and living space, and the immediate precipitants. The authors display thorough knowledge of the city, including its landmarks, and the geography of the riot. They provide detailed maps of riot zones and of the residence tracts of white and black rioters. Through detailed examination of case histories, they identify rioters as protestors, celebrities, hoodlums, unthinking participants, and victims, but most of all as "ordinary citizens." In this chapter and in chapter 2, "Of Hoodlums and Hillbillies," they dismiss the notion that rioters were riffraff, and with that the implication that riots are the action of deviants rather than episodes that draw in normal citizens with legitimate and serious complaints against the social order.

Chapter 3, "Faces in the Crowd," focuses upon the detailed case histories of a number of black and white arrestees—looters, spectators, and killers—and provides sociodemographic information on different types of rioters. The main point of the chapter is that the riot is a medium through which many different kinds of motivations and circumstances are interlaid—competition, self-gain, revenge, politics. Chapter 4, "And Victims, Too," carries out the diversity theme, describing the heterogeneity of the victims. In chapter 5, "Causes Célèbres," detailed attention is paid to the cases of individuals who were apparently railroaded for riot crimes they did not commit and of the struggles to gain justice for them.

Chapter 6, "From Hastings to 12th Street," compares the 1943 disturbance with the riot of 1967, and describes similarities. In both cases, people were on the streets because of the intense heat; there had been local interracial incidents for some time; the black community was under stress because of its economic state and its living conditions; there were rising expectations by blacks; and as immediate precipitants, there were rumors of atrocities committed against innocent blacks. Quite correctly, Capeci and Wilkerson saw 1943 as more of an interracial fight, while the 1967 rioters "generally protested an abusive system . . . [and] completed the transition from communal to commodity violence begun by their forebears; less interested in interracial combat, they fought more along class lines" (p. 165). The concluding chapter, "Broader Perspectives," attempts to employ data derived from the investigation of 1943 rioters as a frame by which to examine other collective racial incidents, both in the past and on through the Bensonhurst, New York, disturbance of 1989. Buttressed by a scant survey of theory, from Le Bon through contemporary perspectives by social scientists, and of official inquiries, this section is difficult to follow; some of it repeats earlier material, and some—particularly the survey of theory and the discussion of earlier riots—would have been better placed in the first two chapters.

What is best about the book are the extensive detail in the discussions of rioter profiles and the impression that the authors really got to know the riot locale—as if they had walked the streets where battles were pitched and people died to reconstruct the meaning of the events. Yet the book has significant flaws. The most important is the authors' habit of imputing motives and emotional states to the riot participants. They describe white participants with weapons as exposing "perennial bloodthirsty motives" (p. 55), as "bloodthirsty," or guilty of "the emotional appeal of bloodlust" (p. 119). Writing of a black man arrested after throwing a brick through a drugstore window: "Rather than destroy his own dwelling, however, he selected a more practical target. . . . His possession of the blade indicated fear and possibly Southern tradition" (p. 11). The unwary reader may accept the many "perhaps" explanations that sometimes stretch credibility. Explaining why police threw a black soldier to the ground, the authors argue, "Perhaps Lewis's military dress, which symbolized black status and respectability, infuriated the police, who received little respect in the ghetto" (pp. 112–13). Editing leaves much to be desired, for expressions like "the largest majority of rioters" (p. 193) are not infrequent.

These problems are not fatal. Despite them, I recommend the book as a valuable resource for programs and libraries in collective behavior, race, and urban studies.

Emotions and Violence: Shame and Rage in Destructive Conflicts. By Thomas J. Scheff and Suzanne M. Retzinger. Lexington, Mass.: Lexington Books, 1991. Pp. xxiv+208. $35.00.

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A theory to explain all human violence is proposed boldly by Thomas J. Scheff and Suzanne M. Retzinger. In a "micro-macro" theory of social action, they assert that the key concepts of shame and alienation, pride and social attunement apply to intergroup and international relations as well as to interpersonal dynamics. In their view, the instinctive human relationship is cognitive and emotional attunement between persons or between groups. This attunement brings pride, trust, mutual identification, and clear, respectful communication between parties. Each partner understands and ratifies the other person's thoughts, feelings, actions, intentions, and character. The opposite extreme is human disconnection, of which shame and alienation are the individual and social aspects, respectively. Disconnection is marked by a lack of mutual understanding and cooperation and by silent withdrawal or destructive communication, such as sarcasm, blame, or threat.

Scheff and Retzinger hypothesize a causal sequence from shame to rage to violence when shame is not acknowledged openly by all present. Acknowledgment allows social bonds to be repaired, ending shame. Traditional societies acknowledge shame, but modern societies repress or disguise the shame and alienation.

The authors draw from many sources in a comprehensive, well-written literature review and theory development. They were influenced strongly by psychologist Helen Block Lewis's research on infant-caretaker relations and psychotherapy sessions. Lewis saw the social bond as a continual process, the current state of which was signaled by shame and pride.

In a tour de force of multimethod research, Scheff and Retzinger examine television programs, marital counseling, conversations, and biographical and literary data. Dramatic photographs reveal the surprise and shame of hoax victims on television's "Candid Camera" and portray the emotions of game-show contestants winning or losing prizes. Scheff and Retzinger also explore hidden shame-rage in Goethe's novel, *The Sorrows of Young Werther,* and present a labeling theory based on studies of psychotherapy sessions. Their analysis of discourse and nonverbal behavior shows how unacknowledged shame leads married couples to an angry escalation of quarrels.

A fascinating chapter on Hitler reveals broader theoretical problems. They explain Hitler's appeal to the German people in terms of his own unacknowledged fury at his father. "Unconscious vengefulness motivated Hitler and connected him to his followers" (p. 142). Germans felt stigmatized by the harsh, humiliating terms of surrender, but Hitler offered them a folk community, ending their disconnectedness, and he encouraged them to express their humiliated fury. Is it not reductionistic to explain the social fact of war in terms of a psychological motive such as the humiliation-rage sequence? Participants in a social movement such as Nazism have different motives—not always humiliation, but instead, perhaps, material gain, conformity, or idealism. The therapeutic concept of open acknowledgment seems irrelevant on the international scale. Would Nazism have been prevented if all nations had acknowledged openly that defeated Germany had been shamed? It is more plausible that inflation, anti-Semitism, a militaristic tradition, and other political and economic forces would have produced fascism and war, regardless of the emotional dynamics of individual Germans.

Scheff and Retzinger's humiliation-rage cycle resembles the older, reductionist frustration-aggression and authoritarian personality theories once accepted as explanations of fascism, racism, and war. All these theories underestimate response variability. Frustration does not always lead to aggression, but can elicit alternatives such as increased striving, passive resignation, or self-blame. Every society's history has episodes of humiliation, perhaps "unacknowledged," but fascism and war do not occur invariably. My own parents were German refugees and I heard how the victors' blockade of Germany had continued in peacetime, causing widespread famine, including my grandmother's death. But we fled, while other relatives stayed in Germany and opposed Nazism. Emotions are profound experiences, but people do not express them identically through any behavioral response cycle.

How can we study an unconscious motive and know whether people are in attunement or alienation? Scheff and Retzinger concede: "Most of the alienation and shame that our approach detects are not part of the subjects' conscious awareness" (p. 33). To study unconscious dispositions empirically, the authors present microanalyses of emotional interaction. For example, "the clearest sign of pride is holding up one's head, looking others in the eye," but not staring (p.147). Evidence of shame is in avoiding eye contact, hiding, and leaning apart. But these empirical indicators are useless when studying larger groups or nations.

This is the most important sociological analysis of shame since Helen M. Lynd's *On Shame and the Search for Identity* (New York: Harcourt Brace, 1958), but it will appeal most to readers who are sympathetic to concepts of unconscious motivation and therapeutic expression. The attunement that the authors regard as the ideal bond is typical of intimate relationships, not our many instrumental and aggregate interactions. Scheff and Retzinger's analysis is more persuasive at the micro, dyadic level than as a macrogroup, societal, or historical theory.

On the Mersey Beat: Policing Liverpool between the Wars. By Mike Brogden. New York: Oxford University Press, 1991. Pp. 184. $29.95.

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When first used in the discipline of history in the 1960s, the oral history method aroused a great deal of criticism among orthodox historians, who preferred archival sources. But in sociology, the method merged with long-established and respected methods, like the in-depth interview and the life-history technique, and is still very popular. It also fitted what Peter Berger once called sociology's debunking and unrespectability motifs, its intention to go beyond official versions and to focus on groups at the margins of mainstream industrial society: the "hip outsiders" as Goffman once referred to them rather than the "dull insiders." By looking at events from the vantage point of those at the bottom of society, oral history provides us with the accounts of people who do not normally figure in the official documents and records—the soldiers rather than the generals, the workers rather than the bosses, the followers rather than the leaders.

The method therefore has only very rarely been used to study elite groups or those connected in some way with the state. Mike Brogden's book is one example of just such work. It records the accounts of 24 policemen in Liverpool between the two world wars. He is wrong to claim (p. 1) that it is the only oral history study of policing, but certainly there are not very many. Policing in Liverpool has intrinsic interest. For a long time it was the largest and most expensive police force outside London, it was involved in several industrial disputes and sectarian clashes, inherited from nearby Belfast, as well as race riots. A police union was also briefly formed there, and the police went on strike in 1919. These were momentous events both for themselves and the development of modern policing in the United Kingdom.

But one of the most enduring features of everyday life is its routine even in extraordinary circumstances, and ordinary people experience as mundane the great events in which they are involved. Brogden's respondents are no different, and they recall the day-to-day, humdrum, and routine nature of their work. This fact provides an interesting counterweight to the emphasis given, on both sides of the Atlantic, to a return to community and neighborhood policing. Police work in Liverpool was the epitome of beat policing, and Brogden shows that its practitioners saw it as the ultimate in disciplined, supervised, timetabled, boring work. In this sense the police were, as Brogden stresses, part of the oppressed; as tightly controlled and regimented by the police bureaucracy as the people they policed.

One of the other paradoxes explored in *On the Mersey Beat* is the nature of social control in the city. People were not so much regimented by law but by middle-class notions of respectability. The main function of policing at the time was to keep Liverpool's streets clean of the poor and the unemployed, and the drunks, prostitutes, gamblers, and urchins whose work site or playground was the streets. Policing primarily involved "moving on" people, and arrests for other than minor misdemeanors were rare. But this job of cleaning the streets of the working classes was done by men (there were no policewomen at the time) whose origins were themselves thoroughly working class. The state dealt with this problem in two ways. First, it locked policemen into a bureaucratic system of control and regulation. But it also stipulated that policemen could not be from the district, even the city, in which they worked, thus continuing the colonial policing practice of "strangers policing strangers."

Writing oral history can be difficult because of the dilemma associated with all qualitative research between description and analysis, between allowing the respondents to speak for themselves, and interpreting and commenting on what is said. After a few introductory remarks, Brogden allows the policemen uninterrupted opportunity to speak for themselves, except for an occasional comment of his own, which is placed in italics. For some this will be a strength of the book, for others, a weakness. I see it as a strength, but I would have liked a conclusion in which the central themes mentioned at the beginning were brought together again.

The Bourgeois Epoch: Marx and Engels on Britain, France, and Germany. By Richard F. Hamilton. Chapel Hill: University of North Carolina Press, 1991. Pp. xii+293. $37.50 (cloth); $13.50 (paper).

Geoff Eley
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This is a very odd book. Its aim is to test the adequacy of Marxism as a social theory by considering its "claims about class and class dynamics" in the specific context of British, French, and German history (p. 2). Yet Hamilton elects a very peculiar strategy. He takes the scattered journalistic fragments Marx and Engels devoted to British history, one of Marx's substantial contemporary commentaries on mid-19th-century events in France (*The Class Struggles in France,* written in 1850), and four of Engels's writings about Germany between 1847 and 1874 (though not, curiously, his rather interesting unfinished addition to "The Force Theory" chapters of the *Anti-Dühring* written in 1887–88, which was eventually published as *The Role of Force in History: A Study of Bismarck's Policy of Blood and Iron* [London: Lawrence & Wishart, 1968]), and treats them as though they were works of historical scholarship. As such he finds them seriously wanting, both on grounds of their internal logic and consistency of argument and on grounds of their inadequate empirical research. How surprising, one might respond. And this book is marked above all by the stunning discrepancy between the grandiosity of the author's claims and the trivializing polemic of the delivery.

In each of the three main country chapters, Hamilton's procedure is basically the same. He presents Marx's and Engels's own claims concerning the disposition of classes and their agency in politics, lifted with descriptive literalism from the small number of immediate texts to which he confines his attention; he comments on their internal inconsistencies; and he measures them against an unreflected and capricious selection of historical works of vastly differing provenance and status. The form of the chapters varies somewhat. The one on Britain is constructed mainly through the writings of a few salient historians, including Christopher Hill and Laurence Stone, due to the actual paucity of substantial writings by Marx and Engels on either the 17th or the 19th centuries of British history. The French chapter, on the other hand, is a blow-by-blow account of the single text of *The Class Struggles in France,* drawing here and there on historical literature, some old and some new. The German chapter omits historical and historiographical contextualization almost completely, focusing entirely on Engels's own writings, though stopping short of the full range of what he had to say. The book ends with a class-by-class commentary on the role of social groups in the 1848 revolutions (bourgeoisie, petty bourgeoisie, workers, farmers, intellectuals), a jejune dismissal of Marx's claims for class determination, and a brief homily on the lessons of history.

The book is a curious throwback to an older anti-Marxist genre of the 1950s. It selects the narrowest and most uninteresting ground on which to judge the usefulness of Marxism as an approach to history. For instance, either one could proceed more abstractly, by setting the political journalism preferred by Hamilton with the concurrent works of formal theory and the critical apparatus developed by the sympathetic Marxology in order to construct a more sophisticated and serviceable theory of class, or one could treat the texts used by Hamilton in a more thoroughgoing way for what they were, in order to historicize Marx and Engels more securely in the political discourse of their own time. In this latter respect, the political journalism is much less interesting for what it can tell us about the specific events in which it was embedded than for the political perspectives Marx and Engels elaborated between the early 1850s and mid-1860s, which formed the basic programmatic framework for the European socialist parties when they began to appear in the 1870s. Thus Hamilton refers neither to Richard N. Hunt's two-volume study, *The Political Ideas of Marx and Engels* (London: Macmillan, 1975), nor to the essays of Gareth Stedman Jones, nor to Roman Szporluk's fascinating *Communism and Nationalism: Karl Marx versus Friedrich List* (New York: Oxford University Press, 1988). From the former, more abstract-theoretical point of view, Hamilton falls short of the most sophisticated Marx scholarship of the 1970s, including the essays of Stuart Hall, which he does not cite (e.g., "The 'Political' and the 'Economic' in Marx's Theory of Classes" [*Class and Class Structure*, edited by Alan Hunt, pp. 15–60, London: Lawrence and Wishart, 1977], which argues from *The Class Struggles in France,* and "Re-Thinking the 'Base-and-Superstructure' Metaphor" [*Class, Hegemony and Party,* edited by Jon Bloomfield, pp. 43–72, London: Lawrence and Wishart, 1977]).

Moreover, there are flourishing debates involving Marxists and non-Marxists around two of Hamilton's chosen subjects—the "great arch" of British political development between the 15th and 20th centuries, and the peculiarities of Germany's "revolution from above" in the 19th century—which allow the concept of "bourgeois revolution" to be reengaged. But although relevant works make occasional appearances in Hamilton's footnotes, this particular context of theory and history is largely ignored. Shockingly, Hamilton's cursory glance toward Belgium as "a fourth case" (pp. 46–48) fails to mention Samuel Clark's thoughtful article, "Nobility, Bourgeoisie and the Industrial Revolution in Belgium" (*Past and Present,* no. 105 [November 1984], pp. 140–75), and this neglect is unfortunately typical of the spottiness with which the given literatures are critically integrated into the discussion. Neither historians seeking fresh analytical perspectives on familiar material nor sociologists looking for historically informed critique will be satisfied by the weakly contextualized commentaries the book actually provides.

Finally, this book is poorly organized, with an overgrown endnote apparatus that bears a loose relationship to the narrowly conceived parameters of the text. The undisciplined digressions in the endnotes (many of them about Nazi Germany) are no compensation for the book's lack of convincing theoretical and historical ambition.

The Rise of Professional Society: England since 1880. By Harold Perkin. New York: Routledge, 1989. Pp. xvi+604.

Chan-ung Park
University of Chicago

One of the most important changes in the sociology of professions in recent years has been its new efforts to apply a historical perspective. A historical approach provides a framework that explains social and historical variations in the nature of such key concepts as profession, professionalization, knowledge, profession-market or profession-state relationship by the variations in social interactions among professions, other occupations, the state, and the public. Such an approach rejects any theoretical positions that can claim their validity beyond the tests of time and place, whether they are oriented toward social function, the market, or power. It also expands a narrow profession-centered framework to alternative state-centered, market-centered, and public-centered ones, which promotes active interactions between the sociology of professions and other disciplines such as the sociology of class or state.

Harold Perkin's ambitious book is an example of how an excellent historical analysis can enhance our understanding of professions and professionalism not just as occupational characteristics, but as a fundamental principle of social organization. His main thesis is that the post-Victorian British society experienced a social transformation, from class society to professional society, whose sweeping social impacts are equivalent to those of the Industrial Revolution. He defines professional society, distinct from class society, as follows: "A professional society is one structured around career hierarchies rather than classes, one in which people find their place according to trained expertise and the service they provide rather than the possession or lack of inherited wealth or acquired capital" (p. 359).

It is tempting to dismiss Perkin's thesis as another variation of the new class theory in which experts are viewed as new ruling elites. The distinctiveness of Perkin's work, however, lies in its impressive power to demonstrate how a professional social ideal, a moral principle of social distribution based on social efficiency and expertise, permeated a whole range of social spheres from everyday life to politics. The central aspects of professional society are, first, a shift of a social ideal from class-based social ideals of landed aristocrats, capitalists, and workers to a career-hierarchy-based professional social ideal and, second, the professionalization of occupations. Perkin does not deny that classes still do exist in professional society. He argues, however, that all classes should speak the language of professions to justify their claims for the state resources by the criteria of the professional social ideal, that is, efficiency and expertise.

Other major dimensions of professional society are as follows: (1) Vertical structures and horizontal rivalries of professional career hierarchies replaced horizontal structures and vertical antagonism of classes. (2) Corporatism, "the tacit *entente* between the employers, the trade unions and the state" (p. 286), has evolved as an institutional framework of professional society in the forms of corporate economy, corporate state, and corporate society itself where professionalized representatives with their own agendas control the system. (3) Professional society involved the rise of the welfare state in which public and nonprofit sector professions (e.g., civil servants and welfare professions) increased their control over the state resources as the administrators of welfare policies. (4) Along with the rise of human capital, professional society witnessed a shift in the concept of property from an absolute property, which recognizes an owner's unconditional control over properties, to a contingent property, which recognizes only "property rights contingent upon the performance of some justifying service" (p. 123).

Contemporary British society, Perkin argues, was fundamentally professionalized to the extent to which recent reactions against the state-supported professions and attempts to privatize the state intervention (e.g., Thatcherism) are not efforts to reject professionalism itself and to go back to the Victorian society. It is an attack of the private sector professions and their ideology—"equality of opportunity" (p. 443)—on the public sector professions and their ideology—"equality of outcome" (p. 443)—for more control of the state resources. The "equality of opportunity" argument claims the distribution of the state resources according to merit, whereas the "equality of outcome" claims the distribution according to need.

Paradoxically, Perkin's attempt on a comprehensive analysis of professional society is marred by the limited scope of professions he examines. The author derives a professional social ideal from a special segment of professions, that is, corporate managers, union leaders, civil servants, and academics, without examining traditional professions such as lawyers or doctors. As the author claims, traditional professions and their solo practitioners have numerically decreased. It is, however, still important to examine a wide range of professions to derive their social ideal before its permeation into other classes can be discussed. What if the professional class itself suffers from class fragmentation as much as other classes? In his study of the 19th-century British professions (*Professional Men* [New York: Basic Books, 1966]), W. J. Reader demonstrates how strongly the legal and medical professions were fragmented by the social classes of their members. Unless the author shows that traditional professions themselves overcame class fragmentation by the class-neutral ideal of efficiency and selection by merit, the rise of professional society is still an assertion.

The Rise of the Therapeutic State. By Andrew J. Polsky. Princeton, N.J.: Princeton University Press, 1991. Pp. 287.

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University of Lund

As a non-American, I am at a loss to critically review the historical case material of this important book: the rise of the philanthropic movement, its ascent to state power through "science-as-ideology," its injection of middle-class values into American social life, and its persistent tenacity today, despite federal funding cuts, as a disorganized "therapeutic state." This latter state of affairs, says A. J. Polsky, is particularly serious, as it is difficult to locate it as a subject for proper analysis. It must be done, however, as it poses a threat to modern democracy and modern citizenship. Polsky follows the path of Foucault and Donzelot: the rise of the therapeutic state is the rise of an autonomous power system with myriads of small power techniques out of real human control. Against the therapeutic state Polsky visualizes a political one in which human agency is brought back. The sociological project of the last century is really a corollary to the therapeutic one and is thus incriminated. Did the rise of sociology—as an expert science of social integration—impede the political self-organization of human actors?

But as an outside reader I have the benefit of another historical process to draw on, and I can qualify Polsky's thesis that the therapeutic state is a menace to democracy and to political organization among the poor and marginal people in general. The Scandinavian welfare state was from its very inception political in the sense that Polsky suggests was missing in the United States; here, the working class organized itself as a political force and launched social rights on a large scale. However, the political road to modernity, as in Sweden, did not strengthen "individual rights" more than its therapeutic counterpart in the United States. On the contrary. But Polsky probably has in mind a different route to modernity than the one offered by European welfare states, and toward the end I shall have occasion to question some of his (analytical) assumptions.

Polsky's analysis covers the philanthropic movement from its beginning as a charity organization, primarily among middle-class women seeking to target other (working-class) women to better fulfill their homely virtues, up to the present disarrayed state with therapists claiming a special professional service market, specializing in child abuse and in problems among the urban poor. In the Progressive Era, philanthropy took hold of institutions and invaded the university social science programs; the "behavioral sciences" squeezed out social philosophy (not the least from sociology) and helped translate the original social question into a set of segmentalized "social policy questions" without any necessary inner coherence. Most important, a cadre of "social experts" emerged from the newly established schools of social work. The *moralizing* efforts of the early philanthropists were then translated into the *normalizing* interventions of large-scale experimental programs to improve upon the social character of (marginal) individuals. A contradictory flaw in the therapeutic project then surfaced, and it has since then augmented. What perhaps had worked in an individual case—or at least was less harmful—failed utterly when promoted into a large-scale program. There are no scientifically secured indications whatsoever that "human service programs" have beneficient results, or achieve their aim to improve upon social life conditions, especially among those who are the worst off.

At the bottom of Polsky's presentation lies the assumed contradiction between two sets of rights: individual, formal legal rights as against social (collective) rights. In order to state his case, the author relates the history of the juvenile court system and the single mother's aids program in the United States. Progressivist judges made bargains with youngsters: if they agreed to "social treatment," they were let free of felony charges. If single mothers agreed to conduct themselves properly, they were given aid. The new treatment program (guided by psychoanalysis or behavioral science postulates) meant prolonged surveillance and had different and more profound implications on the lives of individuals than the occasional "visits" of charity personnel in a previous era.

Polsky's presentation is much enriched by historical nuances. Local politicians, dependent as they are on electoral votes, here and there resisted the therapeutic call, and local efforts to organize marginal people did provide alternatives, although they often failed to survive. The drifts and fashions over time in public opinion created problems, not the least in financing. The more orthodox members of the legal profession, and strong defense attorneys in particular, have mounted resistance against the bargaining of the due process of law. In the wake of the New Deal, and later in Johnson's Great Society, the therapeutic efforts experienced a boom. Today the changing political climate has disarmed state power and the ideological currents have shifted toward "greater personal responsibility," not the least among the group of previous "clients." Other policy intellectuals, the economists and management experts, have taken the seats of power left vacated by social experts (and sociologists). But it is curious to note—with Polsky—how tenacious the therapeutic establishment really is. Today, therapists dwell in a new and informal market, and the casework methods of their individual practices are in fact promoted by their having left the formal bureaucratic control of the state apparatus. The new market approach to social services has generated a myriad of agencies and programs, and it is this disorganized and diffuse power system that Polsky deplores. It poses a threat, he says, both to democracy and to political citizenship.

A critical point, not developed analytically but implicitly assumed, is Polsky's antithesis between individual (formal legal) and social (collective material) rights. Already Max Weber has warned that there is an incompatibility between "lawyer's law" and "laymen's law," between formal and material rights. In the Continental legal theory tradition of today (I am thinking of Günther Teubner, and more recently Jürgen Habermas and Klaus Günther) there are ways of dealing with this rupture without upsetting the very route of modernity. But Polsky turns rather to the Foucault tradition, and he pays the price for so doing; he sells out modernity.

It is a real merit of Polsky's book to bring the complex argument on the contradictory nature of modern rights into Anglo-Saxon social theory. Nevertheless, his concluding beliefs that a "political" rather than therapeutic path (as in the United States) will secure individual rights seems to me questionable. Polsky is a "communitarian" and not a welfare theorist; he believes that a community could organize itself from within and further its interest on a grass-roots level. This imagery seems to me as promissory as ever that of the philanthropic project. It is doubtful that individual rights can at all be guaranteed in such an imagined communal setting. Weber left us with the Catch 22 argument here; the individual and the modern (bureaucratic) state is the dual result of the modern body politic!

The Quest for Authority and Honor in the American Professions, 1750–1900. By Samuel Haber. Chicago: University of Chicago Press, 1991. Pp. xiv+478. $39.95.

Calvin Morrill
University of Arizona

"The now familiar depiction of the growth of the professions as a movement toward economic monopoly or social modernization is clearly too limiting" (p. ix) writes Samuel Haber in the opening pages of his tour de force history of the American professions from the mid-eighteenth century to the twentieth century. With these words and their accompanying footnotes, Haber eschews what he labels the "systematic analyses" offered in Marxist and Weberian accounts of the rise of the modern professions. He admits to having learned a great deal from more recent sociological analyses of the professions, but he insists that many such arguments are not particular to the professions and so jettisons them as well. Haber distrusts these frameworks as much because of their concentration on single explanatory factors as for their emphasis on contemporary affairs (rather than the past) as the key to understanding the present.

This is a mistake, according to the author, for two reasons. First, it overlooks the ways that historically situated individuals and groups understand their own behavior and, in one sense, make their own history. Second, it overlooks the "secret" of the professions: the smuggling of the premodern into a modern world.

The premodern *in* modern professions is the claim to a particular lifestyle of honor and authority once granted only to an ascribed aristocracy. In premodern society, aristocrats could make unique claims on the ability to direct and command in the society. They also lived as "gentlemen" above common "men," just as humankind was thought to live "above" the rest of the animal kingdom. In the second half of the 18th century, the social institution of aristocracy crumbled and was then decimated by the revolutionary spread of democracy. In such a world, at least in the West, only the professions "could create and sustain such a gentlemanly vision and bearing . . . and to preserve it, with some modifications and qualifications, into the increasing inauspicious setting of the nineteenth century" (p. 14). And this is the way people aspiring to and in the professions understood them.

The preeminence of specific professions during the span of his study, Haber argues, owes to the preeminence of fundamental questions galvanized by intellectual thought and critical events. The clergy held sway, from 1750 to 1830, addressing questions of morality originally raised two centuries earlier by Calvin. Between 1830 and 1880, lawyers came to the fore to address questions of rights over a century after Locke, in the wake of the French and American revolutions, and during the onslaught of Jacksonian democracy. Finally, from 1880 to 1900, the medical profession dealt with questions of human needs sparked by the Darwinian revolution. In this fashion, Haber not only examines the professions, but the impact of ideas on institutions as well.

The book thus takes a chronological, tripartite structure, and in each period it contains a wealth of information on professional institutions and their incumbents. Haber makes good use of case studies, usually of individuals (esp. during the 18th century), both as exemplars of more general experiences and as key "switch men" of ideas. In the second period, case-study comparisons are used of the professions (mainly medicine and law) in two cities, Cincinnati and Memphis, to demonstrate the dominance of law and the ascension of medicine in the mid-19th century. Haber includes more than what he calls the "nuclear family of professions"—the clergy, law, and medicine—in his examination of engineers and college professors during the final decades of the 19th century.

As a sociologist, one could criticize Haber for focusing too much on individuals (in some passages, the work almost reads like "great man" history, only of the professions instead of the state) at the expense of institutions or for tedious tangents in the case studies of Cincinnati and Memphis. As a disciplinary protectionist, one could criticize Haber for not weaving at least some of the relevant insights from the sociology of the professions (his footnotes indicate that he is familiar with the literature) into his analysis, particularly when he considers the jurisdictional battles in medicine and engineering during the final decades of the 19th century. One could finally point out the irony of dismissing the usefulness of so-called systematic analysis and then simply substituting a system more to one's liking (e.g., Haber prefers the influences of honor to that of the material interests implied in Weberian and Marxist frameworks).

No doubt many will embrace the sophistication and care of *Quest for Honor and Authority*. I am also sure that the book will be debated among those who study the professions; they will raise the criticisms stated above as well as others. No matter how one ultimately regards this work, however, it underscores the persistent importance of social identity in explaining individual and institutional behavior of all kinds. Certainly expertise, power, and wealth are part of professional identity. But Haber's message is that it is the *need* for a social identity of high social standing (and it is implicitly cast as a psychological and social need in Haber's work) that is most fundamental to all endeavors that might be considered professional. If Haber's volume places too much importance on ideal interests, it is nonetheless a welcome addition to cultural histories of the American middle class. It will undoubtedly be an important touchstone for future examinations of the professions.

Expected Miracles: Surgeons at Work. By Joan Cassell. Philadelphia: Temple University Press, 1991. Pp. xxviii+281. $44.95 (cloth); $16.95 (paper).

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This is an ethnographic investigation of surgeons and surgical work. It is based on 33 months of fieldwork and formal interviews with 31 surgeons. It is a provocative book, notable not only for what it says about the world of surgeons and surgery, but also for the position it takes with regard to "value neutral" ethnography.

This book announces itself to be different, and it is. In the preface, the author rails against what she feels is the all-too-familiar ethnographic investigation of powerful groups or individuals in which the social scientist assumes a position of moral superiority vis-à-vis those being studied. The author categorizes such studies as "righteous" "ethnographies of indignation" (p. xvi–xvii) and finds them additionally lacking because they blind the investigator (along with subsequent readers) to the "true" ambiguities of the situation, which, if understood, could have led to "significant changes" (p. xviii). Cited transgressors include Eliot Freidson's *Profession of Medicine*. In contrast, the author proclaims to have produced a balanced study that neither "deplores" nor "denounces" but rather "understands" (p. xviii).

To this end I find this book only partially successful. To its credit, *Expected Miracles* contains a wealth of descriptive material about surgeons and their work, an excellent selection of quotes from interviewees, some beautiful epigrams, references to a variety of literary characters and sources, a nice glossary, and an exquisite set of cross-references embedded within the text (allowing the reader to move directly to other sections of the book).

Some of the more troubling aspects of this book can be observed by examining chapters 5 and 6, which sit at the heart of this publication. The first covers a day in the life of a "compassionate young surgeon," and the second a similar "day" with a less than admirable colleague. Taken as a pair, these chapters underscore a disconcerting tendency for rationales to pose as explanations, and, in turn, for rationales to masquerade as understanding. The chapter on the compassionate young surgeon is truly breathtaking. The text and quotes sweep the reader along in a plethora of delights and admirations. Here you encounter the surgeon of your dreams, technically sophisticated and interpersonally skilled. He is sensitive, introspective, and moral. He heals as well as cures (an important distinction in *Expected Miracles*). Our second surgeon, however, does not stand in sufficient contrast to the first. Although we witness in great detail a characteristic recklessness that results in the death of a patient, our revulsion is muted. For example, we are led through several interactions in which the surgeon argues that he made no errors and thus there was no culpability. Furthermore, we learn that although the surgeon did not discuss critical options with the patient ahead of time, the need for any presurgical consultation was obviated, according to the author, because he belonged to the same moral and social community as his patient, and therefore was in a good position to understand his patient's preferences. What is troublesome here is not so much these "understandings" as the absence of field data (quotes, etc.) that might independently establish such a claim. Thus we never "hear" the surgeon commenting on his perception of his patient's wishes, nor how, in fact, he had a social bond with this patient. Similarly, the author tells us that the surgeon "knew he failed" (p. 161), but once again we are not privy to any data independently supporting such a conclusion. At the end of these two chapters, what we have encountered is an ode to beauty followed by the story of a flawed prince whose "fault" lay not so much in placing himself above his patients as in being trapped by his fellowship with them. Our reckless surgeon has been transformed into something akin to another victim.

Equally troubling is this book's analytical and explanatory lapses. Although we read that surgeons possess a different emotional temperament (more akin to those of fighter pilots, "a breed apart," p. 34), and that the proper surgical temperament is selected by chiefs of surgery when they choose new house officers, we never learn how this selection is accomplished. As a consequence we understand nothing about how a surgical culture might be transmitted or how anticipatory socialization might play a role in this process. Elsewhere, we read that women surgeons face a double bind (e.g., if they have the right temperament they are perceived by their male colleagues as pushy and nonfeminine, but if not they will be perceived as poor surgeons). We also hear that the author found each of the seven senior women physicians she interviewed to be "remarkable" in having "beat the odds" (pp. 18–19). What we do not find is any explanation of what they did to beat the odds and how.

What we *are* told is that surgeons can be distinguished from members of other specialties by their walk and gait ("Surgeons walk with an upright, confident stride," p. 42), and that the author is able to so delineate. We also learn that surgeons are being "besieged" by "petty" administrators and threatened with bureaucratic self-protection "gradually displacing [surgeons'] heroic, miracle-working mystique" (p. 187). We hear (again without supporting data) that frequently it is "those patients who demand empathetic, time-consuming, individual care from their doctors . . . who wish to reduce the price of that care" (p. 195).

In some instances the conclusions offered are quite interesting (a proposed relationship between surgical temperament and the "paranoia" of surgeons being one example), but too often it is not clear if what is being said represents a simple description of the surgeons' perspective or a conclusion by the author.

It is the tone of the preface that establishes the overall temper of this book. Here the author spends considerable time introducing both critics and antagonists in order to detail why they are mistaken or misdirected. Such an approach is unfortunate because it invites the reader to take sides. Thus, when the author shares the existence of external reviewers, who found this manuscript "exasperating" (for its lack of ties between typologies and data) or who characterized it as more a "personal memoir than a scientific study" (p. xxii), she invites agreement. Although I did not share in these particular conclusions, I did find myself troubled by this book's lack of analytical attention as well as the author's tendency to intermingle what sounded like surgeon-based rationales with what might otherwise be explanation. At the same time I found the descriptions of surgical work to be quite fascinating and many of the book's distinctions (e.g., between curing and healing) to be provocative. If you wish to learn a great deal *from* surgeons, by all means read this book. If your penchant is to learn *about* surgeons, I might first turn to some of the books criticized in the introduction of this book.

Women in Engineering: Gender, Power, and Workplace Culture. By Judith S. McIlwee and J. Gregg Robinson. Albany: State University of New York Press, 1992. Pp. xiii+248. $17.95.

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Women constitute only 7.3% of all U.S. engineers, making engineering the most male dominated of all professions. In this engaging and intelligent study Judith McIlwee and J. Gregg Robinson set out to identify the factors that have contributed to this extraordinary imbalance. They do so by productively combining material from questionnaires, sent to 406 working engineers, with in-depth interviews with 52 women and 30 men drawn from the larger sample, all graduates of two Southern California engineering schools. Adopting what they call a "conflict-structural" perspective, they argue that, taken individually, neither gender socialization theories nor structuralist analyses of work are adequate to understand why engineering is so dominated by males. Instead we need to understand how these factors interact to create a distinctive engineering culture peculiarly unfriendly to women.

This book is a welcome one. It is part of a rethinking of the sociology of engineering that has the potential to transform the field in a way that parallels the revolution that has, in the past decade or so, transformed the sociology of science. As the latter's center of gravity has moved away from the occupation-centered study of scientists to the sociological investigation of scientific knowledge, a key role has been played by analyses of the "woman question in science" (see Sandra Harding, *The Science Question in Feminism* [Ithaca, N.Y.: Cornell University Press, 1986]). This book by McIlwee and Robinson takes a tentative first step down that same road and suggests how similar attention to the "woman question in engineering" may make us take seriously the content of technical practice.

A central feature of McIlwee and Robinson's argument is that there is a distinct "engineering culture," characterized by a form of locker-room, macho tinkering, in which male engineers strut their stuff in competitive displays of hands-on technical virtuosity. This culture intimidates many women engineers who lack self-confidence in their own practical skills despite excellent mathematical abilities and academic credentials. Such displays are particularly powerful forms of exclusion, the authors argue, since they are most prevalent in the place where engineers as a group are most dominant and autonomous, the research and development (R&D) lab. It thus undermines women's self-identification as engineers and leads them away from what the authors see as the heart of the engineering profession. Only by forsaking such places could the women in the sample find success, and to do so they had to "sacrifice" engineering values in the pursuit of managerial and other people-focused careers.

Although the discussion of engineering culture is central to the argument there is much else of worth in this book. For those seeking to increase female enrollment in engineering programs, the important but almost accidental role played by family and friends in acquainting mathematically skilled girls with engineering is an important insight. The discussion of the interactional structures of engineering schools, and the way these women struggled to overcome the male biases of teachers and fellow students, should prove useful to those working on minority retention in colleges. The book is generally an excellent example of the way a well-crafted, relatively modest research program can foster the sociological imagination.

It is not, of course, free from problems. My major concern is with McIlwee and Robinson's willingness to see the cult of macho tinkering as *the* archetypical engineering culture. There is simply too much evidence to the contrary. Many engineers, perhaps most, see engineering as an occupation built on careers which, inevitably and desirably, lead away from technical practice. Many *male* engineers willingly choose an environment, represented here by the aerospace industry, which stresses technical management rather than virtuosity, and do so with no real sense of loss. Engineering is a pluralistic and business-oriented profession in which technical virtuosity is only one form of successful performance. If we take a cross-national perspective, engineering cultures are even more various. Tinkering is certainly not a central part of the engineering culture in France, where engineers disdain hands-on practice as the job of technicians and prefer to stress the importance of mathematics and abstract reasoning as a source of distinction. Yet these are precisely the skills that most women engineers in McIlwee and Robinson's sample feel most comfortable with. This is not to deny the existence of local cultures of competitive tinkering, nor to deny its impact on women or other engineers who find this cultural style offensive or difficult to emulate. I do wonder, however, whether even in Southern California it is as critical in explaining discrimination as McIlwee and Robinson imply, since the relatively women-friendly environment of the aerospace industry was as much the result of strong, government-enforced, affirmative action programs as of its bureaucratic rather than engineering culture. In the light of Sally Hacker's discussion of the role of mathematics in the patriarchical culture of engineering (*Pleasure, Power and Technology* [Boston: Unwin Hyman, 1981]), we certainly need to be careful of overgeneralization from this one sample.

Despite these questions about the universality of any single engineering culture—and, to be fair, the authors do not make so strong a claim explicitly—this is still a very suggestive book. By its very claims it raises questions about alternative styles of engineering. What, for example, would a women-centered engineers' style look like? How do such styles differ by industry or by country? Further, what are the implications of this and other engineering styles for both the actual practice of engineering and the kind of products and technology developed in such settings? By looking closely at local engineering cultures we can begin to link studies of the social organization of design worlds with an examination of the content and practice of technology. This promises to be a fruitful agenda, and *Women in Engineering* is recommended equally for the questions it raises as for the contributions it makes to our current understanding.

Women between Two Worlds: Midlife Reflections on Work and Family. By Myra Dinnerstein. Philadelphia: Temple University Press, 1992. Pp. xvii+223. $34.95 (cloth); $16.95 (paper).

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Women between Two Worlds is a rich descriptive study of women who came of age after World War II. Myra Dinnerstein uses a life-course approach to illuminate the changes in the lives of 22 white, middle-class women who began their adult lives as full-time mothers but later pursued professional careers. She describes the conflicts and adjustments these women experienced as they balanced the competing demands of work and family.

The study participants were interviewed in the mid-1980s, when they were in their forties. Participants were recruited through professional newsletters, women's organizations, word of mouth, and mailing lists of women faculty at two universities. Respondents were drawn primarily from one mid-size western city and were included in the study if they were born between 1936 and 1944, had spent at least three years at home with children, had an intact marriage, and had professional training or career involvement. Dinnerstein argues that this sampling strategy allows her to focus on the gender oppression experienced by privileged white, middle-class women.

Based on her qualitative interviews, Dinnerstein describes the phases of development these women experienced from adolescence to midlife. Most of the women graduated from high school as "traditionalists," expecting to become full-time mothers. Those women who developed nontraditional interests in high school or college found that there were few socially acceptable alternatives to marriage and motherhood. Regardless of their initial career orientation, all the women eventually married, had children, and became full-time mothers.

These women had varying reactions to their experiences as housewives. Among the "contented" housewives were those who intended to return to work when their children were older ("time-outers") and those who had no plans to work ("long-termers"). The "discontented" housewives were distressed by their lack of control over their time and activities and felt guilty about not enjoying time at home with their children.

All of the women returned to the labor force after raising their children. This decision was relatively easy for the time-outers, who had planned to stay home only temporarily. The long-termers returned to work in response either to a deteriorating marriage or the family's financial need. I was surprised to learn that, despite the distress they experienced at home, the discontented housewives were the most reluctant to return to work. Many of them did so only at a husband's insistence or when the problems of their daily existence at home reached a crisis stage.

Although they had successful careers, the women were constrained by their dual commitments to work and family. Most gave priority to the needs of their families when making career decisions. Many husbands were supportive of their wives' careers, and some helped out with housework to ease their wives' burden. However, none were willing to share responsibility for household labor equally.

Despite its rich qualitative material, this study is seriously limited by its sampling strategy, which makes it impossible to address many of the book's questions. To explain why women who stayed home to raise children eventually returned to the labor force, one needs to compare such women to those who did not return. To conclude that the forces pushing nontraditional women toward full-time motherhood were overwhelming, one would need to recruit respondents without requiring that they spent three years as full-time mothers. Yet women from appropriate comparison groups (e.g., women who stayed home after their children grew up, women who did not marry, mothers who worked continuously, etc.) were deliberately excluded from the study. Dinnerstein seems to recognize this limitation and occasionally refers to relevant comparison groups from other studies (see, e.g., p. 89). But this is no substitute for a larger and more carefully selected sample. Without that, the book cannot answer many of the questions it raises.

Moreover, even when the book focuses on questions about married, middle-class professional women, the sample is too small and homogeneous to capture meaningful variation. Seven of the women were college professors, and 11 of the 22 couples included at least one spouse who was a college professor. Academic careers pose special constraints and opportunities not typical of other professional careers.

Despite these limitations, the author's findings and conclusions are largely consistent with those of previous studies. The book is well written and presents a skillful qualitative analysis. Yet the book does not break new ground, and much of the material will be familiar to family and gender sociologists. Moreover, the book's methodological limitations are likely to frustrate most sociologists. The study should have been more broadly conceived to explain why some women made the transitions described in the book while others did not.

New Families, No Families? The Transformation of the American Home. By Frances K. Goldscheider and Linda J. Waite. Berkeley and Los Angeles: University of California Press, 1991. Pp. xvi+303. $34.95.

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A growing number of young people experience their parents' divorce and remarriage, and a growing number experience a period of independent living during their young adult years. How will these changes affect the future of the American family? Are these changes leading men and women to enter marriages where there is equal sharing of economic and domestic responsibilities (new family)? Or, are they leading men and women to abandon marriage and parenthood (no families)? Goldscheider and Waite are optimistic that evidence points toward the outcome they personally prefer—the growth of "new families."

The book briefly discusses demographic changes since the 1950s that affect American families and reviews findings of public opinion data on sex role attitudes over this time period. But the heart of this study is a detailed analysis of data from the National Longitudinal Surveys (NLS). Most attention is given to the panel of young women 14–24 years old in 1968 who were interviewed nearly every year for 15 years. Within this sample, the emphasis is upon women between 14 and 17 years old in 1968 and between 29 and 32 in 1983. The samples of young men (14–24 in 1966) and mature women (30–44 in 1967) also are analyzed when they are needed to help fill in the picture. Juxtaposing the research question and the data source, one is forced to ask how the authors can answer the question they pose with the data they analyze. How can the issue of social change be addressed with information on a 15-year segment of the life course of one cohort? The authors are aware of the limits of their data and note that they do not have information on actual change in behavior across cohorts over time. Nevertheless, they argue that these data allow them to infer (with qualifications) the direction of social change.

Raising the controversial question about the future of the family provides a structure for organizing the topics discussed (and, no doubt, will increase the number of people induced to open the book). But, I do not think that Goldscheider and Waite are persuasive in arguing that findings from their analyses point to a future of increasingly egalitarian, satisfying families for men, women, and children. The evidence they present is sufficiently mixed or ambiguous that alternative spins can be given to its meaning. Further, to address this question one needs to give far more attention to other changes occurring in American society. Fortunately, the value of this book does not hinge on its contribution to the big debate. Between the introduction and conclusion is a wealth of information on interesting and important smaller questions related to family and gender roles.

Two themes are most central to this study. One deals with effects of growing up in a nontraditional family (defined here as one that did not include both biological parents when the child was 14). How does family of origin affect a young person's plans and attitudes toward future work and family roles? How does it affect actual marriage, childbearing, and divorce behavior during the young adult years? The second theme concerns how living outside of a family setting in early adulthood changes one's attitudes and plans and how it affects subsequent family behaviors. The outcome of most interest is division of labor in the family. How much do husbands and children share in the household tasks? While exploring these themes in detail, a number of related questions are addressed.

A look at the effects of young women experiencing independent living illustrates the type of findings and the challenges they present for interpretation. Compared to women who never lived independently, those who lived away from parents after age 17 expected to have fewer births, approved more of mothers' working, and planned to work more in the future. These findings remain significant in a multivariate analysis, suggesting that independent living leads to less traditional family values. It is surprising, therefore, to find that women who lived independently before marrying shared fewer household tasks with their husbands than women who moved directly from their parents' home into marriage. Further, the women who lived independently before marriage shared fewer household tasks with their children than did other women. These, and many other findings, raise interesting questions regarding the long-term effects of the changing family experiences of young adults upon the growth of egalitarian families.

New Families, No Families advances our understanding of individual characteristics and family background factors that shape the type of family that young adults create. Equally important, it raises a number of questions for other researchers to explore. The authors are on target when they argue the need for more family research that is focused on men. Additional studies that replicate the analysis of this one on more recent cohorts of young adults may also advance our understanding of the future of the family.

Child Care, Parental Leave, and the Under 3s: Policy Innovation in Europe. Edited by Sheila B. Kamerman and Alfred J. Kahn. New York: Auburn House, 1991. Pp. 248. $42.95.

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Motivated by the neglect of very young children in U.S. social policy, Sheila Kamerman and Alfred Kahn examine child-care and parenting policies in six countries: Austria, Germany (FRG), France, Hungary, Finland, and Sweden. This volume extends and updates the editors' previous work on child and family policy in industrialized countries. This compendium provides further evidence of the backwardness of U.S. policy in caring for children, especially for very young children.

This book serves as an invaluable reference guide for cross-national sociologists who wish to examine women's status and/or work and family issues in any of the selected countries. However, this wealth of information is not easy to read. I advise U.S. researchers who are not interested in these specific countries, but who are interested in child care or family policy, to read only the introductory and concluding chapters.

The introductory chapter documents how recent U.S. federal policies, such as the Family Support Act of 1988, largely ignore families with children under age three, and how the 1990 child-care legislation is inadequate to the needs of families with infants and toddlers. This chapter, along with the concluding chapter, succinctly illustrates the similarities and differences in the parenting policies offered by the selected countries. Austria, Germany, France, Hungary, Finland, and Sweden share the fundamental goal of caring for very young children at home for various periods of time immediately following childbirth. Each of these countries has some form of paid, job-protected leave after the birth of a child. Different policies include extended or supplementary job-protected leave —either as an earnings-related cash benefit or as a flat-rate benefit—the right to work part time, or an emphasis on expanding the availability of child care for toddlers. Yet the specific components of parenting policies vary among these countries in terms of the mechanisms and motivations. Kamerman and Kahn set the stage for the individual country chapters by asking what the United States can learn from the experiences of these European countries.

The next section of the book consists of six chapters, each focusing on a single European country. Scholars from each country describe the development of child-care services and parenting policies in the context of domestic, demographic, and economic changes. The editors should be applauded for soliciting the perspectives and expertise of these cultural insiders.

Some authors are more successful in placing the evolution of parenting policies within a cultural context than are other authors. The chapters on Austria, France, and Hungary provide the best discussions of how parenting policies develop in response to demographic, economic, and political concerns. Christoph Badelt depicts the controversial nature of parenting policies in Austria as a battle among interest groups such as the Catholic Family Association and political parties at the federal and provincial levels. Marie-Gabrielle David and Christophe Starzec effectively present French family policy within the development of the country's overall social security systems. Maria Adamik shows how Hungarian parenting policies have been shaped by low birth rates, a desire to promote full employment, an excess of unskilled female labor, and an inadequate supply of child care. Indeed, the Hungarian government initially designed its child-care grant policy to encourage women to stay at home to care for their very young children because of the high costs and poor quality of infant and toddler care.

Christiane Schiersmann wrote the chapter on the Federal Republic of Germany before reunification with the German Democratic Republic (GDR), and, thus, the information on current German parenting policies may be somewhat out of date. Since this chapter does not provide any background information on parenting policies in the GDR, it is difficult to chart the future direction of German child-care and parenting policies in the coming years. Likewise, Adamik suggests that Hungarian family policy may be in jeopardy because of the financial limits of the state, local authorities, and private firms.

In the final chapter, Kamerman and Kahn briefly compare parenting policies in the six countries in terms of strategies, eligibility criteria, benefit levels, benefit durations, financing, coverage, and participation. They discuss the consequences of the national policies for the well-being of children and for gender equity. For example, Sweden's parenting policies promote gender equality—Swedish men are more involved in fathering than men in other countries. On the other hand, Hungary's child-care grants and allowances encourage traditional gender roles.

Unlike their earlier work, Kamerman and Kahn do not suggest that the United States should implement a European-style parenting policy. Instead, they suggest that we can learn from the experiences of the Europeans in developing a policy package that maintains parental choice, builds on the current U.S. policy infrastructure, and preserves local diversity.

Natural Allies: Women's Associations in American History. By Anne Firor Scott. Urbana: University of Illinois Press, 1991. Pp. xii+243. $29.95.

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Once we bother to look, the unnoticed may prove omnipresent. This has surely been the experience of scholars of "organized womanhood" in the United States. Whether the issue is the origins of the welfare state, the history of environmental policy, or the constitution of taste and the construction of libraries, the clubwomen, moral reformers, and benevolent ladies appear everywhere. The convergence of feminist scholarship and social history has produced a rapidly growing literature on women's efforts at organization for political action and mutual benefit. Even Anne Firor Scott, a noted historian of American women, acknowledged that "If I had known in 1979 what I know now, this book might not have been written, for, . . . I had no idea of the magnitude of the task" (p. 1). Persevering against an ever-growing tide of publications, however, she has produced *Natural Allies: Women's Associations in American History,* an encompassing and insightful assessment of this expansive field.

To impose order on this literature, Scott organizes her discussion around two historical periods—from the Revolution to the Civil War and from the Civil War through the 1920s—and, within these periods, by both organizational form and organizational purpose. The first half of the book traces the emergence of three central types of organization: the benevolent society, the moral reform association, and, during the Civil War, the soldiers' aid societies. Having traced the origins of this wide-ranging array of women's organizations, Scott turns to their purposes: religious reform, self-improvement, municipal reform, and social justice. Throughout these discussions, she turns the mass of new scholarship to analytic ends, using comparisons between North and South, black and white, to challenge interpretations of organized womanhood as primarily a phenomenon of middle-class and upper-middle-class white women outside the South.

In the place of these group-specific explanations, she organizes her discussion around two more or less implicit arguments: the extent to which and the ways in which groups made use of the form of the voluntary association reflected their positions within the broader structure of American society. The first of these arguments rests on a view of voluntary association as only one possible form of collective action or vehicle of individual attainment. With access to business, the professions, and politics, men used the voluntary association as an essentially residual form, "a convenient way to get certain things done, . . . a way to assert masculinity or to define the male sphere, a place to do business or enjoy sociability with no women around" (p. 176). For women, the voluntary association was almost the *only* place to get things done and, consequently, was expansive with respect to issues and central to the career-building efforts of many talented and ambitious women of the period. But if the voluntary association provided a solitary channel for women's efforts in the 19th century, the golden age of organized womanhood was brought to an end by the very success of women's efforts to gain entry to other fields of endeavor.

While the formation of voluntary associations is determined by the absence of other options, Scott argues that the form of association is shaped by the group's goals and practices. Women's associations were transformed as members moved from goal to goal, supplanting efforts to reform individuals with attempts to change social and economic conditions. Each new form, in turn, prompted the cultivation of new skills or "business methods" that influenced the ability of organized womanhood to act collectively. As women sought to create careers within voluntary associations, those associations adopted bureaucratic forms and paid staff positions that could sustain careers.

Throughout her account, Scott remains both comprehensive in her coverage and graciously skeptical about the consequences of organized womanhood. Rather than offering an uncritical celebration, Scott suggests that this flowering of association may have had unanticipated consequences as devastating as the prolongation of the Civil War, through the determination of both Northern and Southern women to produce supplies and nurse the wounded. She is sensitive to the ways in which women's associations perpetuated inequalities of class and race. But the comprehensiveness of *Natural Allies* is also the source of a certain analytic weakness. Insofar as women's associations appeared everywhere, among all sorts of groups, we lose a sense as to *why* they appeared in a particular form at a particular time. While offering a typology of association, Scott does not offer an explanation of the succession of forms apart from the shifting goals of the membership. Nor, insofar as voluntary associations are treated as empty vessels shaped by the activities of the membership, do we gain a sense of the constraints of this organizational form. Yet surely the insistent nonpartisanship that characterized voluntary associations from men's fraternal orders through the General Federation of Women's Clubs helps to explain the difficulties confronted by politically inclined women's associations once they had gained the right to vote.

These are questions, however, that could not have been framed clearly when Scott began this book over a decade ago. Consequently, the questions are themselves testimony to the accomplishments of *Natural Allies*. As Scott concludes, "I hope this book is a good beginning, but it is only a beginning" (p. 183). In providing a comprehensive, engaging, and consistently insightful account of a long-neglected aspect of American history, she has given us a very good beginning and much more.

Social Mobility and Social Structure. Edited by Ronald L. Breiger. New York: Cambridge University Press, 1990. Pp. xii+364. $44.50.

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Ohio State University

The last 15 years in sociology have seen the birth and development of "the new structuralism," that is, approaches that identify and use aspects of social structure to explain socioeconomic inequality. Analyses include assessment of effects of market characteristics on worker outcomes, studies of job segregation within firms, and portraits of the economy as divided into multiple segments. Ronald Breiger has edited a volume that informs our knowledge regarding one subset of this larger tradition: that concerning structural approaches to the study of social mobility. In the volume's introduction, he traces the history of the new structuralism to a reaction against the status-attainment tradition in sociology and the human capital perspective in economics. He considers the extent to which mobility analyses are structural in character and argues that the chapters in this volume are fundamentally concerned with how social structure conditions mobility processes.

One of the fundamental contributions of the new structuralism has been an intermarriage between the study of organizations and stratification, as several chapters suggest. Peter Marsden and Karen Campbell study processes of organizational recruitment, an underresearched topic. Although they acknowledge their sample of manufacturing workers likely underestimates the explanatory power of their models, their work should provide a foundation for broader inquiry into organizational selection. Karen Gaertner traces the upward mobility of public school administrators in Michigan between 1968 and 1973 and is able to identify the most typical paths of upward and downward career mobility during this period. James Rosenbaum contributes a very insightful, interdisciplinary review of alternative models of career mobility within organizations. He summarizes evidence supporting the tournament model of mobility from a study of a major corporation, as well as noting compatible evidence from other empirical research. Robert Althauser and Arne Kalleberg both conceptually develop and empirically evaluate models of firm internal labor markets, with particular attention to predicting the likelihood of job shifts over time. Andrew Abbott uses vacancy chain models to study the administrative staffing of public psychiatric hospitals between 1840 and 1930, while John Padgett analyzes patterns of mobility of U.S. congressmen through House committees between 1961 and 1971.

Other chapters address mobility issues within the broader context of occupational or industrial systems. Jerry Jacobs presents convincing evidence to suggest that aggregate patterns of occupational sex segregation mask both women's substantial entrance into and exit from male-dominated occupations. He suggests that working conditions in male-dominated occupations may prevent women from staying once they enter sex-atypical occupations. Jack Brittain and Douglas Wholey demonstrate that labor-market practices are noticeably influenced by industrywide forces, including labor demand and variations in demand for products. Joel Levine derives "equivalence categories," or groups of occupations that have similar internal mobility patterns. He finds that industry is an important determinant of mobility, thus providing evidence consistent with Brittain and Wholey's arguments regarding the importance of industrial differentiation. Breiger uses mobility data to identify social classes, and he reports both empirical success and challenge in presenting a set of conclusive categories.

Two additional chapters make theoretical contributions. Nan Lin links social mobility and social resource theory and thus provides a potentially important bridge between research on social mobility and increasingly important thinking regarding the microfoundations of macrolevel social structures. Christopher Jencks poses key questions regarding the theoretical grounding for scales of occupational prestige and occupational standing. He argues that researchers should construct a closer correspondence between their theoretical interests, in either social mobility or occupational standing, and the scales they use to measure these outcomes.

It is legitimate to note that several of the entries in this volume are, to varying degrees, derived from other publications. For example, Karen

Gaertner's study of organizational careers originally appeared in 1980 in *Sociology of Education;* Jerry Jacobs's study of circulating mobility is well developed in his recent book, and both Andrew Abbott and James Rosenbaum note some overlap with their respective early works. A more profound issue, however, is whether this volume helps to promote the collective conversation we should be having in the field of stratification. Considerable evidence suggests that most analyses that fall under the rubric of "the new structuralism" are formulated either in opposition to or in isolation from past and ongoing work on social mobility. Also, many analysts of social mobility construct their research agendas as though 15 years of empirical evidence regarding structure and stratification were largely nonexistent. This volume sets an important example for the integration of these traditions by using a number of structural approaches to address the problems of social mobility that have been so intellectually fruitful for our field. Undoubtedly, important intellectual progress can and should be made by these respective traditions in isolation from each other. But to the extent that the intellectual intermarriage of these two traditions is potentially fruitful, this volume should provide one foundation for it.

From Middle Income to Poor: Downward Mobility among Displaced Steelworkers. By Allison Zippay. New York: Praeger, 1991. Pp. 144. $39.95.

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University of Dayton

When social histories of the Reagan years are composed, case studies like this one on displaced manufacturing workers should provide the building blocks of the analysis. This book describes the results of a cross-sectional survey using in-person interviews with three classes of respondents: 102 displaced steelworkers from the Shenango Valley near the Ohio border of Western Pennsylvania; 10 displaced workers who had relocated outside the area; and 22 directors of community service organizations located in the valley and county. The result is a portrait of disrupted lives, family stress and coping, social service inadequacy, subemployment, and downward mobility in a decade ironically celebrated as the boom years of the last quarter of the 20th century.

The book provides data that form a counterpoise to some recent interpretations of the problems of the working poor. Some social policy analysts treat the status of America's working poor as mainly derived from low skills, low education, and low productivity. Their focus is upon the human capital characteristics of the workers themselves rather than the types of jobs they hold. This study reports on the nature of reemployment after the plant closings in the steel industry and shows that for this population, hard work can no longer be equated with economic success. A majority of displaced workers who found jobs after the shutdowns were reemployed in such service sector jobs as janitor, delivery truck driver, and security guard. Work at minimum wages, involuntary part-time schedules, no benefits, and intermittent employment replaced their histories of steady, well-paid jobs with fringe benefits and skill development. Women, blacks, and middle-aged men suffered the longest unemployment and greatest income losses. Their downward mobility reflects the broader circumstances of workers displaced from the millions of manufacturing jobs that disappeared from the United States in the 1980s, jobs that have yet to be replaced with economic activity employing comparable skills, worker conditioning, and appropriate compensation.

Written in fresh and jargon-free language, the book describes the emotional pain and stress of the workers and their families, and the social support networks associated with forced downward mobility. Replacement jobs of short duration, frequent layoffs, and poverty-level incomes produced a predictable and devastating chain reaction of personal and family difficulties, from property losses to the "normal" social pathologies, to diminished life chances for children. The author describes and evaluates family coping strategies and other social supports. She shows how the magnitude of worker displacement and the wide regional impact defied individual, personal methods of resolution. Surprisingly little collective action took place to protest worker circumstances and obtain relief. The author argues persuasively that the failure of the Carter administration to endorse loan guarantees needed for a steelworker employee buyout of nearby plants in Youngstown, Ohio, depressed worker hopes of responsive public policy.

One of the strongest contributions of the book is the author's cogent analysis of the inapplicability of existing social service supports to the particular circumstances of displaced workers. The author evaluates the eligibility requirements for welfare programs, the decline in public funding at a time of escalating need, lack of outreach, workfare requirements, and the many serious limitations of the Job Training Partnership Act. She also shows how existing public programs were unable to ameliorate the condition of displaced workers. Also, the failure of private, market-based adjustments to restore equilibrium at a decent level of family support is reflected in the author's descriptions of subemployment, pockets of poverty, and little regional redevelopment.

This book is a solid resource and is recommended for library purchase as well as supplemental reading in upper division sociology and social work courses. It contains an especially effective collection of respondents' statements on the major themes and a clear treatment of the limitations of social welfare programs and critiques of existing policies with respect to displaced workers and their families. The selective bibliography is useful, as is the historical background (regrettably absent is the decade of the thirties). The only limitation on using this cloth-bound book as supplemental reading for advanced courses on work and occupations, families, and social stratification is its cost in relation to its relatively short length.

From Protest to Acquiescence? Political Movements of the Unemployed. By Paul Bagguley. London: Macmillan, 1991. Pp. x+223.

"Parish-fed Bastards": A History of the Politics of the Unemployed in Britain, 1884–1939. By Richard Flanagan. New York: Greenwood Books, 1991. Pp. xiv+289. $45.00.

Richard Lewis
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Both these books try to tackle one of the most baffling issues in the social history of modern Britain, and indeed of all Western capitalist societies: the failure of unemployment to radicalize politically the unemployed. While the two books deal with a similar theme, they are quite different in method and approach. Paul Bagguley seeks to offer a viable sociological model for political activity (or inactivity) by the unemployed, while Richard Flanagan's work is a historical analysis of political activity by the unemployed in Britain up to the Second World War.

To sustain his case, Bagguley engages in a critique of existing sociological models, mostly based on studies from the United States, in which he seeks to expose the methodological as well as the theoretical flaws of current analyses. In particular, Bagguley finds the neo-Marxist work of F. F. Piven and R. A. Cloward (*Poor People's Movements: Why They Succeed, How They Fail* [New York: Pantheon Books, 1977]) too dismissive of the value of conventional political activity by the unemployed. He points out that election results can matter a great deal to the unemployed. He is even more critical of the behaviorist analyses of K. L. Schlozman and S. Verba (*Injury to Insult: Unemployment, Class and Political Response* [Cambridge, Mass.: Harvard University Press, 1979]), which he believes tend to decontextualize the experience of unemployment and also fail to see the key role of the state in shaping both the form and the content of political activity by the jobless. Bagguley argues that Schlozman and Verba see the state as a purely neutral agency. Attempts to apply behaviorist analyses to the British situation are also criticized for failing, inter alia, to take full account of pre-unemployment political activity by different groups of jobless people, especially trade union and student activists.

Bagguley's alternative model for political activity rests on what he sees as three key factors. First, he considers the role of the state as the agency for relief of poverty and the extent of local discretion in relief payments that the unemployed may influence if they are well organized. His second factor centers on the nature of organizations for the unemployed. He distinguishes between monological organizations, which are run for and not by the unemployed, and dialogical organizations, which are run and controlled by the unemployed themselves and which also seek to transform rather than simply reform the social and political order. Finally, he seeks to set the above two factors in a cultural, mainly ideological, and community context.

Bagguley engages in a comparative study of the experience of unemployed political activity in the interwar years with that of the early eighties. His study of the interwar years, based mainly on secondary sources, is focused on the National Unemployed Workers (Committee) Movement (NUWM), which Bagguley argues was a quintessentially dialogical movement that could tap into local cultural and community forms of resistance and mobilize the jobless against the full rigors of the state poverty-relief mechanisms. The success of this form of activism was broken by state power, bureaucratic rather than coercive, which eliminated local discretion in relief payments and therefore fatally undermined the value of collective action. This strategy bred a fatalism among the unemployed, as collective action met defeat after defeat. Thus, in the context of the early 1980s, while there were local points of resistance to state unemployment policies, there was nothing that could be compared to the NUWM. From a detailed study, based largely on oral testimony of the Brighton Unemployed Workers' Centre, Bagguley argues that only where the appropriate cultural, ideological, and community values exist can collective action by the jobless be sustained. It was only in this context, rare in the eighties, that Bagguley found anything to match his view of the interwar experience of collective protest. He also found that, normally, political activity by the unemployed was determined by values and beliefs acquired before they became jobless.

While Bagguley's study is useful in qualifying existing analyses and certainly in exposing methodological and theoretical weaknesses in them, the book does not really offer a comprehensive, alternative viewpoint. The attempt to underpin the analysis by providing a historical context is not really successful as the comparison with the interwar years is a broadbrush study, resting on a very positive view of the NUWM based mainly on secondary and other published sources, while the study of the later period is founded on primary research in a very local context. Bagguley's conclusion that the elimination of local discretion in benefit payments, and the essentially monological, frequently state-funded agencies established in the eighties for the unemployed militated against collective political action by the jobless, is hardly an earth-shattering discovery. He tends to overstate the dialogical nature of the NUWM, and he also paints a picture of collective protest in the interwar years contrasting with collective acquiescence in the eighties which probably would not stand up to a more detailed study of the earlier period. The book can be used to introduce students to the subject, but Bagguley's valuable original research on the early eighties would have been better served by a more focused monograph.

The attention-catching title of Flanagan's book derives from a term of abuse hurled by police at the family of a Birkenhead unemployed workers' movement activist during a violent raid on his house in 1932. There is nothing detached or uncommitted about Richard Flanagan's study. It sets out to reject current analyses, which stress the political passivity of the unemployed and portray them as helpless victims, incapable of affecting their destiny. His assault on this "dissembling scholarship" (p. 2) commences with a fairly orthodox neo-Marxist historical survey of the nature of unemployment and unemployed political activism from Tudor times up to the implementation of the New Poor Law and into the era of the Social Democratic Federation. It is, however, with the interwar years that Flanagan is mainly concerned, and, like Bagguley, he focuses on the NUWM as the chief agency of collective protest by the unemployed themselves. While his survey is interesting and provides a useful digest of anecdotal evidence drawn from a wide range of primary and published sources (including novels), it hardly disproves the current orthodoxy that the unemployed were not potentially or actually a major threat to public order or that collective action by them could affect their destiny. Even Flanagan is forced to admit that "it cannot be shown that unemployment radicalized the working class of the inter-war period, or for that matter, any other period" (p. 194).

The book is particularly useful, however, in two specific respects. First, it helps to kill the once widely held but erroneous idea that the NUWM was simply a front for the Communist Party of Great Britain. Second, it has a valuable chapter on the role of state-supported voluntary social service for the unemployed. This much-neglected aspect of interwar unemployment policy deserves more in-depth treatment, including some local and regional studies. Flanagan's book provides a neo-Marxist critical overview, especially of the National Council of Social Service, which it will be interesting to see tested and challenged.

Hierarchical Structures and Social Value: The Creation of Black and Irish Identities in the United States. By Richard Williams. Cambridge and New York: Cambridge University Press, 1990. Pp. xii+189.

Philip Kasinitz
Williams College

Comparing the historical experience of African-Americans with that of white immigrants to the United States is a tricky business, one that is often fraught with invidious distinctions. Richard Williams's account of the making of "black" and "Irish" Americans avoids these pitfalls by examining the historical circumstances under which these social identities were constructed. Unfortunately, while the intentions of *Hierarchical Structures and Social Value* are admirable, the book falls short of its promise.

Drawing on structuralism, Marxism, and world-system theory, Williams's main purpose is to argue that both "black" and "Irish" identities "are socio-historical creations within the structure of the society rather than natural groups that were transplanted from Africa and Ireland" (p. 145). Williams uses a wide range of historical scholarship to make the case for seeing race and ethnicity as the markers of emerging "slots" in the American social and economic hierarchy. He argues that the belief in race and ethnicity served to transfer the social stigma attached to slavery and unskilled labor from these structural positions to the people who filled them, thus obscuring the true nature of social relations.

On the issue of race, Williams attributes considerable importance to the fact that the original unfree laborers in North America were native Americans and whites. The small number of Africans in the early Virginia colony seem to have more or less shared the status of indentured servants, with inheritable African slavery becoming the dominant form of unfree labor only after about 1680. From these facts he derives support for the notion that the solidification of the slave system created the racial category of "black," rather than racial distinctions creating the institution of chattel slavery. This is, of course, essentially the classic argument made by Eric Williams in *Capitalism and Slavery* almost half a century ago. Yet while Richard Williams cites Eric Williams, and a number of other well-known historical works, he still writes as if this version of the early days of American slavery were generally unknown.

More unexpected, and more interesting, is Williams's review of how the expansion of European economic influence starting in the 16th century eventually made large numbers of Africans available for enslavement in the new world. Similarly, he argues, the partial nature of the incorporation of Ireland into the world system, together with the underdevelopment of wage labor in Ireland, forced millions of Irish peasants to emigrate during the 19th century. This created the basis for their incorporation as the most exploited rung of the "free" labor market in the United States, Canada, and Great Britain. Unlike most analysts of race and ethnicity, Williams takes seriously the impact of these processes on the sending societies and treats the arrival of slave and free laborers in the United States as part of the broader history of capitalist expansion. Further, he argues that the creation of racial and ethnic identities masked the diversity within these groups (not all Africans, after all, were available for enslavement) and focused attention on the groups themselves rather than on their relationship to the social structure.

At many points Williams's argument seems overly ambitious. He is, after all, attempting to do justice to 300 years of history on three continents in 147 pages of text! Yet if his case for the social construction of racial and ethnic categories is less original than he sometimes implies, it is, on the whole, reasonable enough. *Hierarchical Structures and Social Value* is marred, however, by the repeated setting up and knocking down of theoretical straw men. Clearly the idea that race and ethnicity are social constructions is not, in and of itself, a particularly "hard sell" for a book whose presumed audience is sociologists. Yet Williams seems to think that this is a radical notion. He states several times that "social scientists" assume race to be "primordial" or "a natural category that can legitimately be utilized to explain social inequality" (p. 132). It is unfortunate that Williams, who is normally quite meticulous about citations, does not say which "social scientists" he has in mind.

Further, even those sympathetic to Williams's structuralist approach are likely to find his actorless conception of history a bit much. He writes about the "assignment" of groups to particular "labor slots" and uses the language of smoothly working systems, obscuring the messy, unpredicted nature of much of the history he describes. He is also capable of breathtaking oversimplifications, such as describing the English civil war as simply the struggle of "rich merchants" against the landed aristocracy (p. 78). Finally, the reviewer would be remiss in his consumer consultant functions if he did not mention that Williams's prose is in the Parsonian tradition: it ranges from mildly overwritten to downright turgid.

Despite these flaws, *Hierarchical Structures and Social Value* does manage to pull together a wide variety of historical evidence to make an interesting, if not always convincing argument about the origins of racial and ethnic stratification in the United States. If that argument is not as novel or provocative as the author seems to think it is, it is nevertheless one worth making.

The Fine Line: Making Distinctions in Everyday Life. By Eviatar Zerubavel. New York: Free Press, 1991. Pp. xii+205. $22.95.

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In *The Fine Line,* Eviatar Zerubavel conducts one of the most readable and erudite (83 of the book's 205 pages are references, bibliographies, and indices) excursions into the realm of social cognition. Continuing his investigations of the "taken-for-granteds" of social life (and extracting methodological riches from what most individuals typically ignore), Zerubavel moves from time (e.g., *Hidden Rhythms* and *The Seven Day Circle*) to the even more fundamental notion of cognitive boundaries. It is not surprising that such a work has appeared in this era of increasingly blurry lines, from medicine's uncertain demarcations of life and death to the increasing moral relativism and diminishing faith in absolutes.

The work begins with an elaboration of the cognitive and social foundations underlying our sensory parsings of everyday life and subsequent recognition of social frames. Boundaries—the "fine lines that keep different experiential realms separate from one another" (p. 84)—are, as the title suggests, the author's focal point. They are the central mechanism by which social order is carved out of chaos, discrete categories are recognized within continuums, and by which the self becomes distinct from other selves. Zerubavel's fine lines carry heavy weight, cognitively, morally, and socially.

Lines produce mental and social gaps that artificially inflate perceived distances and differences between social entities: because of political lines, we think of Santiago as being closer to the United States than Moscow; because of age-graded lines, society views a 21 year old as having more in common with a 50 year old than with a 15 year old; because of school district lines, two 10-year-old friends living across the street from each other may be bussed to two different schools; because of old antimiscegenist lines, a U.S. citizen with one sixty-fourth African-American blood would be classified as "black," but one with one-seventieth could be "white." Because of their segregative power, lines are the prerequisites of categories and distinctions, guiding individuals to "know" what is similar, what can be ignored, and what patterns are to be recognized.

Individuals vary in their sensitivity to and need for lines. Zerubavel develops two ideal-type minds from those at the extremes of this sensitivity: the rigid and the fuzzy. The rigid abhors the ambiguous and the chaotic and cherishes clear distinctions, purity, order, and laws that transcend specific contexts. These are the folks good at pigeonholing and solving "Find the Faces in the Tree" puzzles. Detesting the categorically impure and liminal, they object to homosexuality, drugs, and melting pots. Theirs is the mindset of authoritarians and, in the extreme, the closure-seeking agoraphobics who "dread all forms of open-endedness" (p. 49) and the sufferers of anorexia and bulimia, who are obsessed "with maintaining a rigidly bounded self" (p. 51).

The fuzzy mind, on the other hand, feels aversion toward all boundaries, mocks and plays with conventional (and arbitrary) distinctions, and gravitates toward ambiguities, novel syntheses, and communal experiences through which one can blend in with one's social surroundings. Seeking to eliminate barriers between individuals, these fluid-minded folks are attracted to Marxism, cosmopolitanism, and universalism. Such fluidity characterizes the worlds of the very young, mystics (who "renounce the very idea of 'classifying and dissecting,' promoting instead a holistic view of reality whereby everything flows into everything" [p. 84]), and the psychotic.

Social lenses amplify the cognitive tendencies to either create or blur the gaps between the mental entities staked out by social reality. A cultural predominance of one mind over another is reflected in architecture, art, language, intermarriages, food (the eclectic cuisines of the fuzzies and the meat and potatoes of the rigids), games, gender distinctions, and privacy needs.

The ideal mind is, not surprisingly, "flexible" and permits both analytic and synthetic thinking. Personal and social identity require the separateness and insularity provided by the boundaries—hence the need for the rigid. Innovation and change require the fuzzy. Each mind requires the other: "Boundaries . . . protect us from the endless anguish generated by open-endedness" (p. 119); creativity "presupposes boundaries that one can transgress" (p. 118).

So by what criteria can one judge such a work? Comparable to the bottom-line R^2 of methodological inquiries, a theorist's work can be judged in terms of its scope and its ability to establish connections between apparently unrelated things. With his ideas, Zerubavel links the Berlin Wall, animal rights, Marxism, Escher's art, religious purification rituals, knock-knock jokes, and the Dewey Decimal System.

Identity and Ideology: Sociocultural Theories of Schooling. By Stanley William Rothstein. New York: Greenwood Press, 1991. Pp. ix+161. $42.95.

Ira Katznelson
New School for Social Research

This is an elusive book. It makes two central points, each of which is plausible. The first is that "the schools that capitalism establishes have a primary interest: the development of an ideology and culture that make it natural for students to accept proletarianization" (p. vii). Rothstein argues forcefully, if not distinctively, that capitalism requires schools to produce ideologies and practices to create a working class amenable to the requirements of a capitalist order. The second is that a host of theorists concerned with the relationship joining schooling and capitalism have substituted a mechanism-free functionalism for considerations of agency, resistance, and change. Bourdieu, for example, is upbraided for theories said to "dissolve the individual, removing him from consciousness and an ability to influence what is happening to him in the classroom" (p. 58). By contrast, Rothstein advocates "a sociocultural theory of schooling . . . [that is] grounded in the individual's ability to heighten his or her consciousness of schooling experiences" (p. 125). Schooling, he argues, establishes a terrain of ideological class struggle. Read this way, the book is an invitation for scholars, teachers, and students to understand the place of mass education in capitalist societies and to seize opportunities offered by the educational system to fight for meaningful change.

In pursuing this twin mode of argumentation, Rothstein offers a number of quite interesting discussions of the work of a number of social theorists—including Althusser, Lacan, Bowles and Gintis, and Bourdieu—as well as probes of culture, ideology, and signification. Though uneven in clarity and tone, these considerations are thoughtful and often provocative. Yet the main achievement of *Identity and Ideology* is contrary to the intention of its author. The book highlights key tensions in Marxist and functionalist theory about the relationship of structure and agency without adjudicating them. Marxism, of course, never has resolved the relative weight it wishes to place on contradictions internal to the structure and logic of capital as opposed to conscious class agency. Functionalist accounts of diverse kinds find it difficult to accommodate purposeful mechanisms into satisfactory explanations that do not burst the bounds of functionalism itself. Utilizing schooling as a point of departure, Rothstein articulately restates these quandaries and shifts ground from one emphasis to another, but he does not test the various competing claims empirically, nor does he find a satisfactory theoretical stance from which to argue about and explore the modern history of schooling in the West.

I regret the author's implicit decision not to advance theory through an evaluation of the book's competing claims by studying actual schools, teachers, administrators, and students—that is, by looking closely at the actors Rothstein claims have an important hand in shaping the social reality that structures their lives. This is especially disappointing because Rothstein has had personal experience as a school teacher. Instead, he has opted to produce a volume devoted to a presentation of his reading of key authors and to the development of a theoretical discourse whose audience is never clearly specified.

On its own terms, the book is unevenly realized. It does not consider in a sufficiently nuanced way the internal tensions between the authors and between the types of work it endorses. Figures as diverse as Tönnies, Balibar, and Basil Bernstein are presented one after the other as if their ideas mesh without any difficulty. So, too, holistic and agency-centered explanations are juxtaposed as if their relationship is not problematical. Rothstein speaks on behalf of treating capitalism less as a structuring cause, as in base-superstructure versions of Marxist social theory, than as the element that joins together "the state, the economy, the culture, the schools, and other features of society as interconnected parts of an indissoluble entity" (p. 39). To be sure, as Susan James has argued, holism and individualism can be reconciled (*The Content of Social Explanation* [Cambridge: Cambridge University Press, 1984]), but Rothstein does not demonstrate the manner in which he proposes to join his version of capitalist holism to his agency-centered emphases. Further, the status of the various European and American historical cases that he treats as illustrative material is not clarified: is there an embryonic comparative-historical project awaiting birth here, or is history utilized merely as the source of anecdotes that "prove" the appropriateness of a specific theoretical project?

Yet, it need be said, even if Rothstein has not meshed his intelligent reading notes to make an entirely satisfying whole, they are nonetheless worth pondering as an effort to come to grips with unrealized issues where the history of education jostles with some of the toughest unresolved concerns of social theory.

The Mennonite Mosaic: Identity and Modernization. By J. Howard Kauffman and Leo Driedger. Scottdale, Penn.: Herald Press, 1991. Pp. 308. $15.95.

Passing on the Faith: The Story of a Mennonite School. By Donald B. Kraybill. Intercourse, Penn.: Good Books, 1991. Pp. 316.

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In 1975, Goshen College professor of sociology J. Howard Kauffman published, in association with Leland Harder, a sociological profile of the progressive wing of the Mennonite family based on research conducted in 1972 (*Anabaptists Four Centuries Later: A Profile of Five Mennonite and Brethren in Christ Denominations* [Herald Press, 1975]). In 1989, 17 years after that original research, Kauffman and a new colleague, Leo Driedger of the University of Manitoba, carried out a second study of the same five North American denominations—the Mennonite Church, the General Conference Mennonite Church, the Mennonite Brethren Church, the Brethren in Christ Church, and the Evangelical Mennonite Church. *The Mennonite Mosaic* is the report of these two researchers and the presentation of the data they have generated from the new survey administered to 3,083 members of the five denominations, a select sample drawn from 153 different congregations. The stated purpose of the survey was to "obtain a profile of the religious beliefs, attitudes, and practices of members of the participating denominations" as well as to "provide clues regarding the impact of modernization on Mennonite life and thought" (p. 274). The former objective was the same as that governing the 1972 survey; the latter goal was new to the 1989 endeavor.

The authors have divided their presentation into three parts. The opening section of the book deals with the impact of modernization on Mennonite identity. In it the authors provide both a historical sketch of the Mennonites as well as insight into the regional settings that shape Mennonite realities in the present day. In the second part they focus upon five aspects of Mennonite identity that contribute to the construction of their social world. Kauffman and Driedger speak of religion, community, family, institutions, and ethnicity as supports protecting that identity. In each of these areas they have measured change and also charted continuity. The third part of the volume is an exploration of the consequences of modernization among the Mennonites. In particular, it is an examination of the changes and adjustments made by the denominations. Kauffman and Driedger address such topics as the alternative networks of service established by the Mennonites, the ways in which they compare with other Christian denominations in North America, and the evidence of increasing pluralism within their ranks.

It is, of course, impossible to summarize the substance of the report in this review. Kauffman and Driedger provide an immense amount of data in 87 tables and 12 figures. These data invite close examination by those interested in longitudinal studies as well as in comparative studies. Most of the tables compare and contrast the findings of the 1972 and the 1989 studies. The authors find, to no one's surprise, that modernization "tends to seduce people toward secularization, individualism, and materialism" (p. 271). Yet that is not an unbroken or uncontended process among the progressive Mennonites, for the countervailing forces of religion, family, community, and institutions work toward continuation of a sense of distinctive identity.

It is not coincidental that the foreword to *The Mennonite Mosaic* was written by Donald B. Kraybill, a professor of sociology at Elizabethtown College and the author of several books dealing with Anabaptist groups, including *Passing on the Faith: The Story of a Mennonite School.* Kraybill is interested in the same question that drives the survey by Kauffman and Driedger; namely, What happens when modernity assaults a traditional religious community? Yet he seeks an answer using a very different approach. He has written what he calls a "light social history" of the first 50 years of Lancaster Mennonite High School (LMHS) in Lancaster County, Pennsylvania. His narrative is an institutional chronicle, an account of the beginnings and the development of the school. Abundantly illustrated with more than 200 photographs, black and white as well as color, and containing 14 tables summarizing other information, this history leaves few topics unexamined.

Kraybill's principal audience for his history may be the alumni, supporters, and friends of the Lancaster Mennonite High School. His sympathetic and generally uncritical perspective makes this jubilee volume particularly appealing to insiders. Likewise, the fact that, in the appendices, he lists every graduate of the institution for the past 50 years as well as the names of faculty members, class mottoes, class songs, and class gifts, suggests that the publisher may be hopeful that this volume will sell among the school's former students. Kraybill, himself a member of the class of 1963, does not hide his affection for his alma mater.

But there is more here than first meets the eye. Kraybill successfully carries the reader into mid-20th-century Mennonite culture and shows the dramatic changes that have taken place in that parochial world during the past 50 years. He is especially effective in describing the dilemma felt by community leaders. The school was founded by religious and social sectarians who feared the influence of the world and determined to preserve their values and traditions by protecting their youth. They launched the school at the most inauspicious time—the opening years of World War II. The founders—especially the first principal, J. Paul Graybill—intended to create a protective environment that would also nurture the faith of Mennonite students. Self-denial and humility were the watchwords for what one faculty member called the "yielded life"—submission to Christ as a ruling power in all aspects of existence. Initially the school demanded a controlled environment with prohibitions and prescriptions governing virtually all aspects of life. Regulations dictated clothing styles, leisure activities, courtship patterns, and the use of such items as radios and musical instruments. Competitive sports and other activities that fostered individualism or competitiveness were ruled out. Even the faculty members were subject to close supervision.

That was the situation 50 years ago. During the intervening years, internal and external pressures mounted steadily, and the result has been slow but certain accommodation to the modern world. Changes in leadership, faculty, students, activities, and curriculum—these testify to the fact that the Mennonites of Lancaster County have not been immune to the forces of change affecting other religious and social groups in America. Perhaps the most striking evidence in the volume is the photographs that fill its pages. Long-skirted, black-stockinged, bonneted young women from the 1940s stand in stark contrast to the stylish coeds of today; plainly dressed young men of the earlier period seem equally out of place alongside the T-shirted youths at LMHS in the 1990s.

Despite the obvious changes, Kraybill would have us believe that certain fundamental commitments remain central. Here is where his "light history" and the survey data compiled by Kauffman and Driedger intersect. Both acknowledge the impact of modernization, but both also document ways in which the Mennonite community manages to preserve a variety of cherished values. In the case of LMHS, unity is no longer expressed through dress and ethnicity, but rather through a common faith. In the case of the progressive denominations, basic Mennonite values seek expression through alternative means, including distinctive ethical activities. Both studies compel reflection on the never-ending pressure for change and the processes of time.

Japanese American Ethnicity: The Persistence of Community. By Stephen S. Fugita and David J. O'Brien. Seattle: University of Washington Press, 1991. Pp. vii+218. $30.00.

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Structural assimilation into the institutions of the host society by immigrant populations in the United States is associated with the erosion and replacement of ethnic bonds with ties based on class, occupation, and religion. A study of Japanese-Americans in three California communities by Stephen S. Fugita and David J. O'Brien reports a constrasting pattern of structural assimilation without erosion of ethnic ties.

The persistence of formal and informal ethnic ties under rapid structural assimilation among the second or *Nisei* and the third or *Sansei* generation is linked to a cultural orientation of "peoplehood" which the *Issei* or first generation adapted and shared with later generations.

Structural and demographic influences condition this process. There are 11 chapters to assess this thesis. Approaches and available studies are reviewed in the first three chapters. Chapters 4–10, "The California Survey," "Early Socialization by Generations," "Friendship and Voluntary Association Patterns," "Economic Activities and Networks," "Intermarriage," "Political Attitudes and Participation," and "Persistence and Change in Perceived Social Boundaries," provide data and analyses. Generational status, education, income, type of employment, rural-urban status, intermarriage status, and religious affiliation and participation are employed in cross-tabulations or more complex regression analyses to evaluate the general proposition. The final chapter reassesses the basic thesis in light of the evidence.

The study contributes in major ways to the theoretically interesting problem of the linkage between the rate and level of structural assimilation and the erosion or persistence of shared ethnic activities. More direct measurement and comparisons can build upon the careful research reported by the authors. How a sense of "peoplehood" is linked to given economic and occupational contexts requires more work. How specific economic adaptations (pre- and postimmigration occupational history) relate to given issues and attitudes is required.

Where quasi kin ties and sentiments are thought to be responsive to structural and demographic influences, the sources must be specified and evaluated directly. More is needed on whether these adaptations continue on the basis of core family/village/provincial (*ken*) networks developed prior to World War II under unremitting discrimination and segregation. How they functioned along the West Coast (indexed in part by the continuing strength of segregated ethnic Christian church arrangements) requires analysis. Even in the eastward dispersal during and after World War II, how these networks were adapted and survived in Cleveland, Chicago, and other environs requires attention to details, such as forms, density, and depth.

The impact of incarceration during World War II requires more work on how individuals within different segments adapted by utilizing quasi kin bonds and exchanges. The camp and postcamp experiences may have both centrifugal and centripetal tendencies. Under role reversal and severe cross pressures, *Nisei* had to lead and *Issei* had to follow in the camps. Some *Nisei* found that they had to work as appointed and elected leaders in uncharted waters in the camps. A large number of youths were sponsored to pursue high school and postsecondary studies outside of the camps. Others were sent to work outside of the camps. Young men served in large numbers in the armed services. How ethnic ties were adapted among the different segments (by generation, age at time of internment, involvement in camp organization and leadership, and other experiences in more integrated settings) needs study. How these less segregative experiences affected their *Sansei* children requires study.

Throughout, each respondent's overall level and type of assimilation (educational, occupational, associational/structural, marital, cultural) must be related to the type and depth of formal and informal ethnic ties and activities under different contexts.

To assess the impact of specific structural and demographic influences, we require comparisons of the Japanese experience in Hawaii, Canada, and South America. In the Japanese immigration to Hawaii there were proactive family reunification and return policies, labor-protection policies, and community development policies that were endorsed by the receiving government. There was no incarceration. For West Coast immigrants not under contract, the above policies operated in some form within a somewhat more literate and perhaps urban base. Adaptations also may have been influenced by the stream of Japanese scholars and university students sponsored under a very strong study and training abroad policy. Controlled comparisons among other immigrant populations with convergent and divergent cultural orientations are needed as well.

Of Revelation and Revolution: Christianity, Colonialism, and Consciousness in South Africa. By Jean Comaroff and John Comaroff. Chicago: University of Chicago Press, 1991. Pp. xx+414. $60.00 (cloth); $18.95 (paper).

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This sophisticated study should ideally prevent colonialism from ever again being reduced to a simplistic paradigm of domination and resistance. Drawing on two decades of archival and field work, Jean Comaroff and John Comaroff, professors of anthropology at the University of Chicago, have written an analysis of one southern African people's experience of the colonial encounter, circa 1820 to 1920. Unlike nearly every earlier study of Christian missions, this book scrutinizes the missionaries and their culture every bit as intently as it examines the society the missionaries hoped to convert, in this case, that of the southern Tswana. The authors have cast a wide net. In order to understand the impact of Christianity on African minds, we are made to know the world that produced David Livingstone and all the other heroic proselytizers of the 19th century. Then we are led to appreciate how European hegemony was established over southern Africa and what it meant to the ways Africans viewed their own world and interpreted their own experience. Europeans' power stemmed, in large part, from their ardent faith in rationalism, positivism, and the cult of the fact. Faced with this "universalizing ethos," the Tswana were weakened by their own "cultural relativism and political autonomy" (p. 310).

By moving between European—or, more particularly, Nonconformist—and African perspectives, the authors present a fuller historical anthropology of the colonial encounter than I have seen in any other scholar's work. While Landeg White's *Magomero* (Cambridge University Press, 1987) came closest to depicting the kinds of social disruption brought to African households by the missionaries, even that fine book kept mainly to the realm of politics. After introducing the cultural categories and discourses of 19th-century Britain via such exemplars as Wordsworth, Dickens, and the Brontës, the Comaroffs direct our attention to the entire social order on the fringes of the Kalahari Desert, not to its political dimension alone. The details of the first encounter are depicted in terms—water, space, and language—that early 19th-century Tswana would have found significant. By the end of the book, we have learned how the Protestants had helped to sow the seeds of a new political culture, one that would flourish in the crevice between, on the one hand, their teachings about "a liberal democracy of self-determining individuals" and, on the other, the practices of a "racially coded, class-ridden system of domination." A proposed second volume promises to bring the consequences of that contradiction up to the present so that we may understand "the modes of consciousness and of struggle" (p. 6) in South Africa today. In this way, the authors hope that anthropology may kick over its old traces and serve as "an instrument of liberation" (p. xiii).

Of Revelation and Revolution is the culmination to date of the life's work of two especially inventive anthropologists. Their previous writings, alone and together, include such diverse topics as law, gender, resistance, medicine, marriage, and the privatization of land. They have clearly listened attentively to those critics who have blamed their discipline and sometimes even their own work for obscure language, ahistorical schema, and excessive concern with epistemology. This book suffers from none of those flaws: it is generally easy to read, it was spawned by a marriage of history and anthropology, and it deals, "perhaps hopelessly ambitious[ly]" (p. 6), with the macrosocial forces that have shaped consciousness and behavior in southern Africa. Their sources are suitably diverse: interviews, papers from Tswana communities, novels, archives of the London Missionary Society and the Wesleyan Methodist Missionary Society, and state-of-the-art literary and social theory.

Can the "total history" of such an encounter be written in an utterly evenhanded way? The available sources do make such a balance extremely difficult to achieve. The sheer bulk of the writing by missionaries, not to mention the poetry, novels, and social commentary of other 19th-century Britons, allow their history to appear tumultuous, while the relative scarcity of first-hand Tswana records inhibits that sense of vitality. And so, the world of their Tswana contemporaries is asserted, rather than demonstrated, to be "dynamic" (p. 128). European ideas about gender, for example, are depicted as contested and in flux, while the impression is given that the Tswana flatly accepted that females be excluded from public places because of their polluting heat. And yet the Tswana easily jettisoned some such beliefs even from the early days of the encounter, and they simultaneously believed in natural as well as supernatural causes of events, as any investigation of their medical practice reveals. Did the Tswana really believe that "words shared in the reality of their referents" (p. 224) in a magical way set them profoundly apart from Europeans? On the contrary, it would seem that critical distance from local orthodoxies did characterize early Tswana "consciousness," and that their "sociocultural order" was more contested than would appear from the rather mechanical model of it given in chapter 4. Comparing that important chapter with the two on Britain preceding it, we find that the European party to the great encounter appears to have been moving organically and the African one to have been strangely still.

When the authors decided to avoid a linear narrative and to discuss a moment of social transformation, their choices entailed costs, sometimes at the expense of clarity. Such language as "at once prefigured yet fortuitous" (p. 212) may capture the struggles and the muddle accurately, but it can be puzzling to read. By eschewing a linear narrative, the authors have created a sharper picture of the beginnings of the encounter than of how its terms changed over time. Historical time is depicted especially less clearly toward the end of the study, when the 20th century has begun.

Grappling with uneven data and the marriage of two disciplines, the Comaroffs have succeeded in writing a richly creative book that no student of the colonial encounter anywhere in the world can afford to ignore.

Heavy Metal: A Cultural Sociology. By Deena Weinstein. New York: Lexington Books, 1991. Pp. xi+331. $24.95.

Simon Frith
Strathclyde University

Deena Weinstein wrote this book "to show how sociology can inform public discussion of heavy metal" (p. 3). She is writing, that is, in a long and honorable tradition of the American academy: her purpose is to cast a cool empirical eye on a topic beset with public prejudice and misunderstanding. And heavy metal musicians and fans are not just misunderstood—despised by progressive and reactionary critics alike—but positively abused: their records are labeled, their concerts are banned, and their songs are put on trial.

Simply as a thorough and dispassionate sociological study of a controversial social phenomenon, then, *Heavy Metal* is exemplary. Using a combination of research methods—all sorts of observation, questionnaire, and interview, systematic content analysis, a comprehensive trawl through the few academic studies, and heavy metal's own extensive literature—Weinstein provides a rigorous analysis of all aspects of heavy metal culture, including the musical genre itself, the performers, the audiences, the media, and the shows.

It is a considerable achievement for an academic to describe a piece of pop culture so accurately (and with such historical sensitivity)—the book is mostly free of those trivial errors that usually afflict popular music studies—and it is particularly striking that Deena Weinstein, "an adult female," should so successfully enter the world of the male adolescent. But, then, she is rather more partisan than she initially implies. Just like any other effective ethnographer, she was only able to make sense of this "strange" scene by becoming part of it: How could one participate so long in such a *noisy* culture without enjoying the din? By the end of her book Weinstein is a heavy metal fan.

She argues, persuasively, that the core value of heavy metal is power. Unlike other popular music forms, which are organized around melody, lyrics, or rhythm, "for heavy metal the sound as such—its timbre, its value, and its feel—is what matters" (p. 27), and it is its subsequent "sonic power" that defines heavy metal's aesthetic appeal, both in its Dionysian celebrations of physical abandon and in its emotional exploration of chaos. But, as Weinstein also makes clear, metal fans were not made or manipulated by a commercial, artistic, or satanic conspiracy; rather, the elements of sonic power in the music were taken up—shaped and focused—by a youth group that already existed: "music is the master emblem of the heavy metal subculture, but it is not its meaning" (p. 99).

For Weinstein, heavy metal music is "a function of the life-styles and mythologies" of its audience and, to be successful, must be consistent with them (p. 99). Its appeal lies in its ability to "empower" a social group that is institutionally (in terms of education, occupation, and prestige) of little value, and, in this respect, heavy metal subculture (a style blend of the hippie and the biker) marks one way in which the "liberated" youth culture of the 1960s was adapted to the exigencies of the blue-collar, white youth experience in the 1970s and 1980s.

Weinstein is at her best when she describes how her "proud pariahs" attempt to live the heavy metal life from day to day. She squelches the slur of heavy metal "mindlessness"—"the deepest pride of the member of the metal subculture is to be an appreciator of great music" (p. 143)—and explains well the difficulties facing heavy metal artists, who have to be, at once, skilled musical technicians (with all that means in terms of craft and self-discipline) and effective subcultural icons (with all that means in terms of a publicly excessive hedonism).

It is not surprising that, having become such a sympathetic participant in the heavy metal world, Weinstein should adopt its worldview. She continually claims, for example, that metal is quite different from other forms of rock and pop, but in many ways it is not, and her accounts of, say, the contemporary music industry or the social organization of a concert are valuable contributions to a much broader sociology of music. On the other hand, her final chapter, on the views of heavy metal's critics, depends on its own caricatures, and I was not persuaded to rush out for Ozzy Osbourne tickets or to change my rock critic's mind on the limitations of the adolescent male aesthetic.

Weinstein argues that heavy metal "does not have a single meaning," but is, rather, a "bricolage," "a collection of cultural elements . . . held together by interdependence, affinity, analogy and aesthetic similarity" (p. 5). And she proceeds to use this term, *bricolage,* on every possible occasion as some sort of theoretical totem. The concept is neither helpful nor necessary (the word simply stands for "a complex structure"). As a subcultural analyst, Weinstein is working in the Chicago not the Birmingham tradition. This is a sociology—an admirable sociology—of a "deviant" social group, of a demanding occupation, and of a surprisingly self-conscious art world.

Partial Visions: Culture and Politics in Britain, Canada, and the United States. By Richard M. Merelman. Madison: University of Wisconsin Press, 1991. Pp. xii+287. $40.00 (cloth); $15.95 (paper).

Kevin J. Christiano
University of Notre Dame

As its subtitle indicates, *Partial Visions* is a book about politics and culture. Yet it is *not,* its author insists, an examination of "political culture"—at least not in the traditional sense of that term. For Richard M. Merelman has come to bury the concept of political culture, not to praise it.

The adoption of a "political culture approach," in Merelman's view, is definitely "how not to proceed" (p. 39) in comparative studies of national political systems, because such an approach, with its emphasis on values held in common, "denies any positive role to cultural conflict" (p. 48). So instead the author proposes to augment studies of political culture with an idea he labels "conflictive democratic participation." Rather than simply threaten political stability, conflict between social groups, Merelman contends, can actually stimulate the involvement of citizens in democratic polities. For this reason, democracies "have an interest in group conflict and in protecting and encouraging institutions which mobilize this conflict" (p. 8).

How, then, do the three democracies that Merelman studies in his book respond to this task? If the theory is correct, he hypothesizes, political affairs would induce the greatest level of democratic participation in Great Britain—a country in which hierarchy is a practical fact of social relations, and political contention is organized around groups that are lodged in a relatively static constellation. Canada would occupy an intermediary position; like Britain, its political struggles assume membership in groups, but the connections between the most salient parties to conflict is dynamic, and its social system is a mixture of deference to position and desire for equality. The United States, for its part, would exhibit the least evidence of democratic participation. Here, according to Merelman, the heavy cultural weight given to egalitarianism, an overriding focus on the individual, and constantly shifting relations between social units all contribute to a failure of groups to mobilize citizens for political conflict.

Merelman searches for evidence to support these predictions in a wide range of "nongovernmental manifestations" (p. 35) of culture. The variety in his sources is notable: television comedies from each country are analyzed for how family roles are portrayed; secondary school texts in social studies are consulted for their depictions of trade unions and political parties; magazine advertisements and glossy corporate reports are scanned for what they suggest about the civic values of each society. In no case does "proof" of the theory emerge from Merelman's one-man content analyses; his stance is "interpretive rather than positivistic" (p. 233). Yet he is able to say that the "cultural configurations are *consistent* with the political language and structures of public opinion" (p. 56; italics in the original) in all three nations. And different political systems yield varying degrees of conflictive participation.

Weaving together as many strands of information as he does, Merelman understandably confuses some details of his specific cases. For instance, in at least two points in the text (pp. 131 and 214) he mistakenly places in 1982 the referendum on sovereignty for Québec that the Canadian province's separatist government conducted. Elsewhere (e.g., p. 32), however, he fixes the date of the voting correctly in 1980. Similarly, the bibliography misidentifies several prominent sociologists, including Robert N. Bellah and William Julius Wilson.

An explicitly comparative framework and a strategy for analysis that fuses elements of several disciplines combine to enhance the appeal of this project. Still, methodological purists should be forewarned that *Partial Visions* is not an especially tidy piece of research. The focus of the study whips around abruptly among the three cases at the same time that the narrative leaps from example to example. Supporting literature is cited in abundance, but its direct relevance is seldom established. Quantitative data and impressionistic observations mix freely in the discussion.

Nevertheless, the result of these deviations from the usual routines of social scientific research is itself a kind of departure—a work that is ambitious, original, and highly thought-provoking. Few readers who are acquainted with any of the three cultures that are described in the book will fail to be challenged by Merelman's arguments.

Who Rules Britain? By John Scott. Cambridge, Mass.: Polity Press, 1991. Pp. 171. $47.95 (cloth); $24.95 (paper).

Ralph Miliband
City University of New York

Like a great many writers before him, John Scott is concerned to demonstrate that a ruling class exists in Britain and that it is constituted by a "power bloc" in which the capitalist class predominates. The notion of a power bloc, derived from Gramsci, refers to "an alignment of social groups, generally under the dominance of one of them, which is able to monopolize the levers of political power in a society over a sustained period" (p. 33). As for the capitalist class, it is represented by the people who own and control the main means of economic activity in Britain—industry, commerce, finance, and land.

Much of Scott's discussion is devoted to show how the capitalist class wields a preponderant degree of influence on state policies, either by direct involvement in government or by way of the influence it is able to exercise. There is also a great deal in the book about the ways in which the cohesiveness of the "power bloc" and the capitalist class is achieved, with much emphasis on the importance of the public (i.e., the private) schools (Oxford and Cambridge), prestigious clubs, participation in common pursuits and rituals, and so on.

Scott's delineation of the capitalist class is interesting and cogent, even though there is nothing much new in his exposition. There are, however, a number of issues that are dealt with perfunctorily or not at all. One of them concerns the degree of autonomy that the "state elite," more specifically the government, enjoys in relation to the capitalist class in the determination and pursuit of policy. For his part, Scott seems to accord very little such autonomy to governments—too little in my opinion.

Perhaps more important is the absence of any discussion of pressure from below against both the capitalist class and the state elite. To show that there does exist a ruling class in Britain (as in all capitalist countries) is no doubt well worth doing, given the immense efforts that are deployed to obfuscate its existence. But there is also need to demonstrate how, in regimes with democratic forms and with opportunities for pressure from below, class rule is maintained and defended. Scott refers at the end of his book to the notion of hegemony and the fact that the ruling class in Britain is able to establish its intellectual predominance over society. But the discussion of the issue is slight, and it is rather surprising that the author should have paid no attention to the control of communications networks exercised by members of the capitalist class. It would also have been useful, in a book of this kind, to have had some discussion at least of the other mechanisms that help the ruling class meet the challenges that it encounters from the social groups that are not part of the so-called power bloc.

Culture and Currency: Cultural Bias in Monetary Theory and Policy. By John W. Houghton. Boulder, Colo.: Westview Press, 1991. Pp. viii+210. $49.95.

Bruce G. Carruthers
Northwestern University

Money makes the world go around, and in *Culture and Currency* John Houghton examines an important debate about money. In England between 1797 and 1821, bullionists and antibullionists argued about if and when the Bank of England should return to the gold standard and make its bank notes convertible to specie on demand. To be sure, this was a policy disagreement, but, more generally, it was a theoretical debate about the nature of value and how money possessed value. Why was intrinsic worth, an almost magical quality, attributed to gold? The political context for the controversy was a rich and complex one. England was fighting an expensive war with France, and the bank was perhaps the most important institution for public finances. Whigs and Tories disagreed over how to fight the war and how to fund it. Finance capitalists fought with landed aristocrats and industrial capitalists over financial policy. Houghton's task is to make sense of all the shouting. He brings to bear on this an alchemical soup of theories and ideas: mathematician René Thom's catastrophe theory is mixed with anthropologist Mary Douglas's ideas of group and grid, and power is thrown in for good measure. It is an intoxicating brew, but once the reader's head has cleared it seems that Houghton has made iron pyrites and not the real thing.

The book begins methodically with the elaboration of the theory. According to Houghton, there are three underlying social dimensions (group, grid, and power), each of which can vary from high to low. Logically there are eight possible "ways of life," since the two × two × two table has eight cells, but in reality there are only five feasible combinations. Houghton lists these as egalitarian (high group, low grid, low power), hierarchist (high group, high grid, high power), individualist, fatalist, and hermit. The five social positions constitute places on a three-dimensional topological form named by its inventor, René Thom, the "cusp catastrophe" (it looks like a square bedsheet with a large fold in the middle). All members of a society assume one of the five positions. Each position brings a cultural bias, a predisposition on the part of the occupant to adhere to a particular set of values or ideology. Debates and disagreements arise when people from different positions, with correspondingly different ideas, contest some issue. With five social positions, there are only 10 distinct debates (five times four divided by two), which may explain why all arguments begin to sound the same after a while.

Using this scheme, Houghton proceeds throughout the book to summarize different stages of the bullionist controversy and to classify the debaters and their ideas. Some procrustean pinching and pushing is necessary to ensure that all bullionists and antibullionists fit into one of the five boxes, while at the same time Houghton's description of the ideas attached to social positions is sometimes so vague as to be reminiscent of fortune cookie predictions and newspaper horoscopes. For example, political radicals were egalitarians, and so "would argue a line characteristically egalitarian, that is, one similar to the individualist, but more fundamentalist and with a less elaborated symbolic framework" (p. 43). Most of the book consists of summaries of the political and economic situation, overviews of the debate, and then classification of interlocutors.

This taxonomic exercise is joined by a polemic against a particular philosophy of science. Houghton refutes the positivist argument that the bullionist debate was simply part of the development of economic science by showing that the better theory did not always win, that social position (and political clout) mattered more than analytic rigor, and that turns in the debate were not mechanistically determined by world events. He convincingly defeats the strawman of logical positivism, and since that is the only recognized alternative to his model, Houghton concludes that the grid-group-power-catastrophe scheme must be right.

Houghton's discussion of how domestic and international politics combined with economic interests to shape a cultural debate over money is very interesting. The discussion of the real bills doctrine is informative, and I could not help but notice many parallels between Houghton's period and the financial controversies of the 1710s. His theoretical posturing is unconstructive, however, and I doubt that there will be many converts to this fusion of Thom and Douglas. In the bullionist debate, Houghton had a golden opportunity to say something interesting about the social construction of value, but lacking the Midas touch he leaves the reader mostly frustrated and feeling impoverished.

Governance of the American Economy. Edited by John L. Campbell, J. Rogers Hollingsworth, and Leon N. Lindberg. New York: Cambridge University Press, 1991. Pp. xii+462. $65.00.

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What determines the nature of economic activity? The products produced, the prices paid, the social relationships among producers, sellers, and consumers? In other words, what determines how the economy is *governed?* Modern mainstream economics has built an entire discipline by assuming one answer: markets. Recently, a "new institutional economics," observing how poorly markets describe economic life, has offered an alternative answer: organizational hierarchies. Sociologists have joined economists in modeling and debating the conditions under which markets or hierarchies govern economic activity. Sociologists are now broadening the discussion even wider. *The Governance of the American Economy,* edited by John Campbell, J. Rogers Hollingsworth, and Leon N. Lindberg, argues that markets and hierarchies are only two possible forms of economic governance. By offering a model of a broad range of governance regimes, along with a number of industry-specific case studies, this original and important book offers a new and challenging agenda and represents part of a growing movement within economic sociology to transcend economists' narrowly conceived assumptions about rationality and efficiency. Even the term "governance" implies the *social* relations of power, cooperation, authority, conflict, and exchange that shape economic life.

The heart of the volume's analysis is a typology of economic governance forms and a rudimentary theory of how industries are transformed from one type to another. Markets and hierarchies are only two possible governance mechanisms that vary in their scope of interaction and degree of formalization. Bilateral forms include informal mechanisms of coordination (markets), moderately formalized mechanisms labeled "obligational networks" (subcontracting, franchising, joint ventures), and highly formalized organizational hierarchies. Multilateral forms of governance, which are almost entirely ignored by economists, range from informal monitoring mechanisms (interlocking directorates, dominant price setters), moderately formal promotional networks (research and development alliances, coalitions) to formal associations (trade associations, cartels, syndicates).

This typology is just a scaffolding for the more interesting issue: the transformation from one mechanism to another. Eschewing grand theories that preordain the direction of change (e.g., modernization notions of economies of scale or Marxists' concentration of capital), they outline a standard sequence of the transformation process: stable regimes are plunged into crisis by internal or external shocks (a new technology, a major depression, foreign competition, legal changes). A search process, the focus of their analysis, then ensues. The key theoretical challenge is the factors determining the outcome of the search process. Their answer reflects their empirical richness and their theoretical circumspection: it depends. Certain that efficiency, rationality, and technology cannot explain the outcome of the search process, they emphasize the role of the state, especially in its enforcement of property rights (which they define broadly enough to include antitrust and regulative law). However, in any particular circumstance any or all of these factors might operate. Contingency reigns.

Unlike most edited volumes, this is a truly collective effort. The three editors, a sociologist, a historian, and a political scientist, developed the conceptual model. Graduate students conducted the case studies on a variety of American industries including old (steel) and new (nuclear energy), competitive (meat packing) and regulated (telecommunications), purely private (automobiles) and mixed (hospitals), large (railroad) and small (dairy). Each case study identifies the historical sequence of governance regimes and focuses on the transformation between regimes, illustrating the utility of the general model and the nature of the transformation process. Although sophisticated and historically sensitive, they typically depend on secondary literature, so their conclusions may reflect their sources' perspectives more than unfettered judgment of primary evidence. For example, although the conceptual chapters sharply criticize efficiency models, efficiency considerations are frequently cited in the case studies, based on the research of efficiency theorists. By relying on secondary sources, the case studies also underplay factors that the conceptual chapters emphasize, especially the role of the state and its protection of property rights. In many of the particular chapters, the state is just another actor, qualitatively and conceptually no different from other actors.

The authors describe their theory as "eclectic." I would have liked to have seen a bolder defense of why they think some factors offer better explanations than others (e.g., property rights over efficiency, state over market, etc.), but it is also refreshing to see the careful caveats and balanced treatment of ideologically diverse approaches. But this rich book will, I hope, take its deserved place in the maturation of economic sociology as a formidable alternative to economics.

Gaining Access: Congress and the Farm Lobby, 1919–1981. By John Mark Hansen. Chicago: University of Chicago Press, 1991. Pp. xiv + 263. $38.00 (cloth); $15.95 (paper).

Jess Gilbert
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In *Gaining Access,* John Mark Hansen presents a theory of interest-group influence on policy-making. His theory mediates a debate in political science between those who view lawmakers as pawns of powerful lobbies and those who hold that members of Congress usually make their own decisions. Hansen claims a middle ground, arguing that lobbies "gain access when, in the judgment of congressional elites, they represent constituents" (p. 3). His premise is that legislators attend most to those who help them get reelected. Political parties, local elites, and interest groups succeed in influencing Congress under two conditions: when they offer a competitive advantage regarding electoral uncertainty and when lawmakers believe that this advantage will recur. Hansen gives little explicit attention to theoretical positions other than his own, which is presented in detail. He makes a good case.

His empirical application of the theory concerns the farm lobby and commodity price-support legislation. He shows, first, that despite the formation of the American Farm Bureau Federation (AFBF) in 1919 and the Farm Bloc two years later, the lobby did not win access to most rural midwestern Republicans until 1926, and still later (1931) to the southern

Democrats. Next, Hansen sees New Deal policy as the "maintenance" of Farm Bureau access; indeed, the major farm program, codified in the Agricultural Adjustment Act of 1933, was "the capstone of the farm lobby's ten-year battle for farm relief" (p. 78). Not much else happened, according to Hansen, to affect agricultural policy until 1948. The postwar years began the erosion of the Farm Bureau's congressional access, quickened by the Republican partisanship of the Eisenhower administration. Finally, the sixties and seventies brought several changes: the decline of farmers and their voting power along with the rise of new urban liberals in Congress, the replacement of the AFBF as *the* farm lobby by specialized commodity organizations, and the failure of the food-consumer lobby to gain sustained access.

Hansen's close historical analysis is a welcome contribution—not least because it extends much current research on agriculture beyond the executive branch. Theoretically, as he summarizes, the book explains the timing of access, based on the twin conditions of competitive advantage and recurrence. It also accounts for losing as well as being denied access. He briefly applies the theory to instances other than agriculture, including prohibition and the veterans' lobbies.

Otherwise, however, the book has a quite narrow focus. Its subtitle is too broad; it should be something like "The House Agriculture Committee and the AFBF (Sometimes) Make Price-Support Policy." The book is not really about the farm lobby's influence on Congress more generally, nor does it treat the noncommodity policies since World War I, as Hansen is usually careful to indicate. He misses some points by not examining such larger issues. For example, he asserts that interest-group access is won at the expense of local elites, whereas a closer look at the AFBF reveals that it was led by such elites, especially in the South. Hansen acknowledges this fact in footnotes (p. 85), but his theory ignores it—so does his history, because that fact is irrelevant to his narrow focus on commodity legislation in congressional agricultural committees. For the same reason, Hansen overlooks the significant policy changes between 1933 and 1947 (not to mention his questionable claim that the 1933 act was the farm lobby's plan). In the late thirties, for example, the Farm Bureau bitterly opposed the more liberal New Deal farm initiatives in Congress, which killed the programs during World War II. Hansen writes as if nothing was altered during those years. He devotes barely four pages to the war, and concludes his treatment of 1933–48 with, "The most remarkable feature of this chapter is the continuity, the inertia" (p. 96). Again, he has his reason. The "farm lobby's access was a constant" (p. 96). Hansen finds a narrow continuity where most see broad transformation.

Despite these criticisms, the book is well researched and well written, and other scholars will benefit from reading it. Hansen uses extensive primary sources, especially congressional hearings, and most relevant secondary ones. The book is easy to follow, thanks to Hansen's frequent guideposts and unwavering theoretical focus.

The War on Labor and the Left: Understanding America's Unique Conservatism. By Patricia Cayo Sexton. Boulder, Colo.: Westview Press, 1991. Pp. ix+325. $24.95.

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Amherst College

In *The War on Labor and the Left,* Patricia Cayo Sexton tackles one of the *big* questions about American exceptionalism: Why are labor unions and left parties so much weaker in the United States than in other capitalist countries? The answer, she argues, lies less in the "presumed conservatism of American labor or that group's internal weaknesses" (p. 1) than in the unique willingness and ability of capitalists and the state to control opposition. Furthermore, this control has operated less through any fancy mind-numbing hegemonic processes and more through straightforward, old-fashioned repression.

In part 1, Sexton criticizes theories that focus on the "failings" of American labor. She rejects the idea that American workers have been especially individualistic and procapitalist. American workers, she notes, are as class-conscious as their counterparts elsewhere (according to survey data), and they have often waged strikes and contested workplace control just as militantly. Just as important, many of the reputed causes of conservatism among American workers (the frontier, the lack of a feudal past and hence a revolutionary tradition, a hegemonic individualism) also characterize countries like Australia, Canada, and New Zealand with much stronger unions and left parties. Sexton also rejects the idea that American workers have been uniquely split by cultural differences or subject to either overly sectarian or overly bureaucratic leadership.

Having disposed of theories that, in effect, place the blame for weak unions and left parties on workers, Sexton, in the bulk of her book, recounts the familiar story of the political repression of unions, socialists, and other left forces in America from the late 1800s to the present. Business in the United States has taken a more combative stance toward unions than business elsewhere and has used a wider arsenal of coercive weapons against it, including, notably, hired mercenaries, strikebreakers, and "gentrified union busters." The American legal system has had more laws restraining and fewer enabling labor organizing than other countries. Within this legal framework, all elements of the American state have more readily employed law and armed force to undermine strikes and destroy unions. The U.S. state has also taken an exceptional range of measures against socialists and other leftists, peaking in the Red Scares after both world wars. The one historical exception merely proves the rule: during the New Deal's relatively prolabor regime, unions flourished. Sexton's arguments come most to life when she makes pointed comparisons to other English-speaking countries, for example, contrasting the history of railway unions in the United States and Britain (pp. 77–78, 111).

Sexton's argument, in short, is that America has had no dearth of worker militancy and left dissent, but the willingness and ability of business and the state to repress these has blocked sustained economic and political organization. Why have American business and the American state been uniquely conservative? Sexton offers a few isolated hints. In chapter 15, she argues that the lack of a landed gentry and a feudal past meant that America lacked an aristocratic ethic that might restrain a capitalist elite or justify a more balanced role for the state. It also meant the absence of the intraelite conflicts that might have forced capitalists to enter into alliances with their workers. The rapidity of American industrialization and the relatively great wealth amassed by American capitalists reinforced both their independence and their ruthlessness. Sexton, in short, after rejecting Hartzian arguments about the working class, applies them with vigor to capitalists and the state.

In chapter 10, Sexton emphasizes the often-noted features of the American political system that discourage worker parties and thus limit the working-class presence within government that might stay the hand of repression: a presidential rather than a parliamentary system, district rather than proportional representation in the national legislature, the weakness of political parties and proliferation of pressure groups, the disproportionate role of money, and lower voter turnouts.

Sexton's claim that our explanation of labor and left weakness in the United States should focus on the character of the powers that be, not of the would-be insurgents, makes sense. It fits as well with the dominant emphases in both social history and social movement theory. However, the development of her argument is flawed in several ways. First, Sexton addresses the crucial question of *why* the American business elite and state are uniquely repressive only in fits and starts. Second, although the best evidence for her theory comes from comparisons of the United States with culturally and economically similar English-speaking countries that have stronger unions and left parties, comparative analysis only sporadically breaks through the retelling of American labor history. Third, her initial effort to distinguish repressive and hegemonic processes of domination breaks down toward the end of the book as all processes of control get typed as repressive. At its best, however, *The War on Labor and the Left* reminds us of some of the uniquely repressive features of American society and underlines their importance in understanding the failure of the American left.

Harvesting Change: Labor and Agrarian Reform in Nicaragua, 1979–1990. By Laura J. Enriquez. Chapel Hill: University of North Carolina Press, 1991. Pp. xvii+252. $37.50 (cloth); $12.95 (paper).

Thomas D. Lobe
University of South Dakota

In one memorable scene from "The Battle of Algiers," the FLN leader Ben M'Hidi lectures the revolutionary Ali: "Starting a revolution is hard, and it's even harder to continue it. Winning is hardest of all. But only afterward, when we have won, will the real hardships begin." Oh, so true, as events in Algeria make painfully clear. But all revolutionary socialist governments attempting to consolidate revolutionary gains and transform their societies are confronted with obstacles and pitfalls that are more painful and obstinate than was ever imagined. Socialist goals are frustrated, radical social change compromised, and revolutionaries' energies sapped. Nicaragua's Sandinista experience exemplifies this tragic process.

Laura Enriquez has written a rigorous, succinct treatise on one of those frustrating dilemmas. *Harvesting Change* focuses on the land and labor question: How does a government maintain economic development and growth while redistributing agricultural resources, such as land, tools, seeds, and credits? This seemingly simple problem is made excruciatingly difficult by a number of complicating factors.

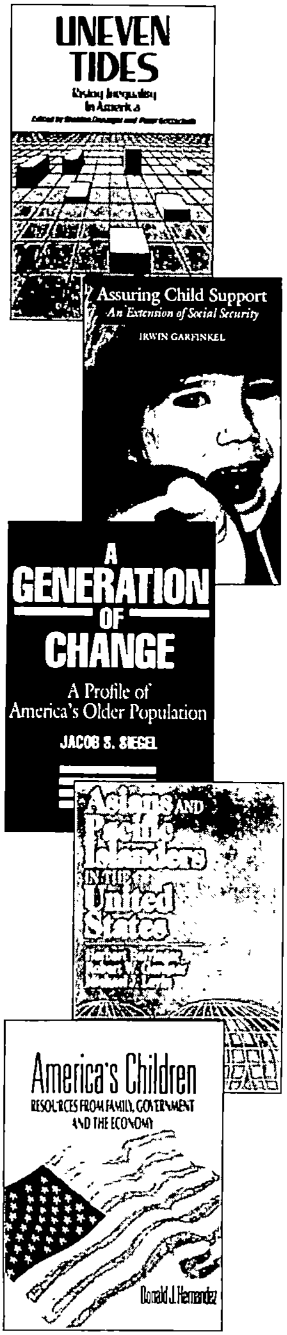
A crucial issue is that land ownership, land use, and labor have long been used for export crops. Though there is a vast literature by progressive writers from the developed world about the need to shift from export crops to subsistence farming, the necessity for hard currency, the resistance of the soil and farmers, and the tastes of the politically important middle class make that shift economically, politically, and culturally unpalatable. In the case of Nicaragua, the Sandinistas' admirable revolutionary goals were to give the peasants land, self-confidence, and educational and medical services. Enriquez points out that this strategy, however, gave rise to a number of problems in maintaining an export economy. Whether in the hands of landlords, the state, or communal owners, an export crop economy requires seasonal laborers, especially for cotton and coffee. But it was exactly this kind of low-wage, backbreaking labor that peasants have always hated. Thus the more the peasants gained an economic stake in the revolutionary Sandinista system, which operated to make it unnecessary to accept prerevolutionary forms of labor, the less reason for working someone else's land. As agrarian reform measures deepened the economic status and self-sufficiency of the Nicaraguan peasant, the status of export crops was further undermined.

Enriquez meticulously develops and details the implications of this political and policy conundrum, as well as the shifting Sandinista programs that attempted to discover some balance between equity and labor, between the promises made to poor peasants and the needs of a changing complex economy. The author spent many years in Nicaragua as a participant-observer. As an insider, she obviously cares passionately about the success of revolutionary transformations, but she does not flinch from enumerating the mistakes, shifts, and contradictions in Sandinista policy formation and implementation.

One example that all who journeyed to Nicaragua during the early 1980s will recognize is the problem of the labor brigades. The Soviet Union, China, Cuba—all socialist governments have argued that citizens must perform labor for both material and moral reasons. The new socialist person might volunteer to work in the fields, not for economic advantage, but because his or her society needs the labor. Such labor brigades were organized during coffee and cotton harvest and planting seasons and were made up of students, office workers, Sandinista cadres, and foreign volunteers. But the efficiency of these volunteers was challenged: land owners resisted their labor as they not only accomplished less than professional pickers and planters (often doing only one-quarter of the work or less), but their amateurism involved destruction—they were known to accidentally ruin the vulnerable coffee bushes. Added to that were the problems of transporting these vast labor brigades to the fields and protecting them from Contra attacks. Increasingly, the Sandinista government saw this solution to the land-labor dilemma as counterproductive.

The author applauds the Sandinistas' effort to destroy many of the most exploitative economic and social relations, and in her conclusions is dismayed at the possibility that the agrarian reform gains may be rolled back by the Chamarro government. She offers alternative strategies for resolution of the labor-supply problem, but they are tentative and unconvincing. She acknowledges that the Sandinista government might have been more successful in developing workable land-labor strategies if the United States had not sponsored the Contra war against the Nicaraguan government and mobilized an economic embargo. However, to her credit, Enriquez does not allow her judgments to be clouded by such wholesale, easy excuses, offering a far richer explanation of the achievements and failings of the Sandinista government.

If Enriquez's work has a failing, it is the absence of a historical and comparative context within which to place the discussion of these policy debates. Her introductory chapter details the theoretical literature on dependency, quoting writers steeped in studies from Nicaragua. Yet there is no discussion of other socialist societies' attempts to break out of dependency, to transform their agriculture, or to develop a revolutionary consciousness. Was there no interest in learning from other socialist countries' experiences? This is especially noteworthy given the legions of Cuban advisors that assisted the Sandinistas and the many Nicaraguan leaders that journeyed to Havana. Although their experience is quite different, the Cubans grappled for 30 years with these same labor problems, including the vexing issue of volunteer, nonexpert agricultural labor. Moreover, Richard R. Fagen years ago wrote the seminal book on an aspect of this issue (*The Transformation of Political Culture in Cuba* [Stanford, Calif.: Stanford University Press, 1969]). A discussion of these policy issues from Cuba, and perhaps North Vietnam, as well as how these historical experiences and lessons were or were not incorporated into the Nicaraguan policy process, would have given more context to this otherwise spendid book. As it stands, however, the Nicaraguan labor–crop output dilemma is written as though the Sandinistas confronted the issue for the first time and alone.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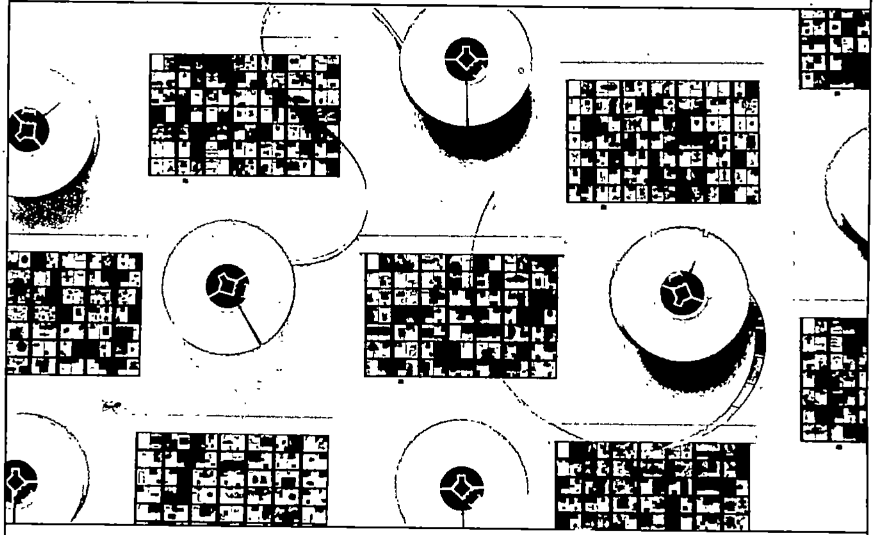

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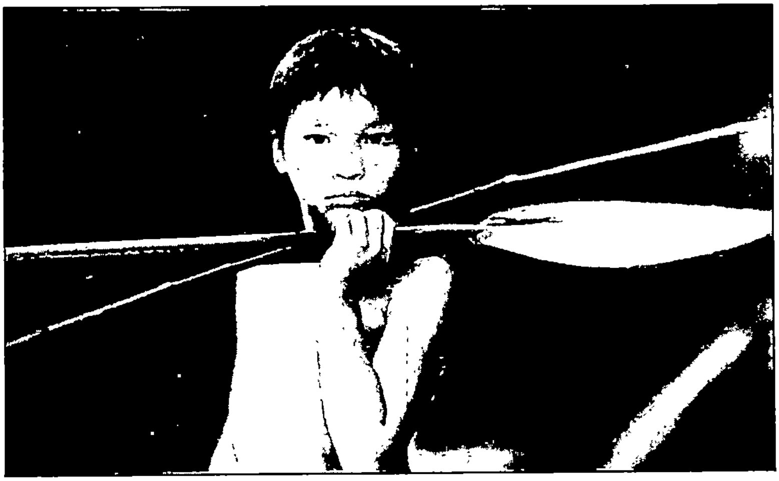

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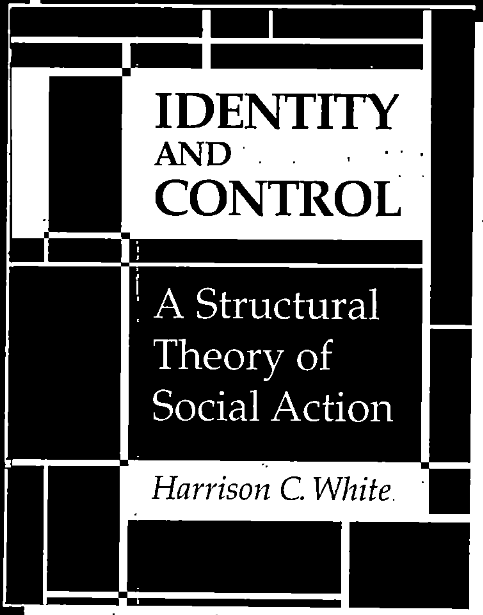

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